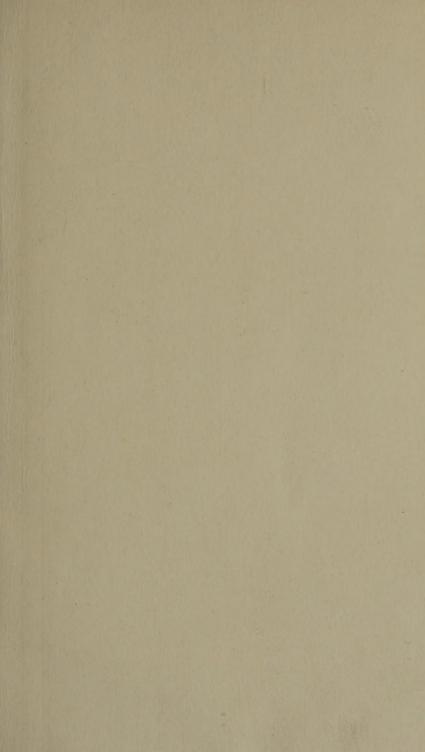Bay 227

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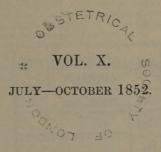# BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

OCTOBER, 1852.

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Periscope.

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PART FIRST.

Analytical and Critical Reviews.

ART. I.

1. *A Gyermekgyógyászat Tankönyve. A pesti gyermekkórhárban tett
vizsgalatai Stapasetalatai nyomán, a tudomány ujabb allaspontjához
Kepest.* Irta SCHOEPF-MEREI, M.D., &c. &c.—*Buda.*

*The Principles of Medicine in Relation to the Diseases of Children, or
Pathological Inquiries and Investigations prosecuted at the Hospital for
Children at Pesth.* By Dr. SCHOEPF-MEREI, Chief Physician to the
Hospital, and late Professor of the History of Medicine and Surgery at
the Royal University of Pesth, &c. &c. Vol. I.—*Buda.*

2. *Beiträge zur Pathologischen Anatomie der Neugebornen.* Von Dr. F.
WEBER, Prosector an der Universität in Kiel. Erste Lieferung. *Kopf
und Rücken.*—*Kiel*, 1851. 8vo, pp. 75.

Contributions to the Pathological Anatomy of New-born Children. By
Dr. F. WEBER, Prosector at the University of Kiel. Part I. *Head and
Spine.*—*Kiel*, 1851.

3. *Die Krankheiten der Neugebornen und Säuglinge vom clinischen
und pathologisch-anatomischen Standpuncte bearbeitet.* Von ALOIS
BEDNAR, Dr. der Medecin. prov. Primararzte des K.K., Findelhauses
in Wien, &c. &c. Zweiter Theil. *Krankheiten des Nerven-Systems bei
Neugebornen und Säuglingen.*—*Wien*, 1851. 8vo, pp. 198.

*The Diseases of New-born Children, and of Children at the Breast, viewed
in Relation to Clinical Medicine and Pathological Anatomy.* By ALOIS
BEDNAR, M.D., &c. Part II. *Diseases of the Nervous System.*—
Vienna, 1851.

4. *Erster Jahres-Bericht über die wissenchaftlichen Leistungen der K.K.,
Klinik für Kinderkrankheiten im St. Annen-Kinderspitale im Jahre
1850—51.* Von Dr. L. W. MAUTHNER, Ritter von Mautstein.—
Wien, 1851. 8vo, pp. 47.

*First Yearly Report of the Clinical Department of the Hospital of St. Anna
for Sick Children.* By Dr. L. W. MAUTHNER.—*Vienna*, 1851.

5. *An Essay on Infantile Remittent Fever, with Especial Reference to its Diagnosis from Hydrocephalus,* &c. &c. By CHARLES TAYLOR, M.R.C.S., late Surgeon to the Royal South London Dispensary.— *London,* 1851. 8vo, pp. 40.

6. *Lectures on the Diseases of Infancy and Childhood.* By CHARLES WEST, M.D., F.R.C.S., &c. Second edition, enlarged.—*London,* 1852. 8vo, pp. 559.

SOME years have now passed, since Lobstein addressed the profession in the following words :—" We must more than ever prosecute the study of the anatomical alterations, which even the most tender age presents, for here, every day reveals novelties and things hitherto unperceived, every day is marked by discoveries in the anatomical history of disease. Who would have said that man, even before his birth, is subject to numerous organic affections? Who, some time ago, would have thought that there exists a pathology of the fœtus, just as there is one of the adult? Who would believe that the short space of the life of the former is characterized by as many organic diseases, as is the course run through by man after his birth? The comparative study of the maladies of these two periods will be fruitful in novel and interesting results." So thought Dr. Graetzer, of Breslau, who, fifteen years ago, undertook a systematic treatise* on the diseases &c. of intra-uterine life, and now Dr. Weber extends the field of inquiry to a sort of *transition-period* between the latter portion of intra-, and the first portion of extra-uterine existence. It would be untrue, and evince great ignorance on our part, to affirm, that a considerable amount was added to our knowledge by Dr. Graetzer's *personal* observations ; on the contrary, his work is mainly a compilation of the diffusely scattered records of many previous investigators; but it still, so far at least as we are aware, constitutes the chief systematic treatise on the subject, and remains to this day the best book to refer to for information up to the period when it was produced. Nevertheless, it should be borne in mind, that several inaugural dissertations (the first, we believe, being the thesis of Duettel, 1702), papers, tracts, &c., had previously been published on diseases &c. of the fœtus ; and Graetzer derived also no small amount of information for his work from dictionaries of medicine, works on the general diseases of children, and on obstetrics, which touched upon his own ground. These remarks apply, also, to the treatise of Dr. Weber, so far as they relate to the fact of the field he occupies having been trodden by other labourers; but the author stands alone in this ; that his work is a systematic one, limited to the transition-period before remarked upon, and quite independent of the writings of other pathologists, since he details the results of his own investigations only. Of course the work of Graetzer includes subjects discussed by Dr. Weber, and that of Dr. Bednàr is occupied with many of them too ; but the former (Graetzer) includes a period antecedent, and Bednàr ventures upon one subsequent, to the dates to which Dr. Weber is limited, and neither are anything like so complete in their information as is our present author, within his own and special bounds. Our readers need scarcely be reminded, that the continental writers, in their general works on the diseases of infancy and childhood, have also given

* Die Krankheiten des Fœtus. Von Dr. J. Graetzer, ausübendem Artze und Geburtshelfer. Breslau, 1837. 8vo, pp. 272.

much increase to our knowledge; but we may direct their attention in a note to those writers by whom the nearest approach has been made in a systematic form to the treatise of Dr. Weber, though a glance at the dates of these works will be sufficient to show how different must be their contents from those of his production.*

" The pathological anatomy," says Dr. Weber, " of the new-born child is less developed than many other departments of this science. Its literature dates chiefly from modern and most recent times, and is distributed here and there in journals. I will only instance the subject of Deformities (towards the elucidation of which Meckel has confessedly done great service), where they are causes of other patho-logical conditions in the latter months of uterine, as also in first months of extra-uterine life. My investigations, upon which this work is founded, relate for the greater part to the bodies of such new-born children as have died in the first days after birth, or during birth, or shortly before parturition. The commence-ment of the disease, consequently, dates in most cases from uterine existence, or from the act of parturition. I have not, however, absolutely and entirely limited myself to the new-born child, but have also availed myself of the autopsies of those cases in which the child died during the first months of lactation; particularly when the symptoms observed during life were, from the appearances found after death, referred with great probability of truth to the above early periods for their commencement." (p. v.)

The first portion of the work now submitted to the profession by Dr. Weber, contains the results of several years' personal observations; and although he is situated at a small university only, and to some extent limited in his autopsies as regards number, in relation to that afforded to many of his brethren at larger schools, he has yet, by the kindness of Michaelis and his successor, Dr. Litzman of the Lying-in Hospital, been able to increase it to an extent otherwise impossible. The author refers, very modestly, in his preface, to his own labours, begging the reader to bear carefully in mind that he considers them merely, as his title-page indicates, " Contributions," and as making no pretension to an approach to a complete treatise on " the Pathological Anatomy of New-born Children."

As Dr. Weber gives no general sketch of what the work is to include when finished, we can but form our judgment from the portion of it now before us, and which treats of the " Head and Spine." From this it would appear that all *monstrosities* are to be excluded, and we thought at first all deformities and malformations as well, until we found several pages occu-pied with an account of *club-foot,* and *spina-bifida* touched upon under affections of the spinal-column. These being included, we are at a loss to account for the exclusion of many things which fall quite as much within the scope of Dr. Weber's inquiries as the above subjects, which, to speak the truth, really fall without it, seeing that they are not induced by the act of parturition, nor is their origin limited to the latter periods of intra-uterine existence.

When noticing the first part of Dr. Bednàr's treatise, we expressed a

* C. J. Trew. De differentiis inter natum hominem et nascendum. Norimbergæ, 1736.

C. J. Trew. Abhandlung von einigen Verschiedenheiten welche an den Menschen vor und nach der Geburt wahrgenommen werden. Nürnbg. 1770.

F. J. A. Gessner. Diss. de mutationibus quas subit infans statim post partum indeque mutata ejus œconomia naturali. Erlangæ, 1795.

J. H. C. Trefurt. Diss. de mutationibus nonnullis quæ primis vitæ diebus infantium recens natorum observandæ veniunt. Götting. 1829.

hope that it would not be long before we again had the pleasure of
meeting with its author, considering how capable he was of illustrating
the ' Morbid Anatomy of the Diseases of Children.'*   We have now the
second portion of his work upon our table, and are happy in bearing tes-
timony to its value in relation to our present subject.   The first part of
his work related, as our readers will remember, to the pathology of the
alimentary canal; the present embraces that of the cranium and spinal
canal and their contents.   No direct comparison can be fairly made
between Dr. Bednàr's and Dr. Weber's works, since the investigations of
the latter are limited in most cases to quite the new-born child, whilst
those of the former extend to patients several years old, although many of
the children received into the Foundling Hospital of Vienna are admitted
within fourteen days after birth.   As it is within the walls of this insti-
tution that Dr. Bednàr has prosecuted his labours, he has had considerable
opportunity of investigating the pathology of what even Dr. Weber himself
would designate the " new-born child," and hence touches upon many of
the subjects more minutely dwelt upon by the latter writer.   In passing
Dr. Weber's work under review, we shall, as opportunity offers, make refer-
ence to that of Dr. Bednàr, so that both treatises may be included under
one head. We shall make the former, on this occasion, our *point de depart,*
having already introduced Dr. Bednàr to our readers (vol. vii. p.107) ; the
more especially, too, as the field within the limits he has chosen is so
peculiarly his own, rendering our labours *less critical* than we might
otherwise have made them.   We pause for a moment, however, to draw
the attention of those interested in the pathology of the diseases of
children to a name which has not hitherto received mention in these
pages — a name to which much praise is due, as those who are acquainted
with the work of Dr. Hügel (reviewed in our fifth volume, p. 362) will
be sufficiently aware.   We allude to Dr. Schoepf-Merei, whose work is
noticed at the head of this article.   There may not be a few, however, to
whom the intricacies of Viennese German† are scarcely so familiar as to
have permitted them to have made acquaintance with the writer in
question; and for their information, and to the credit of Dr. Schoepf-Meri,
we may here observe, that to this Hungarian physician is due the exist-
ence of the hospital for children at Pesth.   This institution was projected
by him immediately after the great inundation of the city by the Danube,
about twelve years ago; and now, by the kind patronage of the Archduchess-
Palatine, Mary Dorothea, it contains thirty if not more beds.   Judging
from Dr. Hügel's remarks, it would appear that the projector had very
great difficulties to encounter ere he attained his purpose—difficulties so
great, indeed, that those only who are acquainted with the peculiar con-
dition of citizen-life in Hungary, induced by the high political excitement
and party feelings there prevalent, can form an adequate notion of them.
Dr. Schoepf-Merei has now become a voluntary exile from Hungary, and we
believe permanently settled himself on English soil.   He has already made
himself known to the profession in this country by some able papers in
one of the monthly and weekly serials; and we have no doubt will earn for

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* Vide No. XIII. p. 110, et seq.
† Even a German (the reviewer in the ' Journal fur Kinderkrankheiten') alludes to the " ensetzlich
lang stylisirte Perioden," which the preface to Dr. Hügel's work contains.

himself here a repute in his *specialité*, as well deserved as that which he has for several years enjoyed in his native land.   Some few years ago, Dr. Schoepf-Merei published a yearly report on ' Practical Medicine and Surgery in Relation to Children.'   This report has been followed by the systematic treatise now before us, which, however, is but the prelude to others, which we trust may follow, and these clothed in the garment of our mother tongue.   On the present occasion, we fear that we can do but scanty justice to the author and his labours; but if, in the course of our remarks, we find opportunity for appealing to his acute observations and experience, we shall do so without further preamble.

We shall now refer to Dr. Weber, who thus commences his introduction:

" The corpse of the new-born child undergoes decomposition far less rapidly than that of the adult.   Development of gases in the cellular tissue, deliquescence, and offensive odours, rarely make their appearance during the first week; more frequently an imperfect dryness, with induration of the skin and fat, are seen. The cause of this delayed decomposition is, I believe, to be found in the less frequent previous existence of those dyscrasic maladies in the child, which undoubtedly dispose the corpse of the adult to a more rapid putrefaction.   In consequence of the protracted appearance of the latter, the corpse of the new-born child is longer available after death for many points of pathological investigation. Nevertheless, certain organs form an exception to the rule, and pass even more rapidly than in the adult into a state of putrefaction.   No portions change more quickly, and yield, even twenty-four hours after death, more fallacious results relative to consistency, than the brain and spinal cord, particularly in those cases where both have been in contact with morbid effusions.   But even when the latter have been absent, the consistency of each becomes markedly less a short time after death, the brain softening earlier than the spinal cord." (p. 7.)

In cases where the chief point for investigation in the autopsy will be the condition of these portions, Dr. Weber advises the body to be laid on the face as soon as possible after death; for although the dorsal position might produce no important alteration in the contents of the capillaries, yet the larger venous plexus covering the dura mater and the whole spinal canal, may become engorged from gravitation, leading, on the one hand, to false conclusions, and, on the other, preventing a perfect examination of the spinal marrow and its coverings.   The whole of Dr. Weber's introductory remarks are worthy of careful perusal, but our limits oblige us to pass to the " Pathological Alterations in the Head." (p. 11.)

The author enters first upon the consideration of those cranial tumours which are the consequence of a stasis in the capillaries of the external covering of the skull, produced by the pressure to which the child is subjected in the lower pelvis.   The capillary vessels can only imperfectly, or not at all, empty themselves, on account of the compression of the veins; they swell, and a serous fluid exudes through their delicate parietes, analogous to the effusion of dropsical swellings produced by pressure upon the venous canals.   In some cases a limited rupture of the capillaries or veins ensues, and a slight extravasation of blood is the consequence.   The situation and size of the tumour depend upon the position of the child's head during labour, and quite as much upon the degree, if not rather more than upon the duration, of the pressure to which it is subjected.   These effusions and extravasations take place in the tissues *above* the periosteum of the cranium, producing a swelling constituting the *caput succedaneum* of many

writers. But effusions &c. also ensue *beneath* the periosteum, giving rise
to a tumour; such is the *cephalhæmatoma* of authors. In the latter also,
blood constitutes the chief extravasation. The distinction here made, as
insisted on by many pathologists, between the two classes of tumours, is to
a certain extent maintained by Dr. Weber, and particularly so, as it appears
to us, by Dr. Bednàr; but it is doubtful whether there are not transitional
forms which unite the two together, anatomically considered, however
well the demarcation seems to be preserved between extreme forms of
these affections. Dr. Weber thus writes—

" The third form of caput succedaneum (*kopfgeschwulst*) differs from the other
two varieties in this, that extravasation not only exists in the subcutaneous cellular
tissue, as also in and beneath the *galea*, but the periosteum of the cranial bones is
elevated to a greater or less extent by effusion of blood beneath it. I have had
frequent opportunity of observing this in caput succedaneum, and view it as the
commencement and lower grade of cephalhæmatoma, without however denying that
the latter may also originate in another way." (p. 13.)

In one of the varieties of *caput succedaneum* alluded to by the author,
such appearances of suggillation, or contusion, are occasionally seen, on
dividing the subcutaneous cellular tissue and aponeurosis, that suspicion
may arise that external force, as a blow, has been applied, and the matter
become a subject for the medical jurist. A case in point is recorded by
Dr. Weber as having occurred in Germany, though no criminal intent was
attributed to the mother, as it was thought likely, from the circumstances
of the case, that the child was born very suddenly, and fell upon the ground
whilst the woman was standing before the bed. Without denying that
such appearances of contusion corresponding to the tumour above it, as in
the example given, might be produced in the manner asserted, Dr. Weber
urges attention to the fact, that—

" Every extravasation of blood in the cellular tissue beneath the scalp is not to
be ascribed to external violence. Its formation during birth, and as a complication
of caput succedaneum, must not be lost sight of in medical jurisprudence.",(p. 12.)

According to Bednàr (p. 178), where there is extravasation of blood in
the scalp and subcutaneous cellular tissue, there is generally effusion of it
between the meninges, and often in the substance of the brain itself.

Cephalhæmatoma is by most writers usually regarded under two forms;
1st, the ordinary form in which an effusion of blood exists between the
periosteum and external surface of the skull,—c. externum, or sub-
pericranial ceph.; 2ndly, where blood is effused between the dura mater
and the cranium,—c. internum, or sub-cranial cephalhæmatoma. The
latter may, or may not, be accompanied by the former; but under any
circumstances it is very uncommon. Bednàr does not appear to have met
with a single instance of it. Dr. Weber is not satisfied with the above
classification, but prefers the following:—1st, cephalhæmatomic tumours
which occur under a healthy condition of the vessels and bones of the
locality affected; 2ndly, those connected with a primitive diseased state
of the vessels or bones, or of both. (p. 17.) The immediate cause of the
latter form is, in the majority of cases, identical with that of the first—
viz., pressure during labour hindering the return of the blood, consequently
producing rupture of vessels. Of the first form, three varieties are alluded
to by the author. The simplest is characterized by the existence of a thin

layer of blood beneath the periosteum, corresponding to the locality of an overlying *caput succedaneum* of that description, in which extravasation of blood, along with the ordinary serous effusion, is formed in the cellular tissue under the scalp.   It is non-fluctuating, and, according to Dr. Weber, more frequently to be met with than the larger and fluctuating varieties. Both the fluctuating and non-fluctuating tumours depend for their production upon the rupture of vessels between the bone and periosteum.   In some cases rupture also ensues in the vessels of the substance of the bone, giving rise to the extravasation; whilst in others the walls of the superior longitudinal sinus suffer, and blood is poured out in the vicinity of the sagittal suture, between the periosteum and cranial bones.   An example of the former is detailed by Dr. Weber; and of the latter he remarks—

" In this instance also a so-called cephalhæmatoma internum was found, easily explainable by the inordinate distension and consequent rupture of the capillaries which connect the dura mater with the inner surface of the skull.   I must here observe, that I have had but rarely opportunity of seeing this; but where it occurred, similar rupture of capillary vessels and effusion existed at other spots, namely, above the orbit between the dura mater and skull.   Although such may be of the same nature as the former, yet the term c. internum is scarcely applicable to it." (p. 17.)

Dr. Bednár writes—

" Once only have I seen the veins between the parietal bones and dura mater gorged with blood, and effusion between the latter on the left side; therefore, of the so-called internal thrombus I can say nothing." (p. 174.)

To return to Dr. Weber :

" It has been maintained that cephalhæmatoma arises at the spots where the skull, in its transit through the lower pelvis, is particularly exposed to the pressure of the protuberances of the bones, and especially to that of the promontory.   In itself, this appears not improbable; for if we reflect upon how great the pressure often is upon the child's skull and its coverings, when the conjugate diameter is small, injury to vessels by bruising will not seem unlikely to happen.   Nevertheless, my experience does not confirm this mode of causation.   I never witnessed at those spots, where the scalp through injury to the integuments showed bruising to have happened, nor even where the bones had suffered from the promontory, nor where the marks of the forceps were seen upon the skin, cephalhæmatoma arise.   On the contrary, where I observed the latter, the above-mentioned causes did not exist. We must adopt therefore, provisionally, the opinion, if the experience of others does not gainsay it or prove the existence of other causes, that the origin of *many* cephalhæmatomata is to be found in the obstruction to the return of the blood produced by pressure over the whole periphery of the child's head, and the consequent rupture of vessels." (p. 17.)

The author thus appears to participate in the views of Valleix, who considers that the " ecchymosis " is due to a circular pressure, which can only be exerted by the neck of the uterus; M. Valleix admitting, however, the existence of exceptional cases, in which the intervention of the osseous walls of the pelvis may act in producing the tumour.   That the latter must be very rarely the case, this writer considers, is proved by the fact of cephalhæmatoma almost always being observed after easy labours only. On the other hand, from this very circumstance, M. Pauli has sought to establish the influence of the bones of the pelvis, since the tumour only then occurs from the head of the infant being exposed to shocks and *brusque* pressure in rapid labours.*   Again, in reference to the opinions of

* Vide Fabre, Maladies des Enfans, t. ii. p. 220.

Valleix, it has been asked : How is it, the cause being constant and inevitable, that cephalhæmatomata are not more frequently met with? The answer is, that the cases which are most favourable for their production are those in which a very large extent of the parietal surface of the head presents at the neck of the uterus, to the exclusion of other portions of the cranium; and such cases are sufficiently rare. Out of 74 examples, Bednàr (p. 175) found the tumour 40 times over the right, 22 over the left parietal bone, and 6 over each; 4 times over the occipital, once over both parietals and the occipital, and once over the frontal bone. The tumour reached its largest circumference over the parietal, and its smallest over the frontal bone.

It was the opinion of many of the older writers, that cephalhæmatoma was always connected with a primitive disease of the cranial bones. Such opinion had its origin in the following facts. On examining the tumour during life, soon after it appears, a hard and *apparently bony* ridge is felt running round and limiting its base, and on *post-mortem* inspection the surface of the bone beneath it is found in many cases to present appearances very different from usual or healthy structure. This ridge was assumed by many (Michaelis, Paletta, Dzondi, &c.) to be the limit or edge of the external table of the bone, deficient from vice of conformation, or destroyed by caries. We believe that Naegele, in 1819 (*vide* Graetzer), was the first to dispute the necessary connexion of the tumour with lesion of the bone, having seen cases " in which the bones were quite sound and smooth." In later times, different views have been held upon the matter. The existence of the *ridge*, in most cases, if not in all, an unusual condition of the upper surface of the bone in some, and a healthy state of it in other instances, are now generally admitted to occur. But what is the nature of this ridge, and what are the relations of cause and effect between the appearance of the surface of the bone and the sanguinolent or other extravasation or tumour? Opposing themselves to the views of the earlier observers before alluded to (Michaelis, &c.), Zeller and Pignè regard the ridge as resulting from the pressure of the effused fluid on the bone within its circle, Dubois considers it formed by indurated or diseased periosteum ; Carus, as the reflected edge of the latter; Krause believes it to be a true osseous ring ; Valleix, an osseous pad or cushion (*bourrelet*) ; whilst Busch, Wokurka, and Bartsch, refer it to " une pure hallucination de toucher !"[*] Finally, M. Dœpp,[†] from observations made on 255 children, asserts that in the vast majority of cases the ridge in question has its origin in coagulation of the blood, precisely at that spot where the periosteum, elevated by the effusion, begins to separate itself from the cranial bone. M. Dœpp, however, admits that in certain and rare cases, where the tumour has remained in existence a long time, the superior table of the skull disappears from absorption, or is destroyed by caries, and that in such instances the ridge may indicate the limits between the locality of the deficient bone and that which remains in a sound condition. A modification of M. Dœpp's views is now received by many pathologists as the true explanation of the matter. It is admitted that the blood in the tumour quickly coagulates, and that the edge of the coagulum imparts to the touch the sensation of a peripheral ring; but that the firm and hard ring, felt as the tumour gets older, is the result of a reparative process, in the course of which a fibri-

nous exudation is poured out, and heaped up in great abundance at the place before indicated; in some cases, also, bony matter is deposited in this fibrinous ridge, so as really to cause it to be an *osseous* circle.* Admitting that there are cases in which an original abnormal condition of the bone exists, predisposing to the formation of a sanguinolent tumour, yet it appears to be the fact, that in a very great majority the appearances of osseous disease are truly the *effect* of the extravasation and not its *cause;* an *effect* seen in the destruction of the surface of the bone by pressure in one set of cases, and in another evinced by an attempt at reparation being made, "bony plates" being formed at the spot where extravasation existed, causing the surface of the bone to appear as if roughened by ulceration or caries.† Upon some of the foregoing points, Dr. Weber thus expresses himself:

"There are, however, cephalhæmatomata, towards whose production a diseased state of the bones and vessels may be said to dispose. Ammon, in his 'Congenital Surgical Diseases,' speaks of cephalhæmatoma in connexion with which the vascular system of the pericranium, of the bones, and of the dura mater, were abnormally altered, the veins of the cranium especially being enlarged. Langenbeck, of Göttingen, also observed beneath the tumour highly-dilated *venæ diploeticæ*, having the appearance of *v. emissariæ*. These observations, coupled with some afterwards to be mentioned, and which I myself had an opportunity of making, appear to me to substantiate the existence of the second class of cephalhæmatoma. I believe that here the immediate cause of the rupture of the vessels is identical with that of the first class—namely, hindrance by pressure to the return of the blood. The rupture ensues with so much the more facility as the vessels are enlarged, and hence, probably, their walls diseased, and more easily tearable. I cannot participate in the opinion of Langenbeck, that cephalhæmatoma may also be a *vitium primæ formationis*, depending as it does for its support upon the absence of the *tabula externa* of the corresponding portion of the cranial bones; for, so far as I know, cephalhæmatomata have never been observed in the early uterine periods. The primitive want of a *tabula externa*, when such happens, particularly when the vessels are at the same time enlarged, cannot be denied to be a predisposing cause, as a following case will show. The case reported by Ammon, where the parietal bone was completely perforated at a small spot, and where a c. internum existed, I am inclined to refer to a secondary process for production." (p. 18.)

"Ammon, in one place mentions, that cephalhæmatomata with pulsation have been seen. Such cases cannot, in my opinion, be numbered amongst cephalhæmatomata without extension of the meaning of the term; for, until now, the escape of blood from the vessels has been implied as its signification, or we must assume that the tumour experiences vibration from underlying pulsating vessels." (p. 19.)

When we reflect, that the extravasated blood lies between the bone and periosteum, and is present in such quantity that weeks are needed for its complete resorption, or even are insufficient for the purpose,—that consequently a large extent of bone (covered on its surface with periosteum, which is connected to it by capillaries, a surface covered now therefore with torn capillaries) is in contact with extravasated blood operating almost like a foreign body, which will in the meanwhile change from its bland quality, it cannot appear surprising—

"That the bone itself at length suffers, perishes at its surface, if only in the form of thin plates, and that then nature, as in analogous cases, undertakes a trial at reparation. In this way is the bony peripheral ring to be explained. It is a line

* Vide Lectures by Dr. West, second edition, p. 39, and his paper in the Medico-Chirurgical Transactions, vol. xxviii.

† Ibid.

of demarcation on the cranial bone, similar to what is observed in largely exposed surfaces of the cranium followed by exfoliation." (p. 20.)

"In such cases, the pericranium also becomes thickened and hardened by pathological exudations at the periphery of the tumour, and increases the sensation of a circumferential boundary on external examination." (p. 21.)

Dr. Weber next alludes to "contusion of the cranial integuments," arising from a large head being forced through a narrow pelvis having a greatly projecting promontory, or from unfavourable position of the forceps. We shall not dwell upon this subject, but pass on to "alterations of the bones of the cranium." Our attention is first directed by the author to flattening of the parietal bone "without fracture or flaw of it," produced in most cases by pressure of the promontory in prolonged labours and forceps cases. Examples of the above have frequently come before his notice, and he discusses at some length the general and important influences which pressure on, and alteration of, the symmetry of the skull, have upon the brain and intellectual element. Without denying that the development of the brain has a great influence upon the development and form of the skull, Dr. Weber believes it "to be undeniable that deviations of the form of the latter produced adventitiously are often prejudicial to the development of individual portions of the cerebrum, and that the act of parturition is by no means rarely such an adventitious cause." (p. 23.)

"Deviations of the form of the cranium will be found, on careful examination, to be far more frequent than is supposed ; and if flattening even of the frontal bone is noticed only after a lengthened period, or not at all, by the parents, far less likely is it that flattening of the parietal with its hairy covering will force itself upon their attention. Through the kindness of Professor Jessen, of Hornheim, I have had frequent opportunity of examining the bodies of persons who had been insane, and have arrived at the conclusion, that this condition of the skull—namely, inequality of form of both sides at the frontal and parietal bones, is of very frequent occurrence amongst insane people. The flattening is, as a rule, accompanied by thickening of the dura mater and firm adhesion of it to the inner surface of the skull parallel with the flattening on the same side—a circumstance from which I conclude, that the state of the bones and of the dura mater is primitive and causative, that of the psychical element being on the other hand consecutive in its character." (p. 24.)

"Overlapping of the parietal bones" and "projection of the occipital bone," are often accompanied by a *caput succedaneum*. The head has then a very remarkable and elongated appearance, which has caused the term "sausage-form" to be applied to it in Germany. A few days after birth, the cranium acquires a more natural condition, progressively improving as absorption of the serous effusion and expansion of the brain ensue. In some cases, however, the parietal bones do not regain a proper and equal position, and the consequence is, that the occipital suffers permanent projection outwards and backwards, giving to the head a peculiar character for life.

"In all crania in which the occipital bone greatly projects, the lambdoid suture contains more or fewer of the so-called Wormian bones. Their presence proves, that during that period of life in which the bones of the cranium are specially developed—as soon after birth—a considerable space has existed between the occipital and parietal bones. . . . . Where the Wormian bones occur along with a greatly projecting occiput, it is not improbable that such a projection of the occipital bone has its origin in the persistency of a deformity taking place during birth." (p. 28.)

Dr. Weber, in concluding his remarks upon " Obliquity of the Skull," thus sums up:

" The conclusion is substantiated, that obliquity of the skull has for its cause, in almost all cases, hindrance to the mechanism of labour from an abnormal state of the pelvis. The more frequent deformities having this origin are the following:

" A frontal bone appears flattened, because it is pushed backwards and beneath the other half.

" A parietal bone appears flattened, because it is pushed beneath the other parietal.

" The occipital bone projects greatly backwards.

" A whole half of the cranium appears flattened, because it is pushed somewhat back, and as if its frontal and parietal bones were driven beneath the bones of the other half." (p. 30.)

Dr. Bednàr touches but slightly on the above points, in the body of his work, but gives a valuable appendix of ten tables of measurements, &c. The first seven tables include the following measurements of 119 boys and 112 girls from 3 days to 8 years and 4 months old—namely, of the greatest periphery of the head, from ear to ear; from the occipital protuberance to the root of the nose; of the transverse and long diameters of the head; of the length of the body, and of the circumference of the thorax. The eighth table contains like measurements of 8 children presenting abnormal conditions of the cranial bones, of the brain, or of its membranes. The ninth, of 6 children having congenital hydrocephalus; and the tenth, of 8 children having chronic hydrocephalus, not congenital, but acquired. The author has been led to these inquiries, from his belief that an account of the circumference and diameter of the head, in relation to the circumference of the chest and length of the body, would be of more avail in those diseases of the brain and its membranes, which are consequent upon the increase of growth and changes of form of the skull, than an account of the weight of the cerebral mass, as the former may be of use in relation to diagnosis during the life of the child. Dr. Bednàr considers his tables of measurements as yet too incomplete to admit of safe generalization from them; but trusts to perfect them for this purpose by a future time. We can confirm from experience the following remarks by Dr. Weber on the " Strength of the Skull."

" Though it may be the rule, that the development and strength of the cranial bones progress *pari passu* with the development of the rest of the body, so that large children have also large heads and strong cranial bones, yet such is by no means always the case. It has sometimes very forcibly struck me, that little delicate children, with small heads, have thick strong bones over a large extent of the cranium, and conversely, that in large, powerfully developed ones, with large heads, the bones of the skull are thin and easily cut through by the scissors. I have observed this so often as to forbid its being considered as exceptional. I am unable to offer a satisfactory explanation of it. Probably the osseous system of the mother, as also the qualitative conditions of her blood, have here some influence. But it will be asked—why, then, do weak, delicate mothers bear, not rarely, large, well-nourished children?" (p. 31.)

The frontal and parietal bones are the only ones which Dr. Weber has seen fissured and fractured by the act of parturition. According to the greater or less extent of the fracture, and particularly the distance of separation between the edges of the injured bone, so is the amount of injury to the vessels of the locality of the fracture. Rupture of small

ones always occurs, as is proved by the most delicate fissure being indi-
cated by a red streak. The periosteum is generally elevated by extrava-
sated blood, and there are marks of sugillation from effusion into the
cellular tissue of the scalp. In these cases, internal cephalhæmatoma may
occur. But the extravasations here alluded to must be distinguished
from those which follow rupture of the longitudinal sinus and of the larger
cerebral veins. It is true that both forms may be present, but, according to
Dr. Weber, the latter are not the consequences of the fracture, but rather
of the same cause which gave rise to the fracture—namely, "a too violent
forcing of the bones over each other." Bednàr, besides referring to the
fissures and fractures, dwelt on at some length by the former writer,
remarks, that

" In the cranial bones of new-born children, chinks are sometimes observed
running from the surface for several lines deep into the bone, generally in a some-
what oblique direction. Their origin we cannot explain, as, from the yielding
character of the bone, we are unable to produce them in the corpse by stroke or
pressure." (p. 173.)

The same writer also states, that the cranial bones of children affected
with congenital syphilis are more porous and brittle than in the healthy
child. Once only has Dr. Weber met with "a large, so-called false fonta-
nelle;" it was situated in the centre of the sagittal suture, both parietal
bones forming portions of its boundaries. Alluding to "membranous
gaps," not interstitial, Bednàr observes,

" These are most frequently met with in the parietal bones, rarely in the occi-
pital; they are found in greater or less number, and when of rather large size may
be distinguished through the scalp by the application of the finger. They indicate
incomplete ossification of the skull, especially of the parietal bones, whose mem-
branous basis becomes converted into bone, without the previous formation of
cartilaginous matter. Well-developed children, and weak and premature ones,
are born in the above condition. It has no further signification, being removed as
progressive ossification of the skull ensues." (p. 169.)

Increase of size of the true fontanelles, and the occurrence of large
interstitial spaces between the bones, are noticed by Dr. Bednàr; as also
premature closure of the sutures. In consequence of excessive ossification,
a wall of bony matter is sometimes formed, which projects for one or
two lines above the course of the suture, and may easily be felt through
the coverings of the cranium. Such premature ossification, besides in-
fluencing materially the form and development of the skull, forms a
hindrance to the development of the brain. To proceed to Dr. Weber:

" One of the most frequent appearances connected with the cranial bones of
new-born children, especially as regards the parietal, the upper portion of the
frontal, and the occipital bones, is, that these bones are deeper coloured from capil-
lary injection than in the normal state, indeed, not unfrequently having a dark
red-blue colour. This may be present to a high degree without there being actual
rupture of capillary vessels." (p. 32.)

" I do not ascribe, in every case, this congested condition of the cranial bones
to the pressure which the head experiences on its larger periphery; for many, and
especially juridical, dissections (in a word, those where, the pelvic relations being
normal, death had ensued after respiration was established), have taught me that a
congestive condition of the cranial bones may be seen in children suffocated some-
times under the bed-clothes. Nevertheless, I must expressly state, that in the
latter instances I have never witnessed the intenser grades of such condition,

even when other evidences of suffocation were plainly existing. These are only to be seen when the head has been subjected to strong pressure." (p. 33.)

We proceed to the meninges. Blood may be effused on the surface of the brain between the dura mater and the arachnoid, from rupture of the superior longitudinal or transverse sinuses, and, it is probable, from that of the veins leading to these sinuses. In a case in which the longitudinal sinus was ruptured, the author found the brain external to the arachnoid " inundated " with half-coagulated, half-fluid blood. It had even passed to the base, forcing its way beneath the tentorium, and encircled the cerebellum. Sanguinolent effusion may likewise take place from the bleeding of capillary vessels; but observation seems to show that this is the least frequent of all sources of extravasation on the brain.

" Occasionally new-born children live for some time with more or less complete paralysis of one half of the face, or they may entirely recover from it. The cause of it is an extravasation of blood, which, as dissections teach me, is situated between the arachnoid and the dura mater." (p. 35.)

" Many of the hæmorrhages on the surface of the brain of new-born children which do not terminate in death, are, I am persuaded, frequent causes of their continued delicacy of health. . . . . The death of *neonati* in convulsions is also in many cases due to such extravasations."

" We are sometimes surprised that in infants who are born with strong pulsating cord and heart, respiration cannot be established in spite of every exertion, the pulsation becoming weaker and weaker until they die. Whoever examines their bodies after death will find in the majority of cases such *post-mortem* appearances as I have already described, and as I shall further allude to when speaking of the vascular system of the spinal marrow." (p. 36.)

Children are now and then born with facial hemiplegia, which, as Dr. West has remarked, is quite independent of any injury to the brain, but—

" As the result of injury to the nerve from application of the midwifery forceps, or, as has in one or two cases been observed, from injury received during the passage of the head through the pelvis, without any instruments having been employed. . . . . In the only case of the kind which has come under my own observation, the distortion of the face, though very great at birth—one eye being wide open, and the corresponding side of the face powerless, so that the child was unable to suck—had already greatly diminished within forty-eight hours, and had quite disappeared within a week." (p. 152.)

Dr. Weber has frequently met with blood effused between the fibrous layers of the dura mater   Its usual position is between the lamellæ of the tentorium, near to the place of junction of the longitudinal and transverse sinuses, and, less frequently and in less quantity, between the fibrous layers of the larger falx near the longitudinal sinus. Considering the dark venous character of the blood, &c., it is probable that it is poured out by the neighbouring sinuses. Dr. Weber gives another theory of its origin, which he thinks admits of something being said in its favour, though yet in great want of accurate observation for its basis. This theory, so far as we understand it, is, that the so-called extravasation is really no extravasation at all, but the natural sanguinolent contents of small supernumerary sinuses. The author makes some interesting remarks upon increased size or width, particularly of the larger sinus. Such increase he regards as not being without influence on the continuance of the life of the child, and as demanding, as much as possible, avoidance of all artificial pressure on the

head. The normal character of the " contents of the sinus" should be borne in mind when making examinations for judicial purposes.

The arachnoid and pia mater are, when healthy, perfectly transparent in the *neonatus;* on the other hand, in advanced age, and in the proximity of the Pacchionian bodies, they frequently appear somewhat opaque, and this without any disease having previously existed.   Therefore—

" Every departure from the normal transparency and colour in new-born children is to be regarded as the consequence of a pathological process.   I need scarcely here allude to the yellowish colour which is always present in the meninges, and in the serous humidity sometimes found beneath the arachnoid in the so frequently occurring jaundice of children.

" Another and more important colouration, especially of the highly-vascular pia mater, is that which is produced by a congestive state of the latter, and which is so frequently seen.   I have long since passed that period, when one discovers congestion of the cerebral vessels every time one opens a skull.   A true congestive condition of a high degree is here alluded to, and it is this which not uncommonly gives to the whole cerebral surface of neonati a deep red colour (as if from saturation), by means of the extremely fine injected capillaries.   That it is connected with a pathological process, the exudation which is present sufficiently proves." (p. 42.)

This capillary injection is sometimes so intense as to produce a dark red colour, and yet no rupture of vessels can be found.   The exudation accompanying it is seen under two chief forms, between which exist many transitional varieties.   In the first, it is found in considerable quantity (ad ʒi.) beneath the arachnoid, is quite clear, transparent, and watery, or analogous to what is met with in certain kinds of *caput succedaneum.* The second is observed when the pia mater is paler in hue, and, so far as its colour is concerned, has very great resemblance to pus.   On closer inspection it looks like a half-coagulated croupose deposit, which may be taken up in large pieces by the forceps.   According to Dr. Bednàr, effusion of blood into the sac of the arachnoid, both on the hemispheres and at the base of the brain, is now and then found in new-born children.   As there is often no other affection of the nervous centres to be seen, such extravasation must be regarded as resulting from the rupture of delicate vessels during parturition. (p. 28.)

We proceed with Dr. Weber to the " colour and consistency" of the cerebral substance. (p. 43.)

" General humidity of the brain of new-born children is one of the most frequent appearances met with, and is almost always found where the whole vascular apparatus without and within the cranial cavity indicates, partly from extravasation which has ensued, partly from great injection of the capillaries, a highly congestive condition. . . . . Individual cases frequently occur, where, notwithstanding the existence of the latter, the brain is relatively dry, or, at most, only normally humid." (p. 45.)

Dr. Weber thinks it not unlikely that in the latter instances, the sudden and severe determination of blood to the brain rapidly produced such a state of apoplexy, as to cause the death of the child before there was time for serous effusion to ensue.   Amongst the causes of " œdema of the brain," Dr. Bednàr refers to protracted face-presentations, which are often accompanied by hæmorrhage into the substance of the brain.   The colour of the brain varies very much according to the amount of blood in the capillaries; transitions from the lightest pale red to a bright red tint may

be seen. Occasionally, where the capillary injection is but slight, the humidity above spoken of is greatest. True capillary apoplexy, so far as *post-mortem* evidences can show, is, according to Dr. Weber, rare in the new-born child; whilst Dr. Bednàr remarks—

" Cerebral hæmorrhage consists in the extravasation of blood into the substance of the brain. It is found in new-born children most frequently in the form of capillary apoplexy, by which the substance of the medullary or cineritious matter is coloured red from punctiform and streak-like extravasations, the intermediate cerebral substance being of a normal colour, or reddened and of soft consistency." (p. 36.)

The author appears to have in view children who survive the birth for some days or even longer, as he afterwards refers to gangrene of the navel as a complication of the cerebral hæmorrhage when the sanguinolent clot is of rather a large size. (p. 37.) Anæmia of the brain in children a few days old is mentioned by Dr. Bednàr, hæmorrhage from the umbilical arteries being one of its causes. (p. 41.)

To return to Dr. Weber:

" In relation to the abnormal consistency of the brain of the child, I have yet to make mention of a condition I have but once observed. This was decided sclerosis of the greater part of one hemisphere."

" The right hemisphere was felt to be of a cartilaginous hardness; the left, of normal consistency. On section, the former cut like soft cartilage, the left not so. For comparison, both hemispheres were elevated to the same level. The left showed normal grey and white substance; the right, cartilaginous, hard, grey matter, bordering upon a light brown hue. At a greater depth the medullary substance was less hard, whilst the outer grey portion was so indurated as to give the former the character of being surrounded by a cartilaginous edge. On still further section, sometimes the grey, sometimes the white matter appeared hardest. Where the brain was of normal consistency, as in the left hemisphere, the capillaries were fully injected, while the whole of the sclerotic portion was in a state of complete anæmia. Further, the *corpus striatum* and the posterior portion of the *thalamus* on the right side were indurated, while the same parts on the left were healthy. The ventricles contained some amount of fluid tinged with blood." (p. 47.)

The child had strong convulsions the first week after birth, but lived until it was six months old.

Alterations of the colour of the ventricular plexus, of the septum lucidum, of the contents of the ventricles, and the occurrence of hydatids in the brain, are next touched upon by Dr. Weber. Upon these points we shall not dwell, but pass on to chronic hydrocephalus. On reference to the works of Rilliet, Fabre, West, &c., and to our article in the 6th volume of this Review (p. 140), the reader will find fitting introduction or commentary to the somewhat curt observations of our author on this malady. But from these we shall take the following extract, as it touches upon the litigated question, as to what is in most cases the origin of congenital internal hydrocephalus.

" There prevails up to the present moment, as Hyrtl, in his 'Manual of Anatomy,' avows, much obscurity concerning the anatomical relations of the meningeal membranes in the interior of the brain. I am of opinion, that where we are deprived of the possibility of investigation in the healthy brain, pathological conditions are often of especial service in clearing up the difficulty. If the dissections which I have had opportunity of making, in connexion with chronic hydrocephalus, had not been prosecuted at a time when I was less mindful of the

prevailing doubts of the anatomical relations of the arachnoid in the ventricles, they would have offered me the means at least of more facile inquiry. This much I can affirm with precision, that concerning the entire investment of the cerebral ventricles by the pia mater, there cannot any longer be the slightest doubt, when the pathological relations of this membrane to the walls of the ventricles have been observed. The pia mater (and the arachnoid also must be here included) in chronic hydrocephalus is morbidly changed to such a degree, that it clothes the cerebral ventricles as a thick, firm, opaque, scarcely tearable membrane. It is, undoubtedly, this serous membrane which is the originally diseased structure, producing the extensive effusion of serum, and moreover gradually becoming hypertrophied to a high degree. The other abnormal appearances met with in the brain, its inordinate distension, the almost membranous thinness of the cerebral matter around the ventricular cavities, as also the abnormalities of ossification, and of the form of the skull, are undoubtedly but consequences of the primitive disorder of the pia mater." (p. 49.)

Whilst upon this subject, we shall not hesitate in making the following quotation from the second edition of Dr. West's Lectures :

" The pathology of chronic internal hydrocephalus involves questions not merely of scientific interest, but of great practical moment; for if we come to the conclusion at which some observers of high authority have arrived, that it is almost invariably the effect of arrest of the development of the brain, all therapeutical proceedings must be worse than useless. The early date of the occurrence of its symptoms, in the great majority of cases, lends support, indeed, to the opinion, that the causes to which it is due must generally have existed before birth; for I find, on the examination of the history of 54 cases, 18 of which came under my own observation, that some indications of it were observed in 50 of this number before the child was six months old; that in 14 of these, its symptoms existed from birth, and that in 21 more, they appeared before the completion of the third month. The knife of the anatomist, too, has discovered evidences of congenital malformation of the brain in some instances, in which no sign of hydrocephalus was apparent until several weeks after the child's birth—a fact which still further deepens the dark colours in which this malady has been portrayed. . . . . Still, large as is the proportion of cases in which symptoms of chronic hydrocephalus have existed from birth, I am disposed to believe the exceptions to this not to be so extremely rare as some imagine, and am further of opinion, that, even in cases of congenital hydrocephalus, inflammation of the lining of the lateral ventricles, such as produces it after birth, may, in some instances at least, have excited it during fœtal life." (p. 91.)

Dr. Weber follows up his observations on chronic hydrocephalus, by remarking upon the frequent simultaneous existence of disease of the central organs of the nervous system, and of deformities of the hands and feet. The first paragraph devoted to the subject we think is amply sufficient, and consider that the further and long disquisition upon club foot, the relations of the tarsal bones, malformation of the knee-joint, &c. &c., are quite out of place in a systematic treatise, ostensibly limited to the pathological anatomy of the brain and spinal cord. We think that Dr. Weber has seriously injured the systematic character of his book by such a ridiculous interpolation.

" Pathological changes of the Spine" are considered in the second division of his work.

" The spine, in all its portions from the surrounding muscles to the cord itself, presents such important morbid alterations in the corpse of the new-born child, that in no examination of the latter can close investigation of it be neglected, without the risk of overlooking that which is most important, and of remaining in

ignorance of the particular circumstances causing the death of the child during birth, or preventing respiration from being established after birth, though strong pulsations were present. As long as I neglected opening the spinal canal in neonati, and sought for morbid changes only in the larger cavities, many phenomena, when compared with the nature of the labour, remained without explanation, which the autopsy nevertheless — as experience has taught me — might generally have explained." (p. 60.)

"The morbid alterations observed without and within the vertebral canal, may, with few exceptions, be included under two groups—viz., those having reference to abnormal distribution of blood, and those connected with morbid effusions. The former are not rarely met with alone, but frequently also in connexion with the latter, and as causative of them. The effusions here, as in the head, are often caused by unfavourable circumstances during parturition. In certain special diseases of children, observed during life, in the general convulsions of infants, in trismus, and in spina bifida, extravasations and exudations always play a chief part. The diseases of the new-born child particularly, so far as they have been observed in the head and spine, are throughout much simpler than in the adult, and consequently furnish less variety in post-mortem examination. What we do find, however, exerted great influence upon the life and health of the child." (p. 61.)

The author alludes, at some length, to the colour and distribution of the blood in the spinal muscles, and to the various circumstances inducing changes in the above. A pale colour of the muscles is seen in almost all children born more than a month before the time, in those who during uterine life have been influenced by a dyscrasic condition or malady of the mother, and in those dying during or soon after birth, the mother suffering from puerperal fever. But children born at the full time, and uninfluenced by any evident disease, in whom the skin is particularly white and fine, and the muscular and osseous systems very weakly developed, exhibit, almost without exception, pallor of the spinal muscles. On the other hand, the act of labour often bestows on these muscles, especially the cervical ones, a highly intense red hue. This of course chiefly depends upon great capillary injection; but in some cases rupture of the minute vessels ensues, and extravasations are formed. Passing from without to within the spinal canal, we may remark, that the venous plexus surrounding the coverings of the spinal marrow is often congested, and, in some cases, suffers rupture, consequently permitting extravasations to ensue. These, as in the case of those within the cavity of the cranium, are not unfrequently causes of the child not breathing, although born with a pulsating heart. The spinal cord suffering pressure cannot impart its influence to the periphery at the moment of birth. The amount of extravasation varies considerably. In one instance, Dr. Weber saw the dura mater of the cord, from the atlas to the sacrum, covered, both on its posterior and anterior surfaces, with a thick layer of semi-coagulated blood, but he has never seen extravasation between the periosteum and the bones, as occurs in the cranium. The cellular tissue between the posterior surface of the dura mater and the inner face of the vertebræ, is often the seat of effusions and exudations; one of the most important and—almost without exception—fatal forms of which consists in

"The true croupose exudations of this locality formed during the course of previous *meningitis spinalis.* These are not to be confounded with an adipose layer, sometimes found on the dura mater of the spinal cord of well-nourished children." (p. 67.)

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The signification of the capillary injection of the dura mater itself is indicated by the accompaniments of such injection; on the one hand, they demonstrate its congestive, on the other, its inflammatory character. In the pia mater, injection of its vessels, with exudation into the sac of the arachnoid are found. Frequently the arachnoid bursts, the fluid effused into its sac is discharged, and the spinal cord, throughout its whole length, swims as it were in water. As a consequence of spinal meningitis, yellowish white croupose deposit is observed, here and there, upon or over the entire course of the pia mater. Softening of the spinal marrow is not uncommon, but true sclerosis of it Dr. Weber has never seen. Occasionally it is somewhat harder than normal; such was observed to be the case in some forms of trismus.

" In conclusion, I have to make mention of two particular diseases observed after birth in living children—viz., trismus and spina bifida. Where trismus has occurred, I have always found the chief pathological changes in the vertebral canal, and not in the umbilical vessels. These changes, too, have been found to be pretty nearly of the same character in all the bodies of new-born children who have suffered from the disease. In those also with spina bifida, and who have lived a considerable time after birth, no material difference existed between the anatomical alterations found in the different bodies after death, and I have examined several. In both affections, the coverings of the cord, as well as the cord itself, were morbidly altered." (p. 71.)

Upon the subject of trismus, however, Dr. Weber and Dr. Bednàr are greatly at issue, as the following extract from the latter writer will show :

" The most careful examination of the dead body has not allowed us to discover any visible material foundation of tetanus, even preparations of the separated nerves led to no desirable end, and we must therefore conclude, that the tetanus of new-born children is a nervous affection, of whose essence we are in ignorance, and which consists neither in anæmia, nor in hyperæmia, nor in inflammation of any part of the nervous system, or still less of any other organ." (p. 157.)*

In one respect Dr. Bednàr's opinion carries more weight with it than does Dr. Weber's; and indeed this remark is applicable to any other variation which may be discovered in their treatises. Dr. Bednàr always furnishes us with exact numerical data to assist our judgment. Dr. Weber never does so; he speaks of having " very often," " not rarely," " sometimes," &c., &c., observed certain abnormalities, whereas Dr. Bednàr gives the proportion of their occurrence in a given number of autopsies. In the example before us, we know not how many children who have died from trismus Dr. Weber has examined, and therefore we are ignorant what constancy the morbid phenomena he details may bear to any given proportion of cases. That there exist exceptional cases to the category of which he relates one as an example, is admitted by himself in his previous remarks upon hardening of the spinal cord, which condition he has actually found in the affection we are alluding to; whereas the " model" case he presents us with, showed " the spinal marrow throughout to be pretty soft." (p. 73.) On the other hand, we learn from Dr. Bednàr, that he examined thirty-three children who had died from tetanus, that three times he found hyperæmia of the spinal membranes, three times blood in the arachnoidal sac, and once serous infiltration of the coverings of the cord,

* We may refer the reader with advantage to the Lectures of Dr. West, second edition, pp. 135, et seq., for a further critique upon this point.

&c., &c. (p. 158.) We are far from wishing to see works on pathology presenting the character of books on arithmetic; but *precise* medicine absolutely demands from those dealing with obscure and litigated points of morbid anatomy *precise* data, ere it will yield to their opinions. That we have on the whole formed a very good opinion of Dr. Weber's work, will be sufficiently evident from the use we have made of it—an use, indeed, so lengthy as to have prevented us from dwelling as long as we otherwise could have wished upon the very able treatise of his fellow-labourer, Dr. Bednàr; we trust, however, soon to meet with both of them again.

In our notice (vol. v. p. 362) before referred to, of the elaborate work of Dr. Hügel, on the various European medical establishments for children, we took occasion to inform our readers of the exertions made by Dr. Mauthner, in establishing at his own cost a small hospital for sick children in Vienna, which afterwards, by the pecuniary assistance and favour of the Empress, Maria Anna, was placed on a more stable foundation, eventually receiving the countenance and support of the State. As far back as 1821, it was ordered by the governing bodies, that students of medicine should receive clinical instruction on the diseases of children; but the order was virtually of little use, as no hospital for children then existed. Still the necessity for such important instruction was apparent, whilst in London, in 1852, we deem it of no matter.* By the Emperor's permission the above order was directed, in 1849, by the Minister Leo Thun, to be carried out in the "Hospital of St. Anna," under the supervision of Dr. Mauthner, so favourably known to the profession, not only by his exertions in founding the institution, but by his work on 'The Diseases of the Brain and Spinal Cord,' reviewed by one of our predecessors.† In April, 1850, the class of clinical instruction was opened, to which 51 students subscribed their names, of whom 23 were diligent attendants, and framers of clinical reports. Dr. Mauthner commences his own Report with some remarks taken from his introductory lecture to the course. It gave us no small amount of pleasure to find that the very first words of that lecture were not only identical in spirit, but very nearly so in words, with those with which we commenced an article in this Review; and we recommend the further observations of the author to the notice of our readers, only regretting that they are so short, since they are so good, and that the following extract is all for which we can afford space:

"Great value is to be attached to the observing a child during sleep, for many affections do not become evident until the child is seen sleeping. Very much becomes apparent during slumber from automatic movement, from moaning and sighing; and in relation to the pulse it is remarkable, that in many children it is of quite a different character from what it is when they are awake. In observing a quiet and awakened child, we must before all things direct our attention to the expression of its features. For if it is ever true, it is so of the child, that the countenance is the mirror of the soul. In its lineaments all is veracious; no dissimulation, no hypocrisy, no passion, has disturbed this mirror; and whosoever can read therein, to him is the diagnosis of the ailments of infancy essentially facilitated." (p. 5.)

Dr. Mauthner's Report embraces the first two clinical sessions of 1850, 51.

* We have no doubt that valuable clinical instruction will be offered to students, at the new Hospital for Children lately opened; but we fear that unless the Examining Boards recognise the absolute necessity of attention being paid to it, the *elèves* will be but few in number.

† British and Foreign Medical Review, vol. xxi.

During the first session 71, and during the second 131 children came under investigation. The sexes were equally divided, the ages extending from birth to 12 years. The deaths were 62, and the mean duration of the treatment of each child, irrespective of sex, was 19 days. From the tabular statement appended, we extract the following:—7 boys, 6 girls, had hyperæmia of the cerebral membranes, 2 died; 21 boys, 7 girls, had pneumonia, 8 died; 4 boys, 6 girls, had catarrh of the intestinal mucous membrane, 2 died; 6 boys, 11 girls, had measles, 4 died. No case of hyperæmia of the spinal membranes, or of chorea, occurred amongst the boys, but 2 cases of each amongst the girls; 1 case of diabetes insipidus occurred amongst the boys, not one amongst the girls. Syphilis was equally divided, as also chronic hydrocephalus, and inflammation of the membranes of the brain. The chief subjects chosen for clinical illustration during the first session were connected with "the catarrhal process in childhood." The first case reported by Dr. Mauthner is one of inflammation and prolapsus of the rectum, of which the child died. After death, the signs of chronic follicular catarrh of the upper intestines, besides disease of the lower bowels, were observed. The second illustrates " passive hyperæmia of the brain."

"On the 23rd of April, a mother brought her daughter (Anna Schröder), seven years old, to the clinical department, in a completely unconscious state. The face was pale and bluish, the eyes closed, the pupils undilated, the mouth half open, and there was some stertor. The mother stated that the girl had become sleepy whilst at school the day before, between eight and nine o'clock, that she had afterwards at home complained of sickness and of headache, had vomited yellow-green fluid, and at length, under increasing headache, had fallen into this soporose condition. No external cause could be discovered. The pulse was small, soft, ninety-six, the respiration right and left, anteriorly and superiorly, slightly vesicular, posteriorly and superiorly on the right side during subdued breathing slightly bronchial, on the left indeterminate. Heart's sounds normal, the second pulmonary sound somewhat increased, abdomen sunken." (p. 7.)

" Anamnestically* it was elicited that this girl had been languishing for some time, and had been treated here as an out-patient for pulmonary tuberculosis hereditarily derived from her father; that about three weeks ago she had had varicella whilst at home, been badly nourished, and lived in a damp locality." (p. 7.)

" *Remarks.*—The state in which the child is at present, renders it indispensable that the physician should arrive at a diagnosis. Upon the character of the diagnosis life here depends. There cannot be the least doubt that in the central organ of the higher nervous life the source of the phenomena is to be found; but this is not sufficient for the *therapeia*, and it must first be determined whether exudation or simple hyperæmia of the brain is present, and if the latter, whether it be of an active or passive character."

It was now determined that there was no apoplexy to deal with, that there was no connexion between the earlier pulmonary affection and the present condition, and that the latter was not dependent upon exudation having occurred. It was observed that hyperæmic states are very frequent in children, and that when occurring in the brain they give rise to symptoms similar to those before us. The question then was, is it active or passive hyperæmia?

" When we reflect that no external cause of injury has existed, that no reaction of the nervous system like convulsions has appeared, that in the lungs,

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* Anamnestisch—a term getting into rather frequent use among the Germans. Vide Index to Feuchtersleben's Medical Psychology, published by the Sydenham Society.

stasis and probably also tuberculosis are present, and especially, that many depressing circumstances have influenced the life of the blood, by which it could become altered in its constitution, and the vessels have suffered a diminution of their contractile power, so is it probable that the closeness and effluvia of the school-room and the mental exertions of the child have brought about passive congestion of the cerebral vessels."

" For these reasons, only, cold applications were ordered to the head, leeches and purgatives not appearing to be indicated. Low diet. After some hours, evident diminution of the sopor and some broken sleep occurred; in moments of wakefulness she appeared even conscious; she passed much urine; urine slightly acid, sp. grav. normal, without sediment, or only mucus.   Bowels not relieved; pulse 108. Inf. fol. sennæ ex. dr. j, aquæ unc. ij., sacch. alb. dr. ij.   Improvement rapidly followed. On the 30th of April she was discharged well." (p. 8.)

Dr. Mauthner, in his remarks on the fourth case—one of simple pneumonia—draws special attention to the great value of venesection as a quick and surely effective therapeutic agent (all other resources being neglected) in certain forms of the malady; whilst, on the other hand, in mild forms of pulmonary inflammation, a purely expectant treatment can be followed with equivalent success. The facility with which simple pneumonia may be confounded with " gastric-typhoid fever" is evidenced by case the sixth:

" That increase of and qualitative changes in the fibrin of the blood are found to be favourable conditions for the occurrence of croupose products is not to be denied, since it is a clinical fact, that in many cases such a state of this fluid precedes the local affection.   But as our knowledge concerning the blood-crasis is yet very imperfect, it may occur to the most experienced that croupose exudations shall be formed, of whose existence he is ignorant or only knows when too late. I have myself experienced that in organs whose functional disturbances are easily diagnosed (as is the case with the mucous membrane of the air-passages), pseudo-membranous products may be perfectly formed without attracting notice; how much more possible is this to happen in the gastro-intestinal mucous membrane, whose disorders rarely evince themselves by very pregnant symptoms.   Therefore it becomes the more necessary, since the symptoms of abdominal disorder in children are so obscure, to regard more narrowly every departure from a normal condition, and not, as often is the case, to view them as of little account.

" That the exanthematic excitement of the soft and very vascular mucous membrane of the stomach of children may proceed to exudation without betraying itself by urgent symptoms, the following case will show." (p. 15.)

In the case (ninth) here alluded to, the upper portions of the larynx, the epiglottis and the fauces, were covered with croupose exudation, partly in a semi-fluid state of decomposition, whilst the mucous membrane of the œsophagus was pale, and deprived of its epithelial layer, and that of the stomach was dark red in colour, detached, and covered with a semi-decomposed croupose layer, of the thickness of the back of a knife.   At page 32 of the Report it is stated, that during the epidemic of measles, which occurred in the summer, croup was a very frequent event, both as a consequence of the measles and as an independent affection.   The exanthematic affections of the mucous membrane of the air-passages and of the intestinal canal were most dangerous during the period of cutaneous desquamation.   We would here beg to refer to an article in our fifteenth number, (p. 69,) where reference is made to a paper by Emfris, and to our remarks upon croup, when reviewing the work of Dr. Meigs, as not out of place in accompanying the perusal of Dr. Mauthner's observations upon

various croupose affections.   Scattered through the Report before us, there is much that is valuable upon measles and scarlatina, and their complications.   Cases 11 and 12 afford Dr. Mauthner an opportunity of touching upon syphilis.   As we have already dwelt at some length upon this affection in the child,* we shall here only observe that our own experience does not coincide with Dr. Mauthner's, who finds *pemphigus* the most frequent cutaneous manifestation of the virus in children; forms of *psoriasis* we most commonly meet with.—The thirteenth case in the Report is well deserving of extract:

"It occurred in a girl seven years of age, who for two years and a half had been brought up as a foundling by the mistress of a boarding-house, and until now had remained quite well.   Four days ago she came from school as pale as a corpse, became very hot, and had pain of the neck; during the night delirium supervened. At home a cataplasm was applied to the neck and a purgative given.   As she got worse, however, she was brought to the hospital on the 20th of October, and was immediately received into the clinical department.   The patient seemed strong and well developed for her years, had brown hair, was quite unconscious and delirious; the breath was very offensive; the balls of the eyes rolled outwards, their whites finely and brightly injected, and the cheeks were circumscribedly reddened.   The mouth was half open, the lips and gums brown and dry, the teeth dull and brownish, the tongue very red, the tonsils and papillæ of the tongue greatly swollen, the breathing short, and a hacking loose cough was present.   On the left side, from above downwards, the sound on percussion was dull, and the respiration bronchial with mucous rattle.   There was nothing abnormal as respected the abdomen, the skin was cool, the fingers and toes were blue and cold, the pulse was small, rather hard, 100.

"He who has stood by the bedside of a patient hurrying towards death from an obscure and severe affection, and surrounded by students, who judge his words and deeds; he who has felt that life is dependent upon what he orders, that his gestures, his demeanour, his conduct and his speech determine the judgment that is about to fall upon him, and that vocation, respect, nay, even existence itself, at such a moment, stand at stake—such an one is well aware how difficult is the office of a clinical instructor.

"After the attention of the students had been directed to the important fact, that the child had become ill without any premonitions, that she had probably brought the malady from school, and that a simple pneumonia does not prove fatal in four days, it was decided, that the anginose conditions, the colour and state of the tongue, appeared to indicate *Scarlatina perniciosa* to be the right diagnosis." (p. 24.)

The disease ran a course of a month, the patient recovering.   No exanthem made its appearance, but about the seventeenth day desquamation of the skin ensued, and continued for more than a week.   Ischuria occurred also.

"If the scarlatina, in this case, ran its course only in the form of a pure exanthematic process, *without exanthem,* the subsequent desquamation is difficult to be accounted for.   We know, however, that amongst the poor a transient scarlatinoid exanthem is constantly overlooked; perhaps such was the case here." (p. 27.)

During the winter of 1850–51, scarlatina prevailed; measles afterwards supervened, and continued throughout the year.   As the former epidemic declined, and the latter came on, the combination of the two exanthems was frequently observed.   Cases also occurred, in which, on the third day of the eruption, the measles being at their highest, the scarlatinal exanthem

* Vol. vi. p. 345, *et seq.*

made its appearance. Beneath the morbilloid papulæ, analogous shaped scarlatinal redness existed. The pulse was very frequent, the tongue purple-red, and marked encephalo-symphoresis present. (p. 34.) In other instances, the two eruptions broke out at the same time, the affection being then always very severe. A case of " noma genitalium" is given, in which, after the usual remedies were resorted to without avail, the actual cautery was employed. The child died. One or two purely surgical cases are also detailed.

The Clinical Report we have thus introduced to our readers is a highly-interesting and instructive one, and affords good evidence of the value which may be attached to a course of clinical instruction upon the diseases of children, under so able a physician as its editor.

Mr. Taylor's Essay on Remittent Fever has already appeared in the pages of the ' Medical Gazette,' its author having been awarded the prize of thirty guineas offered by the South London Medical Society, in 1849, for the best treatise on the subject. Mr. Taylor appears to have made himself well acquainted with the writings of British authorities upon the affection in question, and illustrates his own views by numerous cases and statistical reports. As, however, these views have been now for some time before the profession, we shall appeal to Dr. Schoepf-Merei upon a point or two connected with fever in the child :

" In almost all the fevers of children, headache and gastric disturbance are present, constituting often very prominent phenomena, and frequently leading to an erroneous treatment, as their import is mistaken. Febrile headache, an obvious, but often little-cared-for symptom in the adult, almost always gives rise to serious apprehension in the minds of the parents of a sick child ; and the medical attendant, frequently considering it as indicative of the commencement of meningitis, becomes prodigal of leeches and mercury. It may happen that diaphoresis follows, and that a case of meningitis (!) is said to have been cured. But the result of such therapeutic interference is not always so favourable, for the organism of a delicate child very frequently becomes unable from the effects of depletion to pass through a natural crisis. No less an amount of mischief often follows, also, what may be termed the anti-gastric treatment. Both errors in practice are not uncommon, since slight soporosity, convulsions, or vomiting, are not unfrequently the *prodromi* of the acute fever of children. On the other hand, it must be confessed, the above-mentioned and other local affections sometimes complicate the course of the febrile disorder. It becomes, therefore, a matter of the highest importance to study the physical signs of this malady in children." (p. 77.)

Dr. Schoepf-Merei takes great pains in describing "febrile headache in its different forms, and possible transition into meningitis," and " febrile gastric disorder, and possible transition into a local affection." He acknowledges the difficulties of the task, and considers that if a previously-healthy child is suddenly attacked by what may be assumed to be fever, it is in general preferable, in spite of the violence of some of the symptoms, to wait about fifteen hours, or even longer, before recourse is had to " active treatment." By such expectancy, many cases of " acute fever" occurring in the hospital at Pesth, are considered by the author to have recovered in a comparatively short space of time, almost without any strictly therapeutic interference. From 1839 to 1845, one hundred and fifty cases of intermittent fever were admitted into the Pesth hospital. " During the first months of life, we never observed a perfectly expressed intermittent

paroxysm, such as may be seen in children three years of age or more,"
(p. 138.) In the latter, Dr. Schoepf-Merei considers that the cold stage
is proportionably shorter than its average duration in the adult. Fits of
eclampsia were found not unfrequently to complicate the paroxysm of
the fever, particularly in the younger patients. They occurred throughout
the general course of the cold stage, and at the maximum of the hot. The
author points out the frequent mistake made, and its resulting mischief, in
confounding the commencement of a paroxysm of ague with inflammation
of the meninges; the latter, it should be recollected, " never commences
suddenly." It is stated to be bad practice to treat eclampsia appearing
suddenly, or at least shortly after some symptom of acute fever, by
leeching.

Art. II.

Observations in Surgery. By Benjamin Travers, Jun., F.R.C.S., Lec-
turer on Surgery, &c. &c.—*London,* 1852. 8vo, pp. 230.

Mr. Travers writes under every advantage that can stimulate the
energies of an honourable and ambitious man. Desirous of advancing his
own reputation, he had to take care that his efforts reflected no discredit
upon his hereditary connexion with surgery; and was doubtless aware,
before he began to write, that in courting the gaze of the public, he had
also to guard against some risk of invidious comparison with his distin-
guished father. To that father this maiden essay is dedicated. It is the
first-fruits of an "uninterrupted intercourse with a mind animated by the
philosophic spirit of research and observation," which Mr. Travers, sen., has
given ample evidence that he possesses in large measure; and this little
book, therefore, claims from us no common share of indulgent attention.
With every disposition, then, to temper criticism with kindness, we cannot
conceal from ourselves, or our readers, that, in this publication at least,
the pupil has failed to prove himself worthy of his master. We search in
vain for evidence of the " philosophic spirit" just alluded to; and in its
stead are painfully struck with the marks of superficiality, which constitute
the most prominent feature of the whole performance.

Containing materials which more time and labour, and a more me-
thodical habit of mind, might have worked up into a volume of sterling
merit, this treatise seldom rises to a standard of higher value than that of
a collection of miscellaneous remarks, such as might, without much diffi-
culty, have been amassed by a student of average ability during his house-
surgeoncy to a large hospital. The best part of it is undoubtedly that
which relates to Injuries of the Head; and this section, we are very glad
to say, exhibits no lack of promise or originality, and is capable of being
rendered highly valuable and instructive. As for the rest, the MS., in our
opinion, had better have been retained in the author's desk, until such
time as he could elaborate it more fully; and this, we cannot help sus-
pecting, might have been the advice of Mr. Travers's father, as we feel
sure that it would have been that of all who have his reputation sincerely
at heart. It is never agreeable to censure the first efforts of a junior and
a meritorious member of our profession, particularly when, as in this

instance, we feel disposed to extend to him every indulgence; for we thus find it doubly hard to reconcile the conflicting claims of duty and inclination. But, as honest reviewers, we must guard against extraneous influences, as carefully as if we were judges upon the bench; and being careful only for the truth, must utter, without fear or favour, our unbiassed opinion of any literary performance submitted to us.

That Mr. Travers, however, may have no cause of complaint against us, we shall also follow the practice of most modern judges, and state the grounds upon which our opinion is formed; as it is upon the justice of these alone, that the real value of our judgment must depend.

About seventy pages of Mr. Travers's book are devoted to the important subject of Fractures; and the best idea we can give of our impression of them, is to say that they are made up of desultory observations, strung together upon an arrangement, which, defective to begin with, has been most imperfectly carried out. They convey more the notion of loose conversation, such as one might hold over a cheerful after-dinner fire, than of statements deliberately made and calculated for the public eye. Part I. treats of "Forms of Fracture," "Oblique Fracture," "Comminuted Fracture," "Split Bones," "Compound Fractures," and "Extravasation." As a sample of the mode in which these subjects are handled, we present our readers with the whole information vouchsafed to them on the subject of Comminuted Fracture:

"Comminution is chiefly remarkable for its great diversity of aspect. The term is employed when the bone is broken into three or more pieces. A badly comminuted bone may be of itself a sufficient reason for the removal of a limb. It can hardly occur but under the infliction of a force which crushes all the textures proper to, and in the vicinity of, the bone. The life of this part is sometimes destroyed outright, or the textures so damaged that union by bone cannot be anticipated. Abscess is a result of comminution; by this means fragments are detached, or a portion of the shaft dies and exfoliates: both effects have been noticed in the same limb. Where parts are much bruised and loosened, the smaller pieces, if detached, should be removed at once. Under such circumstances the limb may be restored, with a variable amount of shortening and deformity. There are parts of the skeleton especially liable to comminutions, both from their structure and situation. I would instance the foot, the face, also flat surfaces, as the cranium, scapulæ, and wings of the ilia. The process of repair is here necessarily slower than after injury of a more simple nature. A larger production of bony matter is requisite, and the progress of ossific union is not always uniform. It may be far advanced in one situation, whilst at no distant point it has scarcely commenced.

"In the extremities, few badly comminuted bones escape the *ultima ratio* of surgery—viz., amputation." (pp. 11, 12.)

Elsewhere, we find that Mr. Travers considers it "a sound practice without exception," where a fracture has taken place in the lower third of the tibia, to place the leg upon the outer side; but this, though in most cases very good practice, is certainly not of such universal application as our author seems to think; for the back splint is often much more agreeable to the patient, and equally advantageous as regards the fracture. He also makes the sweeping assertion, that "split bones do not unite." (p. 14.) This observation, if correct, would be a very curious one; but experience shows that the fact is not as here stated. Many of the examples of fractures into the elbow-joint are genuine splittings of the bone; and we all

know that such fractures not only do unite, but frequently occasion great inconvenience by the large amount of bony matter which is thrown out into the joint. Very recently, we ourselves dissected an excellent specimen of such a splitting of the lower end of the humerus, which had happened many years before the patient's death.

The second chapter of Part II.—"Remarks upon the Constitutional Treatment of Fracture, Simple and Compound, with Cases"—does Mr. Travers more credit, and may be profitably perused. The next division— " Miscellaneous Observations upon the Remote Consequences of Fracture"— contains the following list of subjects: "Of getting up," "Tetanus," "Hæmorrhage," " Non-Union," " Period of Life at which Fracture occurs, as it concerns the question of Repair and Recovery, &c." ; and, we must admit, presents ample claims to the epithet " miscellaneous" prefixed to it. Thus delirium tremens, surely as common and important " a remote consequence of fracture" as tetanus, is violently divorced from its natural situation in this place, and put to do penance by itself in the shape of a " *note*" at the end of the former chapter. The pith of the note seems to be, that " Opium is the remedy which never fails to compose the patient, but to this end it must in certain instances be administered with great liberality" (p. 54); and again, that its action ought to be watched, because—

" Diarrhœa not unfrequently ensues, which appears to depend either upon a hyper-irritable state of the surface of the bowels, or an absence of healthy bile. Sometimes the chalk mixture with aromatics is an effective remedy. Occasionally minute doses of blue pill with the Dover's powder are employed, with or without the absorbent and aromatic draught above mentioned. The local repair is always retarded by these attacks, so severe indeed as occasionally to compromise the injured limb. But amputation is here a doubtful expedient, and by no means uniformly successful." (p. 55.)

Now it is by observations such as these, that we are made to feel provoked with Mr. Travers. He has here, from his own experience, derived the clue to an important point of practice, but has entirely failed to follow it out, or to put the right interpretation upon facts which he has noticed for himself, and which might have done him some credit had they been better used. In place of this, however, he presents us with a sweeping paragraph in favour of opium, which he must surely be aware is unsupported by the experience of hospital surgeons, albeit it is an opinion handed down from one text-book to another as a sort of medical axiom. Has Mr. Travers never seen a patient dosed to the full extent with various opiate preparations, with the pupils contracted to a pin's point, and exhibiting all the outward signs of the action of the remedy, whilst the delirium continues unabated, and the patient at last perishes, from the combined effects of the disease and its vaunted antidote? There are some cases—we refer especially to the delirium of inveterate porter-drinkers, so often witnessed in London—in which the usual unmitigated and uncombined opiate treatment is wholly useless ; and it is precisely in these cases that nature, apparently desirous of pointing out the way, sets up that diarrhœa which Mr. Travers is so anxious to check. Assist that diarrhœa, however, or rather induce it, by the exhibition of two or three five-grain doses of calomel, followed up by black draughts, and then commence the opium; and the results will convince the most sceptical. Those to whom the plan

is new can hardly realize, without seeing it, the exceedingly fœtid tarry-looking bile of which the gorged liver and gall-bladder relieve themselves, under the influence of drastic purgatives. The fact is, that in these porter-drinkers the blood is already half poisoned by the retained biliary secretion; and if the vital fluid be rendered still more impure by the opium, the patient's chances of recovery are reduced indeed to an infinitesimal amount. That delirium, however, which attacks the small, wiry, thin-looking spirit-drinker, is of a different character, and requires the early and almost exclusive use of opiates.

We have already intimated our favourable estimate of the next chapter, "On Injuries of the Head." Mr. Travers proposes to reduce all classes of head-injuries, exclusive, of course, of incised wounds, to the two divisions, "Commotion," and "Compression"—the term commotion being equivalent to concussion. At page 85, he reduces the whole to a tabular form, thus:

" *Commotion.*

" 1. Fatal, without lesion or reaction of any kind (rare).
    2. Simple, with a brief and healthy reaction (ordinary concussion).
    3. Obscure, with a slow reaction, and recovery more or less complete.
    4. Lesion of the substance of the brain with or without fracture of the cranium, uniformly fatal in the latter, and ordinarily so in the former case.

" *Compression.*

" 1. Directly fatal, whether from the nature or extent of the mischief.
    2. Not directly fatal but remediable by art, and occurring in one of two forms.
       a. Pure, by surfaces of blood or bone.
       b. Partial, by depressed edges or spiculæ, the symptoms being incomplete and marked by signs of irritation."

We are inclined to think well of this classification, which we consider truthful to what is observed in nature, and which has the further merit of being illustrated by some suggestive cases, that cannot fail of proving useful to the student. We extract the following as a favourable sample of the practical guidance which he may derive from this portion of the treatise:

"Head-injury is often remarkable for the great obscurity of its early symptoms, to say nothing of their variety. After the same kind and amount of violence, one man is rendered stupid, and his pupils, whether dilated or not, are inactive; he is drowsy, sick, and moans when spoken to; his pulse is feeble and slow, and his surface chilled. Another is reduced to a condition of positive insensibility, casts his stomach at the time of the accident, but not afterwards; is very cold, has a large motionless pupil, a slow labouring pulse, not unfrequently snores loudly; one arm or side of the face may be paralyzed, along with the bladder and lower extremity; the urine may or may not be permanently retained. The continuance of such a condition is one among the symptoms of a recoverable state, or its opposite. Perhaps there is an appearance of blood about the nares, or it has trickled in small quantity from the meatus auditorius. A third, after a severe blow or fall, is excessively violent and incoherent, requiring restraint to keep him in bed; the pulse bounds; the surface is not cold; and there is an expression of wildness about the eyes. Now, although any one of these conditions may exist, irrespective of any lesion whatsoever, they being all possible forms of recoverable commotion, it may be the forerunner of acute membranous inflammation, or of that hopeless train of symptoms which indicate lesion of the substance of the brain. There are many cases modified in their course and character by habits of intemperance or by a previous physical exhaustion, depraved secretions, or unfavourable mental conditions,

which are from such causes additionally obscure under the effects of injury, so that it is as necessary to inform oneself of the previous habits and position of the patient, when summoned to such accidents, as it is where the disease is idiopathic, or the symptoms of a more slow and insidious nature.

"I always direct the head to be shaved; even where the mischief is slight, it is a useful precaution. One sometimes discovers a wound or bruise by so doing, which might otherwise have escaped detection for days, and it is a positive relief to the entire region, as well as necessary to the efficient use of topical means. A spirit lotion, or a diluted Goulard wash, with the liq. ammon. acetat., are amongst the best refrigerants commonly in use; but cold vinegar-and-water answers as well as any other application.

"With regard to bleeding, abstraction of blood from the arm is of course at times indispensable, but often the slow oozing obtained by good leeches is not only more effective, but by far the safer practice. This more gradual operation of the depleting agent tells with great effect upon cases where the reaction is disposed to be tardy or incomplete. Patients slowly open their eyes and recover consciousness, after trickling leech-bleedings, who had been previously bled copiously from the arm without any evident good effect; and if the heart is too rapidly impressed by venesection *plenó rivó*, along with faintness, there supervenes increased congestion and fresh loading of the sinuses. The respiration under such circumstances becomes more and more impeded, or a fresh fit of violence comes on—excitement without power. Put by your lancet in such a case, for the patient will infallibly die if you persevere. Watch especially the venous circulation; where the pulse will bear the pressure of the finger, order an application of six leeches to one or both temples (I have laid them on one by one), bathe the wounds so made with hot water, and now look narrowly to the breathing, the countenance, and the pulse. When these measures are successful, the soft murmur of child-like repose slowly takes the place of the former retarded or irregular respiration; the face is no longer suffused, nor the features working and distressed; the lips are slightly compressed which lately hung in a semiparalyzed state, flapping, and loose; the pulse is now regular, 80° or 90° in the minute; the patient is reviving, and will reawaken to consciousness by-and-by, when the capillaries are sufficiently relieved, through the influence of a regular pulmonic circulation; the blood is now once more decarbonized, and when the heart in its turn begins to respond to its accustomed stimulus, the reaction may be deemed complete, and the patient is restored by gradually unloading, instead of suddenly emptying, the oppressed vascular tissues. As for counter-irritants, they are useful in most recoverable commotions of the brain, and their diffused action is at times very salutary. Where there exists an early tendency to excitement, they will sometimes control it in a remarkable manner when active depletion fails, or is plainly inadmissible. They are especially operative in those morbid conditions where there seems to be a want of power to rouse and sustain the action of the capillaries. In recent head-injury it is best not to irritate by maintaining a raw surface, or keeping the blister open, as it is called. The relief to be obtained by counter-irritants in such a case is not to be confounded with the proceeding indicated in chronic disease. The soreness and irritation of an issue are very undesirable here, and the exhaustion so produced may be of serious consequence to a person otherwise disposed to early convalescence. The regions best adapted for these applications are the crown of the head and the nape of the neck. The required irritation may be kept up for a period of six or twelve hours, according to circumstances. Blisters should never be used where there is reason to suspect that the brain is lacerated, or irremediably injured; and on such occasions no other objection need be advanced than this, that they are utterly useless." (pp. 89—93.)

The observations upon "Puncture of the Bladder" present nothing remarkable, if we except a letter from Mr. Cock, of Guy's Hospital, on puncture of the bladder per anum—a subject upon which he is well entitled to be considered an authority.

This chapter is followed by one " On several Disorders of the Male Urethra," &c., of which the section on the treatment of stricture commences with the following astounding statement :

" I do not admit the distinction between a temporary and permanent constriction of the urethra : the terms 'spasmodic' and 'chronic' I regard as inapt, and not descriptive of a complaint which exists but in one kind, and which is in its nature unchangeable, depending as it does upon the degeneration or disorganization of a healthy tissue." (p. 170.)

Such a piece of dogmatism carries its own refutation with it ; as also does the following : " It may be roundly asserted, that so long as surfaces remain unbroken, cutting instruments should not be used in the perineal region." (p. 172.)   And further on, speaking of the much-talked of perineal section, Mr. Travers says : " After cicatrization, the parts become as hard and callous as before.   Some of the worst strictures I ever dealt with, existed in a more unmanageable form than ever after this perineal operation." (p. 173.)   Here, then, is direct testimony to the failure of the operation so prominently advocated by Mr. Syme ; and we therefore call upon Mr. Travers, as an act of justice to the profession, who are anxiously expecting information, to publish the details of those cases to which he refers.   A few such facts well ascertained would go far to settle what is yet a very vexed question ; and we therefore tell Mr. Travers, that after the statement he has thus published, this is due from him, no less for his own credit's sake, than for the sake of science and humanity.

Following this, is a " note on lithotrity"—and a " case of lithotomy, in which no stone was discovered"—and in which we can discover nothing worthy of being presented to our readers.   We shall also pass over the chapter on " Forms of local Inflammation ;" pausing, however, to add our testimony to the entire correctness of the following important paragraph upon the treatment of contracted cicatrix :

" I have lately had an opportunity of witnessing some very novel and satisfactory results of the process of extension upon the fræna and contractions of old burns of the neck and limbs.   Mr. Tamplin, of the Orthopædic Hospital, the ingenious contriver of this plan of treatment, has succeeded in straightening and restoring parts so circumstanced in a most marvellous manner where excision had wholly failed, as it notoriously does upon these occasions.

" In one very bad case, a dense cuticular band, confining the forearm, and entirely suspending all power of motion in the injured limb, was completely reduced and disappeared in about twenty weeks after the application of an apparatus along the back of the arm throughout its extent.   The strain, which is never relaxed by day or night, has the effect, in the course of time, of producing an absorption of the substance of the band or web, and as the parts slowly yield under the stern influence of such measures, the power of motion recommences, which should be expedited by passive exercise from time to time." (p. 203.)

We cannot help here making the observation, that the principle of treatment inculcated by Mr. Tamplin, and practised by him at the Orthopædic Hospital, upon contracted cicatrices, is one of the most important improvements in the practice of modern surgery with which we are acquainted. The frightful operations resorted to for the cure of deformities thus produced, are amongst the horrors of our profession ; and almost make the blood run cold in thinking of them ; whilst, as is well known, their success was most doubtful, and their hazard to life very great.   We believe that this

plan of treatment is yet in its infancy; but that the day will come when our obligations to the institution in Bloomsbury-square will be less niggardly acknowledged, and more universally conceded.

The concluding chapter, " On Diseases of the Joints," we cannot regard as sound either in its pathology or its therapeutics.

" A white swelling," says Mr. Travers, " does not depend merely upon an ulceration of the cartilage, but is essentially an unhealthy chronic inflammation of all the parts entering into the composition of the knee-joint, beginning in a synovitis, and terminating in a true ulcerative absorption of the cartilage and bony apophysis in adults, or the epiphysis in young subjects : of course the same remark applies to the hip, ankle, elbow, and wrist." (p. 207.)

We venture to characterize this paragraph as highly incorrect. The true white swelling does not commence in an inflammation of the synovial membrane, but is a disease having its origin in the cancellous structure of the bones entering into the composition of a joint, and consisting essentially in the deposition of *scrofulous* materials, not necessarily or even commonly *tubercular* material, in the ordinary acceptation of the term. This deposition may continue for a length of time without producing any other symptom than an enlargement of the joint; which enlargement is evidently caused by a swelling of the bones entering into its composition, as may be ascertained by manual examination. There is no pain in these cases, until either the periosteum or the synovial membrane has become involved; and, as the readers of this journal are aware, Dr. Redfern's researches have made it highly probable, that the excessive pain complained of when the cartilages are undergoing the process of ulceration, is due, not to them, for they are destitute of nerves, but to the subjacent inflamed tissues.

In the treatment of strumous diseases of the joints, Mr. Travers is a great advocate for the internal use of mercury, as especially useful in the earlier years of life—a conclusion at which he arrived from witnessing the accidental salivation of a very young child, who laboured under the earliest stage of " morbus coxæ." The local treatment, we are informed, " should be soothing, but decided;" and consists in repeated small leechings or occasional cuppings, the former being preferable. " Four, six, or ten, fresh leeches, according to the age of the patient, applied twice in ten days, will allay swelling and local pain for a longer term, and at a smaller cost to the system, than the abrupt employment of double the number." (p. 213.)

" In the chronic stage," also observes our author, " the plan of permanent counter-irritation often fails. The moxa, for example, is of itself a very severe infliction, and seldom in my experience repays the patient for his fortitude in submitting to it. I am not sure that it does not often operate to disturb and interrupt the function of organs, without whose aid and connivance texture cannot be saved, nor the patient effectually rally." (p. 214.)

There is some truth in all this; and the leeching plan is, we are aware, a favourite with some;— but we have not been able to satisfy ourselves of its beneficial results, and have seen it do much harm. In our own practice, we eschew leeches, and above all mercury, except as an occasional alterative. If, in the progress of a case, circumstances compel us to resort to calomel, as circumstances may do, we regard the necessity as a lamentable one, certain to tell against the patient eventually. The local and continued application of the compound tincture of iodine frequently produces striking

results when the disease is in its early stage; and when hip-joint disease has unhappily reached its acutest form, the insertion of a caustic issue of the size of a fourpenny-piece, over the anterior crural nerve, will often obviate the use of mercury, and alleviate pain like a charm.

As we have mentioned the topical application of iodine, we may here observe, that on many parts of the sea-coast people are in the habit of rubbing diseased joints continually with fresh sea-weed, and, as they believe, with the best results.

We here conclude all that it seems necessary to say upon the merits of Mr. Travers's maiden essay; and can assure him that our criticisms have been conceived in the kindest spirit, and expressed with the deepest regret. That our requirements are not unreasonable, may be inferred from the circumstance that they have all been fulfilled in the volume of hospital experience, published some years ago by Mr. Ormerod, under the name of 'Clinical Collections;' and we know of no reason why we should expect less from St. Thomas's than from St. Bartholomew's. It is obvious that Mr. Travers has not kept pace with the recent progress of surgical knowledge; and we sincerely hope that he will make up for the deficiency thus occasioned, before he again solicits the public attention.

Art. III.

1. *Tal om Gymnastiken säsom Bildningsmedel och Läkekonst.* Af Dr. C. U. Sonden.

A Discourse on Gymnastics in their Relations to Education and to Medicine. By Dr. C. U. Sonden.—*Stockholm*, 1840. pp. 30.

2. *Kinesitherapie, ou Traitement des Maladies par le Mouvement, selon la méthode de Ling.* Par A. Georgii.

Kinesitherapy, or the Treatment of Diseases by Movements, according to the method of Ling. By A. Georgii.—*Paris*, 1847. 8vo, pp. 147.

3. *Om Nödvändigheten af Vetenskaplig Kontroll öfver Gymnastika Central Institutet, &c. Kritisk fremstallning.* Af D.

On the Necessity of Scientific Control over the Central Gymnastic Institute, &c. A Critical Essay. By D.—*Stockholm*, 1851. 12mo, pp. 103.

4. *Kinesipathy, or Medical Gymnastics for the Cure of Chronic Disease.* By H. Doherty.—*London*, 1851. 12mo, pp. 24.

5. *The Prevention and Cure of many Chronic Diseases by Movements.* By M. Roth, M.D.—*London*, 1851. 8vo, pp. 302.

6. *Medicina Mechanica, &c.* By J. W. F. Blundell, M.D.—*London*, 1852. 12mo, pp. 292.

Scarcely have the claims of legitimate medicine been vindicated against the attacks of Homœopathy, Hydropathy, and the other fashionable follies and quackeries of the day; hardly have our allopathic ranks, to use the enemy's term, rested for a moment from the combat, when from the distant north come portents of a gathering storm, to task again our energies in the defence of the true science of physic. Old England is threatened once more with a Scandinavian invasion, not indeed this time of savage

Vikings, and uncouth hirsute warriors, but of trained Gymnasts and Athletæ, burning to take the field, and to carry by storm, or else under cover and pretence of science, those strongholds of our art, erected by the combined labours of the medical world during preceding ages. To judge from the proclamations of their leaders, nothing less will satisfy the invaders than the utter discomfiture of the drug trade, as connected with the healing art; the Pharmacopœia shall be scattered to the winds, Apothecaries' Hall shall be closed, and the doctors of the next generation shall forget the Materia Medica, and shall become accomplished athletes, masters of the science of defence, and superintendants of gymnastic exercises.

The discomfited and retreating forces of Hahnemann and Priessnitz, foiled in their attempts to obtain a recognised footing in our ranks, have been, it would appear, received with open arms by the advancing gymnasts; the rebel factions have united together, and strengthened thus, they will again press forward to offer battle, and to invade our colleges and halls, until (they anticipate), wearied with repeated onslaughts, the doctors of this generation shall lay down their arms, and either form an alliance with their conquerors, or ignobly relinquish the field. Such are now our prospects; and such, we venture to say, will they ever be as long as the world shall last. The love of novelty is so deeply implanted in human nature, that an unceasing conflict must be carried on against the aggressions of error; the public ear is easily won by flattering tales of wondrous cures, and still more by the vain prospect of each man becoming his own physician, surgeon, and apothecary; and while they obtain the support of the wealthy and of the ignorant, the charlatans of the day care little for the opposition of honest and well-educated medical men. Still it must be evident to all, that like men who, by some means or other, have obtained a footing in society to which they are not entitled, the quacks, under whatever mask they are concealed, are not perfectly at ease; they seem to know the uncertainty of their tenure, and seek by noise and bold assertions to make up for the deficiencies of their scientific attainments. If a few medical men of rank and standing in the profession, seduced by the apparent modesty of their claims, as at first put forth, have shown some small favour to the professors of the new doctrines, their names are paraded before the public as unblushingly as the long-since deceased Earl of Aldborough is made to figure in the advertisements of Holloway's pills.

The works at the head of our list are, then, a proof that a new movement has begun, whereby a so-called means of curing disease, originating in a distant corner of Europe, is to be extended by its disciples over the whole world; and that England, the paradise, it is said, of knaves, will have her full share of the costs of this experiment, there can be no doubt. It was a bold and clever stroke of policy in the professors of the new doctrine, to associate themselves with the hydropath, the homœopath, and the phreno-mesmerist; they knew well how the vulgar mind had, in many instances, been swayed and cozened by their seeming successes; and in one point all seemed to agree, that allopathic medicine, the legitimate edifice raised by the honest labours of thousands of intelligent men, must now be destroyed, to make way for the pure doctrines they were about to inculcate. But before we examine further into this question, let us say a word upon the various works enumerated at the head of this notice.

We have placed Dr. Sonden's work first, because it is the earliest in date, as well as the most scientific (relatively speaking), and apparently the most honest of all. It has the great merit, too, of being remarkably short, and consequently is less tedious than the others.

Mr. Doherty's pamphlet is likewise eminently brief, both as regards the number of its pages, and the information it conveys; and we should think, the most important announcement in the whole *brochure*, at least in regard to the author himself, is that presented at page 4—" Consultations from 10 till 4, at 54, Great Marlborough-street, Regent-street."

Of the works of more pretension, M. Georgii's essay is decidedly the best, for the simple reason before given, that it is not so long; while for elegance of print, paper, and for the admirable illustrations, we unhesitatingly give the palm to the work of Dr. Roth. It is well that the last-named gentleman has employed so good an artist; it is a comfort to gaze upon these excellent woodcuts of the human figure in all possible contortions, while endeavouring to master the strange Anglo-German language in which the work is written. Dr. Blundell (a Giessen graduate, by examination, as we learn) aspires to a high scientific tone in his 'Medicina Mechanica,' but his language is sometimes obscure, though less incomprehensible than that of Dr. Roth's work. The critical pamphlet by 'D.' is the only book we have before us on the other side, but it is eminently valuable, as showing that the medical profession of Sweden has not received with open arms the gymnastic doctrines of their countryman Ling, as M. Georgii and Dr. Roth would lead us to infer.

Before proceeding to examine the actual merits of the new doctrines; before submitting these to the tests of anatomy and physiology, we shall show to our readers that the accusation of an alliance between homœopathy and kinesitherapy is not without foundation. If the rule hold good in society, that we can best judge of a man by the company he frequents, so, in estimating the truth of the new doctrines, we are bound to inquire by whom they are promulgated, and under what patronage they appear. Dr. Sonden, to his honour, makes no allusion to homœopathy; but in the fifth page of his pamphlet, Mr. Doherty informs us, that " Professor Ling of Stockholm has made kinesipathy as great a means of cure as Priessnitz and Hahnemann have made the water-cure and homœopathy." Still, Mr. Doherty is not anxious to do without medicine altogether.

" Kinesipathy is not," as he tells us in his preface, " a remedy for all the ills that flesh is heir to. Its healing powers are most conspicuous in diseases of a chronic nature. Other modes of treatment are required in fevers, epidemics, and acute diseases. The physician and the kinesipathist may act together in many cases, but their functions are dissimilar. Mechanical agency is quite distinct from chemical agency in the treatment of disease; and kinesipathy is merely an addition to the numerous resources of the healing art."

But the professors of this new science are far from being agreed upon this point. M. Georgii tells us (p. 103, note), " It appears to be a demonstrated fact, that no advantage can be expected from the simultaneous employment of pharmaceutical preparations and the practice of kinesitherapy!" M. Georgii refers to Liebig for the proof, that certain poisons, and metallic substances in general, combine with the albumen of the blood, and thus become, as it were, deposited in various organs, occasioning

a sort of tanning of the tissues; and, he tells us, that arsenic has been repeatedly found in the liver of a dog, several years after the poison had been administered to the animal! M. Georgii is seeking to explain the alleged inefficiency of medicine in those cases where sulphureous preparations have been previously taken by the patient; and thus we are to infer, that sulphur acts upon our tissues, as it does on caoutchouc, rendering it more durable, but greatly altering its nature! If such be the case, our mineral springs at Harrogate should be closed by order of the government, lest the bowels of her Majesty's liege subjects should become permanently obstructed by their use. To avoid so terrible a consequence, M. Georgii proposes, that we should only employ chemical agents in moderate doses, *destined to bring about reactions according to the law of Hahnemann !—a law which is still more recently corroborated by the numerous hydropathic and kinesitherapeutic cures that have been effected.* (p. 105.) Here we have, then, the three grand allies in united force, all brought to bear against legitimate medicine.

Turning to the controversies contained in the pamphlet of 'D.' (No. 3), we find still more convincing proof that the alliance we refer to is not merely contemplated, but has actually taken place. The pamphlet in question is a reprint of some sharp controversial letters that appeared in the public journals of Stockholm, upon the pretensions of the gymnasiasts, and upon their fitness for undertaking the cure of disease, and the instruction of pupils in anatomy, physiology, and the higher branches of our profession. If it be true that the period of instruction for those who propose to practise this new art, does not extend over a period of more than six, twelve, or at the most eighteen months, then we must acknowledge, that athletic exercises not only strengthen the bodies, but must marvellously sharpen the intellects, of the pupils. To judge from the specimen given in the first letter in ' D.'s' pamphlet, the most recondite questions in what the Germans would call transcendental physiology, are entered upon at the examinations at the Central Gymnastic Institute. Whoever the writer 'D' may be, he is no mean antagonist; and though his language is sharp, and characterized by biting satire, yet he never forgets the dignity of the profession, and replies with spirit and power to the attacks of the gymnasiarchs. We shall often have occasion to refer to his letters in the further course of this review; but must first seek in the said pamphlet for the proofs of what we have stated of the unholy alliance with homœopathy. The mantle of Ling, the founder of the gymnastic school, and of the Central Gymnastic Institution of Sweden, has fallen upon Professor Branting and Dr. Liedbeck, who come forward conjointly to reply to the allegations of ' D.'s' first letter. We observe in another letter, that Dr. Liedbeck insinuates that the letters signed ' D.' are the production of Baron Düben, and for convenience' sake we will assume that such is the case. If Baron Düben be really the author, he has no reason to be ashamed of his letters; they are eminently characterized by the desire to elicit the truth, and exhibit that abhorrence of quackery, which can belong only to those who feel the honour and dignity of our profession to be at stake. Dr. Liedbeck is, it seems, a homœopath, more or less; though that doctrine has, as yet, made little progress among the sober-minded natives of Scandinavia. Baron Düben had congratulated his countrymen on their freedom from this delusion, and to this Dr. Liedbeck replies in the following terms:

"Homœopathy, judge it as you will, is a fact, and, like gymnastic medicine, a fact that will influence the whole world. With mighty steps does this system (homœopathy) spread itself over the whole world, even though rash youth or ignorance may condemn its practice among mankind in words such as are used by the author of the letter (No. 1 of D.'s), 'God be praised, we have no homœopaths to refer to in this our land,' (p. 5.) Whether this assertion is a proof of the *animus* of the hippocratic or of the allopathic school, it is not too bold in us to believe, that 'le gros bon sens' of *the public* will, at no distant period, constrain my adversary to speak in a different tone." (p. 18.)

We know not whether Professor Branting is or is not a regularly educated physician; but as his name is appended to the document from which we have quoted the above lines, in conjunction with that of an avowed homœopath, Dr. Liedbeck, we can have no hesitation in affirming that the poison of Hahnemann's doctrines has been introduced into the Central Gymnastic Institute of Stockholm. One of Baron Düben's letters most amply confirms our suspicions.

Malgaigne, in his annihilating critique on Georgii's pamphlet, had already said—

"Que dire de ces merveilles? Il faut une foi robuste pour y croire, mais ce sera une excellente pâture pour les esprits avancés, qui ont dejà adopté les miracles analogues du magnetisme animal et de l'homœopathie."

"Malgaigne," observes Baron Düben, "spoke prophetically of the influence that such doctrines and tales would exert on those who had already adopted animal magnetism and homœopathy. Unless we are greatly mistaken, the Central Gymnastic Institution has already produced its clairvoyants, and it is notorious that it has already, body and soul, allied itself with homœopathy. Observe, for instance, one of the fundamental rules of Mr. Branting's physiologico-pathology. 'The result of the many experiments that have been made upon healthy individuals in regard to the specific operation of movements, has, on the whole, been, that the irritation produced by any movement in any organ, or a similar irritation existing in a diseased part, is relieved by the same movement, which, in the healthy, produces the pathological condition.'"

Let us now prove this rule of Mr. Branting's by one of his own cases.

"A neurosis (neuralgia) in the sensitive nervous ramifications of the tractus costalis of the left side, accompanied with frequent hæmoptysis, miliary tubercles, purulent expectoration, great lassitude, dimness of vision, and excessive depression of spirits, was cured in a month and a half by gymnastic exercises, combined with pressure of the skin (*hudpressioner*), and now, after the lapse of a year, the patient has had no return of the pain, and is besides entirely free from the suspicious thoracic disease." (p. 77.)

We have no original work of Mr. Branting's before us; we can only judge from his joint letter with Dr. Liedbeck in 'D.'s' *brochure;* but if the above is an ordinary sample of his mode of relating cases, we can only say that the fewer cases he reports the better. Ordinary practitioners would perhaps have suspected incipient phthisis in such a case, and they might have imagined, too, that the so-called neuralgia was a slight pleuritic attack, from the presence of tubercles in the lungs; indeed, we are told that miliary tubercles were there existing, though how it was ascertained that they were miliary we cannot understand. If, then, we are to apply the above rule of Mr. Branting's in this case, we shall have to concede that the gymnastic exercises which effected the cure would, in a healthy person, have produced neuralgia of the *sensitive branches* of the intercostal nerve, and not this

alone, but would have occasioned miliary tubercles in the lungs with the train of symptoms before stated.

But we have now, we trust, produced sufficient evidence to convict Messrs. Branting and Liedbeck of being actually allied with the most monstrous quackery of modern times; and this were of itself enough to cause us to turn in disgust from the few works they have ventured to publish in favour of their doctrines, were it not that approbation and favour are claimed for gymnastic medicine in other quarters, to which we have hitherto been accustomed to look up with respect and reverence. We are told by Dr. Roth, that—

"Drs. Retzius, Sundeval, Huss, Liljevalck, Sonden, &c., are also in favour of this system in Stockholm. The Swedish Royal Medical Association at Stockholm has made a public acknowledgment of the high importance of the treatment by movements. The following is an extract from its report, dated 22nd May, 1849:

"'Many members of the Association, from the knowledge that the medical treatment by movements, according to Ling's system, has proved very effective as a curative means, *and has produced extraordinary and most satisfactory results in many chronic diseases*, are convinced that this method, developed with the scientific and practical clearness which is required for the adoption of any new medical system, and practised in harmony with other medical sciences, under the special direction of, or in conjunction with, the physician, will take a high standing in medicine.'" (p. 16.)

We have not, unfortunately, the original document; and from what we have discovered in some other of the works before us, we decline to corroborate the entire accuracy of this translation by Dr. Roth. Granting, however, that it is perfectly correct, it would appear that the Royal Swedish Medical Society is by no means satisfied with the position assumed by the Kinesipaths; but that while they, or at least some of them, admit that extraordinary results have been obtained, they point to the imperative necessity of the whole Gymnastic Institute being subject to scientific medical control. To this it might, perhaps, be thought that no possible objection could be made; but if the heads and directors of this institution have, as we have already shown, allied themselves with homœopathy, it is not likely that they will ever submit to the control of an allopathic board of inspection. But it would seem that, after all, the Royal Medical Society of Stockholm has not committed itself in the way that M. Georgii and his followers would lead us to believe. At page 66 of M. Georgii's 'Kinesitherapy,' we meet with the following assertion :—"The Royal Society of Physicians of Stockholm has at length (*vient enfin*), in the course of last year (1846), requested Professor Branting to publish the results of his experience, *for the sake of science and humanity.*" (p. 66.) The above lines would lead us to infer that the Society, which had hitherto held back its approval or disapproval of this mode of cure, was now at length so convinced of its claims, and so satisfied of the truth of its doctrines, that it earnestly besought Professor Branting (*dans l'intérêt de la science et de l'humanité*) to give to the world the fruits of his experience, and thus to confer a lasting benefit on mankind. The impression thus sought to be conveyed is far, very far, from the truth. The Society, in fact, requested Professor Branting to give a plain, yet sufficiently ample exposition of his system, in order that they might judge of its merits. We give the words as translated from the original report, at page 31 of Baron Düben's pamphlet :

"The Society must regret the reluctance hitherto shown, to afford a scientific report of the results of the gymnastic treatment of disease at the Central Gymnastic Institute; and which reluctance the Society failed to overcome, even by their direct request, addressed, on the 17th of Nov., 1844, to the director of the Institute, Professor Branting; to the effect, that he would lay before the Society the results and advantages obtained by such treatment, so that thus every doubt might be most securely cleared up in regard to a mode of treatment *highly praised by some, and greatly mistrusted by others!* It is on this account that it has been difficult for the Society, as a corporate body, to give any definite and more detailed report." (p. 32.)

This was in the year 1845, and we are now in 1852; yet up to this day no report has been issued by Professor Branting, no scientific exposition of his mode of treatment has been published; there has been, on his part, an ominous silence.

That the governments of St. Petersburg and of Berlin, have shown some favour towards the Kinesipaths is not to be wondered at, when we recollect that this mode of cure is as fashionable among the aristocracy of Sweden, as the infinitesimal doses of Hahnemann are among our dukes and barons of this realm. It must not, however, be forgotten, that the Russian and Prussian governments have both placed their gymnastic institutes under the superintendence of a responsible commission of duly educated medical men.

We have now, we think, laid sufficient proof before our readers, that the mode in which this new system of cure is introduced to public notice, is anything but accordant with that pursued by really scientific discoverers. We have shown, too, that in their published works they have exhibited an avowed tendency to the utterly fallacious doctrines of homœopathy; and to judge from some passages in Dr. Roth's and in M. Georgii's books, the words of Malgaigne have already been fulfilled to the letter, " that the system of kinesipathy would find favour among the mesmerists as well as with the homœopaths." Thus Dr. Roth recommends that in lethargy, as also in fainting (pp. 194—217), " *awakening passes* on the forehead should be employed;" and that phreno-magnetism is not repudiated, is evident from the cure, said to have been effected by Professor Branting, of a case of obstinate insomnia, by circular percussions (*cirkel - hackningar*) over the site (according to phrenologists) of the organ of form, after the organs of ideality had been for a long time percussed without effect. We may aver this to have been a case of phreno-magnetism; but would it not have been better to have tried first the wonderful efficacy reputed to exist in the extremities of the operator's fingers, when merely pointed at the affected organ? It would thus seem as though there is not any monstrous quackery in the world, to which the kinesipaths are not inclined to extend the hand of friendship; all irregulars are invited, and are readily received into their ranks, if they will but shoulder a pike or break a lance against the serried lines of the legitimate practitioners. We have now satisfied ourselves of the nature and extent of their alliances; we have endeavoured to expose the want of good faith that characterizes most of their writings; and we shall next turn to the examination of their doctrines as developed by Ling and his immediate successors. The personal history of this founder of a new sect of practitioners is curious enough, nay, even the year of his birth seems to be a matter of dispute; for while Dr. Roth

tells us that Ling was born in 1766, M. Georgii informs his readers that the great man first saw the light eleven years later (in 1777), and as he died, in spite of his gymnastic treatment of himself, at the early age of 62 (May 3rd, 1839), we conclude that M. Georgii's dates are the more correct. From all that we can gather of his *life*, Ling seems to have been a man of great energy, a poet, a fencing-master, an intense admirer of the old Scandinavian vikings and heroes, and last, and not least, an irregular medical practitioner in connexion with his new system of gymnastic medicine. Of his poems we confess not to have read a single line, and if Howitt's character of his muse be correct, we fervently hope we never shall. "Lenström (says Howitt) avers that Ling's great epic poem, 'The Asar,' is the most long-winded in the Swedish language."

"I have called Ling," says Lenström, "considered as a poet, an apparition from the old world of heathenism. The whole man looked just something of that kind, as he was to be seen in his great gymnastic hall in Stockholm, clad in a strange, hairy, and rugged costume of wolfskin, cut according to his own peculiar fancy, and in which his meagre form presented itself in a style most strikingly original. Ling was in fact an original in everything. Together with poetry, he had, from his earliest years, with the utmost ardour embraced all such knightly usages as stood in connexion with gymnastics, for the universal use of which he enthusiastically contended, as the only means of restoring, in the North, a more vigorous race, a race like those old Berserker who were so dear to him. He thus raised gymnastics into a regular science, (?) based upon anatomical and physiological principles, and created an entirely new department of them. These were the so-called medical gymnastics, which have proved themselves by no means a contemptible branch of the general science of maintaining or restoring health."—*Howitt's Literature and Romance of Northern Europe*, vol. ii. p. 401.

We can only demur to one expression in this curious sketch of the gymnasiarch, where Ling is said to have based his new science on anatomical and physiological principles. Whatever his successors may have attempted, Ling himself was certainly unfitted by his attainments for such a task. Even had he been a good anatomist and physiologist,—which we can confidently assert that he was not,—his fantastic mind, ever seeking after the strange and the wondrous in nature, ever labouring in the same spirit that characterized his poetry, was ill adapted to the long and careful study requisite to form a good physician. Of his mode of diagnosis we need give but one example, which we take from the portion of Dr. Roth's book allotted to diseases of the heart. It is true that we have made immense progress in the diagnosis of cardiac disease since Ling wrote the following lines; but we doubt if, even in the darkest period of medical science, any rules more fantastic and groundless were ever laid down, than those we here present, as inculcated by the great master of gymnastic medicine.

"In order to know the nature of these diseases (of the heart), Ling proposed (twenty-two years ago, when auscultation and percussion were not so well known) the following diagnostic movements. The patient leans towards a free standing prop, his left hand is raised up, and his chest also is kept up by an assistant, in an obliquely forward standing position; the physician presses cautiously one hand on the last rib, and with the edge of the other either between the ribs of the chest, or between the sixth and seventh vertebræ of the back, while the assistants slowly increase this pressure, as well as the bending forward of the patient on one side. If the patient feels a strong pain in the heart, this is a symptom of aneurism; if he feels more external heat, it is a rheumatic fit; if he feels a numbness, and as if

the heart would cease to beat, it is atrophy; if he feels a yearning, it is flatulency; if there is a great moveableness, it is a spasm of the heart; if the number of the pulsations is increased, then the volume of the heart is irregular." (p. 217.)

It is quite unnecessary for us to pass any remark on the crude absurdities congregated in the above lines; we only regret that a few more of such rules are not presented to us, whereby we may judge, not merely of the merits of this so-called science, but of the capabilities of its inventor. With regard to the claims of Dr. Roth and others, that kinesipathy has been patronized in high quarters, and that it has received the favourable notice of the Swedish government, we readily acknowledge the truth of the assertion. Homœopathy finds at the present moment great favour among the nobles of our land, yet the medical profession as a body shrinks from and repudiates its manifold absurdities. But Ling did not come forward boldly at first, and declare open war against all received medical doctrines, as did the notorious Hahnemann. For a long period he devoted himself to the teaching of military gymnastics; and so long as he endeavoured to elevate these into a system, he received not only the patronage of the great, but was favoured and aided by Retzius, and by many of the leading members of our profession in Sweden. The sunshine of royal and courtly favour was too strong for his excitable brain; and, carried away by his ardent temperament, he left the broad path, wherein he might have continued with distinction and honour; and striking off into by-ways and tortuous paths, he had soon wandered so far into the realms of quackery, that his former patrons—at least, such of them as by their scientific education and their knowledge of our art were qualified to form a judgment on the matter—hesitated and declined to follow him. So long as he exercised the art of gymnastics, without intruding upon the domain of legitimate medicine, the merits of his system of military gymnastics were fully acknowledged and appreciated; but when once he plunged into irregular practice, and promulgated his rules for the cure of innumerable diseases by movements alone, the profession shrank from the alliance with one, who, though supported by royalty itself, was no more than an ordinary charlatan. It is possible—nay it is indeed probable—that Ling, in adopting this course, was perfectly sincere; yet we find, that though his pen flowed rapidly as a poet, he published little in regard to his new science of gymnastic medicine; perhaps because, aware of the insecurity of its basis, he hesitated to lay bare its foundations to the critical eyes of science. We have found it no easy task to discover the true order and character of the theory of kinesipathy, obscured as it is by the incomprehensible language of Dr. Roth, or by the would-be scientific exposition of Dr. Blundell; and we have accordingly had recourse to the more temperate pamphlet of Dr. Sonden, for a brief exposition of Ling's views.

Dr. Sonden adopts Ling's own phraseology in describing the gymnasiarch's theories of his new science.

"Life is to be considered as a whole (*enhet*) composed of three constituent parts, *chemical, mechanical,* and *dynamic* life, or in other words *vegetative, animal,* and *spiritual* life. The union of these three elements indicates a perfectly organized being, and all three elements take part in every action performed by such a being; but the varying relations which these three fundamental forms bear to each other, create the differences between them. A completely organized being

can only, however, have an ideal existence ; all health must be merely relative, if we admit the inevitable idea of death and dissolution.   A complete education must therefore not merely comprise the cultivation of the understanding (psychical part), but it must also be directed to the organization as a whole ; it must provide for the nourishment of the child, for bathing, for exercise and hardening the constitution by exposure to the air (vegetative part), that it may not be softened by effeminate habits ; nor should the animal part be forgotten, as regards the rest, the exercise, and the postures of the body.   For the same reason must not only those medicines and that diet be chosen for a sick man, which are suitable to his condition (vegetative), but we must regulate his movements, his position when resting, and the exercise of his mental faculties.   Without such care the treatment of the sick is necessarily incomplete.   Health depends upon the relative balance of the three functions or parts of the organization before mentioned, sickness appears when the balance is overthrown.   To cure sickness, to restore the balance, we must seek to increase the power of the agent or part whose operation is deficient.   It is an error, therefore, to attend in preference to one or other of the fundamental forms while health exists, and it would be equally a mistake to neglect in sickness that form or function which calls for active aid.   It would be wrong, for example, to endeavour to cure a fever by gymnastic operations (Herodicus thought otherwise), for the animal function is in excessive action already, as may be learned from the accelerated circulation and the rapidity of the breathing.   Here chemical treatment would be of use.

" No operation or function can take place in our organization, except by the reciprocal action of the three great fundamental forms of life.   If *animal* life predominates, disease takes on the *vegetative* form ; if *vegetative* life be in excess, disease assumes the *psychical* type ; and should the last named predominate, the disorder will partake of the *animal* character !   Thus fear, joy, or anger, either produce excess of muscular power, or entirely take it away.   Exercise produces a feeling of health, assists digestion, and induces sleep.   The chemical (!) operation of spirituous liquors produces either great depression or great excitement." (p. 15.)

Dr. Sonden goes on to observe, that the great characteristic of Ling's theory is, that he ever looks on the mind and body as constituting one harmonious whole.   In our innocent ignorance we had ourselves come, with all mankind, to the same conclusion ; but this, too, is claimed as a discovery of Ling's.   Through the fantastic language of the gymnasiarch, the reader will no doubt have discovered that a certain amount of truth is here served up before him, garnished with numerous fancies and absurdities ; but yet an indulgent critic might allow, that up to this point Ling's theories might possibly be permitted for a time to stand, under the supposition that he only wishes to illustrate the dependence of one part of our organization on the rest.   But when he comes to the application of these doctrines to practice, in union with his theory of gymnastic medicine, we must confess, with Behrends,* that he carries us far beyond our depth.

" Many will ask," continues Dr. Sonden, " how it is that *external* treatment can affect the *internal organs and parts of the human body?*   The answer lies in the definition of this last word, ' body.'   Our organization, regarded as a whole, must stand in the most complete unity of relation with itself ; and as a chemical agent, in consequence of this unity, can pass from the interior to the exterior of our body, so a mechanical influence can be transmitted from without to the internal organs." (p. 16.)

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* " Die Schwedische Gymnastik hat sich in einer Masse phantastischen Hypothesen gehult, welche den von neuheit befangenen, und besonders den Unerfahrenen blenden."   " The Swedish science of gymnastics has wrapped itself in a mass of fantastic hypotheses, which tend to deceive those fond of novelty, and particularly the inexperienced."

A chemical agent of any kind, we had always believed, when taken into the stomach, was conveyed to all parts of the body by the blood; and we are now told, that a mechanical impression from without must necessarily pursue the same course in a converse direction? We had been taught that one of the great objects of our beautiful and complex organization was to resist shocks from without upon our frame; that the elasticity of the spinal column tended to diminish the shock to the brain when we alighted from a height upon our feet; but now we must believe that the very lifting of a finger is replete with the gravest consequences. Does such an act, then, produce alterations throughout the whole of our system? Is the human body like to a vessel or bag filled with fluid, in which a vibration communicated from without extends throughout the whole mass? Whatever Ling's published theories in this respect may be, it is certain that in his practice he acted a very different part, by the variety and complicated nature of the movements which he prescribes for the cure of disease. The bare enumeration of these movements occupies a whole page in Dr. Roth's index to his book; and some, such as the vibrations of the perineum, prescribed in gonorrhœa, are of so singular a character, that even were we disciples of the gymnasiarch, we should hesitate about the propriety of employing them. It would be a waste of time, of print, paper, and space, which might be better employed, to follow Drs. Blundell and Roth, and M. Georgii, through their whole exposition of Ling's theories; and indeed, it is at all times difficult to determine how far certain theories are to be ascribed to the gymnasiarch himself, or have emanated from the fertile brains of his disciples and successors. Dr. Sonden honestly declares, that

" If we seek for a theory to explain all the operations performed under the science (of gymnastic medicine), it must be confessed that Ling's doctrines do not deserve the name. They embrace merely the most general principles, and do not descend to particulars. They concern merely the commonest principles of life and their mutual relations, without attempting more recondite explanations of these, or of the operations of gymnastic medicine. They assert the claims of gymnastic medicine to take its place as a system of education and of therapeutics, *but they do not explain physiologically the nature of gymnastics.* In my ideas, a true theory of gymnastics would be a complete body of ascertained laws, respecting the operation of movements; we should have rules laid down to explain and exhibit the laws of active muscular movements and their consequences; laws, too, for the action of these movements upon other parts of our system, on the nerves, the bloodvessels, and the absorbents; laws concerning the operation of passive movements or manipulations on the various tissues and organs, as, for example, to illustrate and declare the effect of pressure, or of vibration, applied to a nerve or to a bloodvessel. Of all this, we find in Ling's works merely practical hints, but there is no approach to a developed and completed theory. Most assuredly is it requisite, that those who devote themselves to the practice of gymnastics should have a more thorough acquaintance with medical science, and particularly with physiology and the diagnosis of disease, than can be obtained from the general doctrines promulgated by the kinesipathists." (p. 18.)

We have already said that Dr. Sonden's pamphlet is by far the most moderate in its pretensions of all the works that we have seen on this subject; he honestly admits the want of a true scientific foundation for the system; and yet the successors of Ling boldly claim for this fantastic theorist the honours that are accorded to a Berzelius or a Linnæus. We

had expressed a doubt, in a former page, as to the exact length of the course pursued in the Central Gymnastic Institution in Stockholm, but we have since ascertained, that in a *six-months'* course the gymnasiast is made perfect, and is ready to be sent forth to the world as a practitioner of this new art of healing. Our readers may, perhaps, scarcely credit this assertion, but besides having obtained positive evidence thereof from those well acquainted with the proceedings of this Institute, we can refer to M. Georgii's own pamphlet for a corroboration of this statement.

" The Central Institute of Gymnastics at Stockholm, founded by the Swedish government, occupies a vast space, divided into large halls, some specially destined for gymnastic exercises and fencing, others devoted to the amphitheatre of anatomy, to the anatomical museum, to the library, and to divers classes. The object of this establishment is to form *annually*, to the number of fifteen or sixteen, masters of gymnastics for all the colleges, for the primary and secondary schools, and lastly for the army. The subjects of the courses are, descriptive anatomy with dissection, anatomy in its relation to the movements of the human body, physiology, the principles and theory of gymnastics, the theory of self-defence, gymnastics with and without apparatus, medical gymnastics, the bayonet, the sword, and the sabre exercise," &c. &c. (p. 8.)

Two questions occur to us, and will no doubt suggest themselves to our readers on perusing this long list of accomplishments and attainments, to be completely mastered in a six-months' course. First, we must believe the rising school of kinesipathists to be, as Baron Düben remarks, most universal and extraordinary geniuses, to be able thus to become perfect in anatomy and physiology during a winter session, while they are at the same time devoting no small part of each day to manual exercises with the sabre and the bayonet. We hear nothing of chemistry; but perhaps that too, both practical and theoretical, is included within this wondrously active half year. Again: are we to understand that without clinical instruction, without the means of observing disease at the bedside, these men are sent forth into the world to diagnose and to treat disorders of all kinds whatsoever? If such be the case, we cannot wonder that their reported cases of disease are so lamentably deficient in accuracy of detail, that their descriptions of the various maladies they have heroically subdued, are such as to render it a matter of great doubt, in many instances, what were the disorders with which they had to engage. Nay, it would seem that even in reporting cases which admitted of but little doubt, the usual want of honesty that characterizes the quack, exhibits itself in their published works. At page 69 of his ' Kinesitherapie,' we observe the following statement by M. Georgii:

" Dr. Liljevalck, in his report of the treatment of venereal disorders in the garrison hospital at Stockholm, says, ' In twenty-three cases of urethritis, *many of which were complicated with stricture,* percussions applied from above downwards upon the os sacrum were employed with success,' " &c.

The pamphlet of Baron Düben supplies us with Dr. Liljevalck's *real* report, as follows:

" This gymnastic mode of cure was only employed in simple gonorrhœas, and the milder complications of that malady. If *stricture*, prostatic inflammation, or epididymitis was present, I had recourse to the ancient and usual method of cure (har jeg ätergätt till gammal slentrian)."

It has been justly complained, by those who have preferred legitimate medicine to kinesipathy, that as yet no attempt has been made to give a scientific exposition of the system. Such has certainly been the case in Sweden, where Professor Branting has evidently shrunk from this test, proposed to him eight years ago by the Royal Medical Society of Stockholm. Drs. Roth and Blundell have, however, been more bold in England; but after perusing their respective volumes, we are satisfied that the Swedish gymnasiarchs have chosen the wiser and the better part, of almost total silence on this matter. We can only judge of kinesipathy at its fountain-head in Sweden, by Branting's annual reports and orations; while in the cases that have been published to illustrate his doctrines there is so little detail, so little by which we can judge of the accuracy of the diagnosis, that they must be received with extreme caution. In a truly scientific work on any subject, it is at all events necessary, that the cases of reputed cures effected by the agency of a specific mode of treatment should be especially complete in the detail of their symptoms, so that no possible question could arise as to the real character of the disorder. We are constrained to say, that this essential rule has been most grievously neglected by almost all the writers whose works are now before us: we must confess that the enumeration of their cures reminds us more of Holloway or of Morison, than of the sober and prudential reserve that should characterize a scientific report. In many instances the mere name of a disorder is given, with hardly any of the symptoms we should look for to authenticate the diagnosis. Here, for instance, is one of M. Georgii's cases, copied from his master's, Professor Branting's, report :

" We would refer here to the case of a patient who, to all appearances, suffered from tubercles in one of the hemispheres of the brain, an affection which, for the space of several years, had given rise to violent and periodic headaches. He was at first treated by derivative movements, and subsequently by circular percussions applied with the hand over the superior part of the cranium. This last-named application arrested the headaches; and the excessive heat, the painful and continual pulsations, the convulsions and vertigo, also disappeared." (p. 90.)

" We hardly know," says Baron Düben, " whether the last sentence belongs really to the report or no, for at first it is said that the headaches were periodic, but, finally, we are told that the pulsations were painful and continuous." Our readers will at once perceive the utter insufficiency, in a scientific point of view, of such a report as the above. How was the diagnosis of tubercles existing in the brain at all, made out on this occasion ? Does M. Georgii mean to infer that the tubercles, if they did exist, were dissipated by the circular percussion on the exterior of the patient's cranium ? How did he ascertain that the disease existed in the hemispheres, unless by the marvellous powers of animal magnetism ? Were the tubercles absorbed ? or did they dry up and shrivel before such powerful manipulations ?—Such is a fair and average sample of the cases reported by the heads of the gymnastic school; and their disciples in this country, Drs. Roth and Blundell, have not been more happy in their descriptions of disease. We turn to page 178 of Dr. Roth's curious volume, where the author begins to explain " the treatment of single diseases." The first class of disorders are enumerated as " congestions of the head, headaches, giddiness, *humming* in the ears," &c. For these are prescribed—1. De-

rivative movements of the legs and feet; 2. Passive rotation of the feet, with active passive extension and flexion of the feet—and below is one of the beautiful woodcuts that ornament the volume, where a stout gymnasiast is depicted rotating his patient's toes and foot; and we are gravely told, that the operator, after fixing his patient's leg across his knees, then "moves his left hand, which presses a little on the point of the foot, in a circle from right to left from *three to six times*, and then *the same from left to right*, more or less quickly, according to the prescription. This rotatory movement is repeated two or three times." (p. 180.)

Such of our readers as remember Hahnemann's famous directions for the divers shakings of the elbow and elevations of the arm in the preparation of his wonder-working infinitesimal doses, will not fail to perceive some analogy here. But, after all, kinesipathic treatment, however ludicrous it may seem to the reader and to the man of science, is, like hydropathy, no joke for the unfortunate patient. Dr. Neumann employed the following measures in a case of opaque cornea—1. Pressure on the supra-orbital region twenty or thirty times daily; 2. Passive rotation of the head eight or ten times daily; 3. Frictions in the direction of the superior longitudinal sinus and transverse (*sic*) and the beginning of the internal jugular vein; 4. Flexion of the trunk with *resistance;* 5. Active rotation of the lower extremities; 6. Percussion on the soles of the feet with a cylindrical piece of wood. These were continued for the space of fourteen weeks; but the narrator honestly adds, that he does not know whether the further effects of the treatment entirely restored the eye.

In "chronic inflammations of the larynx and windpipe, and irregular activity of the vocal cords," a tolerably strong vibration, with moderate pressure, is employed on both sides of the larynx and windpipe. In addition to this we find that there is recommended—1. Submaxillary vibration; 2. Double oblique lateral friction of the throat; 3. Point vibration on the windpipe; 4. Double frictions from the throat down the shoulders and arms; 5. Longitudinal friction of the larynx with three separated fingers, and loud speaking, reading, singing, and declamation; though we cannot seriously suppose that these are employed in cases of laryngitis. Passing on to tubercular phthisis, we are gravely told that Ling "advised movements as soon as the lungs *show* traces of incipient ulceration, and difficulty of breathing, cough, congestion of the chest, inflammation, and formation of pus, become manifest." We should have thought that kinesipathy would have seized on the disorder in its earliest stage, and by some wonderful gymnastic combination of movements, have prevented the formation of tubercles at all. But perhaps we are in error; for on the same page we are told that Ling himself, and Professor Branting, were both cured of this disease by movements; and we conclude that what is here meant is, that Ling and Branting were both in this way freed from the predisposition to phthisis. It could not be possible, in the narrow limits of these observations, to analyze carefully all the singular modes of cure by movements, projected and practised by Ling and his disciples. Of Branting's method of reporting cases we have already given a specimen; and let us now see how Dr. Roth acquits himself in this respect. The following is given as a case of phthisis and its treatment; and if the disease itself was not severe, the same cannot assuredly be said of the mode of cure that was adopted:

"The patient was twenty-six years of age, and was principally employed at needlework; she was extremely nervous, irritable, and very thin; feet and hands generally cold; continual tickling in the throat, cough, sometimes spitting of blood in small quantities; the expectoration moderate, slimy, greenish, and like pus; the upper part of both lungs, during the percussion, yielded a dull sound; short breathing, a little stooping, in consequence of weakness of the muscles of the back, the shoulders projecting forward; the digestion deranged, sometimes no appetite, and at other times vomiting, frequent diarrhœa, menstruations regular as to time, but little, and only during one day; leucorrhœa; the skin dry and flabby; the countenance livid, blue circles around the eyes, which were sunk in, the sclerotic coloured like mother of pearl, the lips blue, the pulse almost insensible, great weakness, and continued fear of death. We have no exact copy of the prescription, but as far as we remember the movements were:

"1st. Transverse frictions of the loins from the spine downwards, while the patient was in the supported sitting position.

"2nd. Active-passive extension of the leg, previously placed and kept by the assistant in the same position.

"3rd. Active turning of the trunk in the upright standing position, with the assistance of the operator.

"4th. Active-passive extension of the trunk in the high, long, sitting position, till the trunk was almost in a horizontal line with the legs, and passive-active flexion from this position in the sitting one.

"5th. Active-passive pressing down of the extended leg in the reclined supported sitting position.

"6th. Lateral vibration of the larynx and windpipe, with the double parallel frictions of the throat, and friction down both the shoulders.

"7th. Vibration on the lowest point of the windpipe, and longitudinal frictions with separated fingers on the larynx, downwards.

"8th. Pumpings."

The "pumping" (*sic*) is an entirely passive movement, the commencing position of which is the following:

"The patient is in a supported sitting position, with his arms hanging down quite passively; the operator stands behind the chair, puts both his hands on the sides of the patient's chest, strokes them gently to the armpits, which he holds, and raises the shoulders of the patient, which he keeps in this position for half a minute or longer; afterwards he lets them gently down, and begins again the same movement, which is repeated from three to six times. This movement produces, in the generality of patients, a very agreeable feeling, caused by the artificial passive imitation of the respiratory movements." (p. 210.)

"9th. Passive flying. (!!) The patient is in a standing position, and leans with the head on the chest of the operator, who stands on an elevated level behind the patient; the operator seizes the entirely passive arms on their lowest parts, or on the hands, and makes quick or slow flying movements—three, six, or nine times—one after the other. After an interval this movement is repeated.

"10th. Passive rotation of the feet, and active-passive extension and flexion of the feet.

"11th. Passive rotation and active-passive extension and flexion of the hands.

"12th. Active-passive extension of the leg in the half-standing position, with hips held, resting with the thigh on a transversal bar.

"13th. The active-passive raising of the trunk from the stooping-standing position to the upright one.

"14th. Circular frictions, alternating with vibrating movements on the relaxed abdomen of the recumbent patient, for five minutes.

"15th. Active-passive flexion and extension of the arms, in the sitting position.

"16th. Longitudinal frictions of both sides of the chest, from below upwards to the elbows, in the supported sitting position.

" 17th. Active flexion of one leg, in a half-standing position, while the other rests backwards on the point of the foot, on an elevated level.

" These different movements were not employed at the same time, and not in the above-mentioned order." (p. 213.)

Our readers will share in our wonder at such treatment adopted in a patient, with almost insensible pulse, and extremely debilitated. Marvellous, indeed, must have been the inherent vitality of her sinking frame, to withstand the various active-passive movements enumerated above.* Continuing the case, we are further informed, that

" Two months have passed since this treatment was commenced; the chest of the patient has developed more than half an. inch in the periphery of the thorax; the breathing is less short; the countenance is less livid; the blue circles around the eyes have disappeared; the eyes are less deep; the lips red; the cough not much changed; the expectoration not so much; spitting of blood seldom, and less, and of the same nature; no diarrhœa," &c. &c. (p. 214.)

Throughout the whole report there is not the slightest allusion to the actual condition of the lungs, as ascertainable by the stethoscope; no microscopic observations are recorded of the sputa, no observations of the conditions of the gums, and finally, there is not a word about the state of the pulse. In the next page we find the following rich *morçeau*, on " Acute Œdema of the Lungs, and Incapacity of Expectorating;" quoted, without any observation, from Richter:

" I do not know a better treatment than to put the patient (who begins to be benumbed, and who is unable to expectorate) in an upright position, to keep him on the arms [*sic*], to shake his shoulders, to knock on his back, to excite him by repeated screaming [query, of the operator or of the patient?] to expectorate, and to remove with the fingers the slime from the posterior part of his mouth. Many persons on the point of suffocation are saved from this danger; and I am astonished to see many medical men allow patients so circumstanced to lie quietly and continue their rattling till they die." (p. 215.)

It is evidently not the policy or the practice of the gymnasiasts to allow their patients an interval of rest, and we should be curious to know what are the symptoms of acute œdema of the lungs, and what the rationale of such treatment.

In the same page we have a paragraph on " Adherences of the Lungs," and they are to be treated—it is not said how they are diagnosed, or if fresh or ancient—" by friction in a straight line on both sides of the chest, alternating with a vibration round the thorax, while the patient keeps his arms raised and fixed on an object during this passive movement." (p. 215.)

Dr. Roth is neither luminous nor long-winded in his observations on diseases of the heart and their treatment by movements, while Dr. Blundell favours us with six cases, where hypertrophy of the heart was relieved; and even where disease of the mitral valves (p. 260) disappeared under the all-powerful influence of kinesipathy. Dr. Roth gives us the rationale of the " chopping-and-knocking treatment" in such cases, copied from the writings of Dr. Neumann, of Graudenz.

" Dr. N.," says he, " finds an analogy between the excrescences of the endocardium (which he considers as the products of a previously inflammatory state), and the

* Active-passive movements are explained to be those where the patient offers more or less resistance to the manipulations of the operator.

fibrous or tendinous texture of cicatrices from ulcers, which had formerly been of very long standing [*sic*]. As indirect pressure can [cannot?] be used for increasing the absorption of these excrescences, the movement is substituted by vibrations, point-shakings, choppings, percussion, &c." (p. 218.)

After all, then, the heart, the important organ so wonderfully guarded in our system from external injuries, will not escape "the choppings and knockings" of the kinesipathic operation; but we should as soon expect that the excrescences would be knocked off by a few vigorous well-directed blows, as that they would be absorbed, by tapping the outside of the thorax. There may be indeed a hidden virtue in these tappings, which has not been revealed to Avenbrugger and to Piorry.

"Chronic catarrh of the stomach and bowels, flatulence, and obstruction, are treated by movements increasing the activity of the abdominal muscles, the contraction of which produces a squeezing of the liver and gall-bladder, by which the excretion of blood and gall is promoted as in a sponge!" (Richter.)

Our author is eloquent on piles; see the following:

"*Suppressed piles* causing [?] mental diseases. After a sudden spontaneous or artificial stoppage of discharge of blood or slime, persons suffering from piles often become *lunatics;* these must be treated immediately, without any loss of time, by movements acting very violently on the brain, which must all be done in a horizontal position, and the patient must be overturned [!!], and as quickly as possible kept up and down by the assistants.
"Many fear this overturning, because they wrongly believe that the blood acts only by its weight, and that it comes in too great a quantity to the brain; if the first were the case, the blood would never flow to the head in an upright position; with respect to the too great quantity, the blood is pressed on to the brain only, if the patient being nervous retains his breath, and actively resists while his trunk is moved up and down. If the patient is entirely passive while the overturning is made by the assistants, the head becomes cool," &c. (p. 227.)

Whether a lunatic patient, or even a sane one, would be likely to remain passive under such violent manipulations, we leave to our readers to decide. We observe at page 235, that a similar mode of treatment is spoken of for the cure of epilepsy; and at page 257, when speaking of mental disease, a system resembling the ancient mode of treatment by the turning-chair is seriously advised.

"It is to be observed, that mental disease has been successfully treated by mechanical influences; as, for instance, by the turning-table, on which the patient, with his feet towards the centre of motion, is fastened in a supported sitting or lying position, and then, by a mechanical apparatus, turned round in a slow, quick, equal, or decreasing manner. The curative effect of this manipulation is manifested by the change in the respiration or pulsation, by the production of giddiness, and an uncommon excitement of the *cœnestesis*. This has been useful generally in melancholy and obstinate patients; also to those suffering from fits of insanity, and in mania with a suicidal tendency. Quiet and indolent patients have been roused and excited by this manipulation, and in some nervous diseases, especially in some cases of epilepsy, it has been successful." (p. 257.)

Our readers will no doubt by this time be disposed to cry out, "Hold, enough!" But we could multiply quotations of the same character as the above, to an almost indefinite degree.

Before dismissing the subject altogether, it will be asked of us, if we believe that any great discovery has been made by the Swedish gym-

nasiasts? if, apart from the absurdities and charlatanism which abound in the writings of Ling and of his followers, we have found therein some grains of truth, some golden particles, which, sifted from the rubbish that conceals them, reveal the existence beneath of a mine of scientific research, and which may lead to a revolution in our present treatment of disease, or may at least modify its details? Is this new system one, like hydropathy, that contains elements of good—one that, under due medical superintendence, may really aid us in the alleviation of the maladies that afflict our frame? or is it, like its ally homœopathy, essentially negative, nugatory, and false?

All who have been engaged in the active practice of their profession, will have met with individuals labouring under the countless forms of dyspepsia and its consequences, who, when exercise was strongly urged upon them, as a means of recruiting their energies, have replied, that they were incapable of the least active exertion, that they had not strength to run, to ride, to swim, or to undertake a pedestrian excursion in search of health. These are men who have spent their lives in study, or in the absorbing pursuit of wealth, sitting all day in close counting-houses, or ensconced in easy chairs before a library-fire, who have taken little or no care about the purification of the skin by repeated ablutions of the whole body, and while indulging in the pleasures of the table, have neglected almost entirely that active muscular exertion which is requisite for keeping open the pores of the skin, and preserving the activity of the secreting and excreting processes of the body. Or they are females, who have worn out their strength in crowded assemblies, and in the exhausting chase of fashion, to whom muscular exertion has become a myth fit only for dairy-maids and stout country wenches, but not to be thought of for the pale votary of Almack's. There is, too, the poor pining milliner's apprentice, working twelve, fourteen, and sixteen hours out of the twenty-four, in close ill-ventilated chambers, to whom the fresh air of the country is only pictured as a dream of her childhood, long since vanished, and never to return. In many of the above subjects, debility has really advanced so far, that active movements are at first out of the question, and for such, passive movements, as in the operation of shampooing, may be substituted with unquestionable benefit, when combined with attention to the secretions of the skin, and to the disordered state of the internal organs.

As an adjunct to medical practice, a well-ordered and scientifically managed system of gymnastic exercises would be accepted readily by the profession. The value of active and passive muscular movements in promoting absorption, and in increasing the activity of the excreting organs, has long been recognised; and had the successors of Ling confined themselves to this alone, had they refrained from seeking alliances with the hydropath and the mesmerist, they would have had no cause to complain, as they now loudly do, of the seeming apathy of the medical world. It was only when Ling overleaped the bounds of his own sphere, and invaded the territory of legitimate medicine with an army of wild theories and empiricisms, that Retzius and his brethren shrank from his absurdities, and refused to identify themselves with a system repugnant to common sense, and unsupported by physiological or pathological argument. If there be blame to be attached to these fathers of the profession in Sweden, to these men who have won honourable laurels in the field of scientific investigation,

it is, that, perhaps cowed by popular outcry, and awed by the royal favour, in whose sunshine kinesipathy now basks in Sweden, they did not show a bolder front, and meet the enemy, like Baron Düben, face to face. A few more such critiques as those of Malgaigne, and of the last-named author, would annihilate this so-called science in its present empirical garb; while, under legitimate control, the science of muscular movements and of their effects might be securely and cautiously investigated, free from the monstrous theories and absurd practices which characterize the doctrines and treatment advocated by Messrs. Ling and Branting.

Art. IV.

The Registrar-General's Report on the Mortality of Cholera in England,
1848–49.—London, 1852.

THIS elaborate work, drawn up from the official returns of the deaths in England and Wales, is the production of Mr. Farr, to whom the Registrar-general entrusted it. Mr. Farr was assisted especially by Messrs. Hammack, Thornton, Clode, Cocke, Sowray, and Angus. We think it right to mention these names, as the work is one of first-rate character, and in point both of magnitude and importance, is indeed, to use the common phrase, a national one.

Our readers know exactly the kind of returns which are made to the Registrar-general. Liable to fallacy, and imperfect as they are in many ways, they yet embody a multitude of facts of the highest importance. In the case of the Cholera epidemic, the fallacies arising from erroneous returns and incorrect diagnosis were probably less numerous than they are in other diseases, whose characters are less clearly defined. What errors also may have occurred, must be, to a certain extent, lost in the vast numbers, or compensated for by the great preponderance of correct returns. We conceive ourselves justified, then, in receiving, with Mr. Farr, the official returns of the district registrars as sufficiently accurate to be used in a statistical inquiry.

At the close of the epidemic, a list of every death from cholera and diarrhœa in 1849, was transcribed from the registration volumes; the deaths from these two diseases amounted to 72,180, and it was calculated that simply printing the list would fill an octavo volume of 2500 pages. In order to present a concise view of the main facts, the abstracts, and the prominent facts relating to each locality, were condensed, and are now presented to the public with a running commentary by Mr. Farr, and with various tables and diagrams, prepared by his coadjutors.

Mr. Farr's commentary is full of interest; there are, of course, many points on which he must expect differences of opinion, and there are others which he has evidently not had time thoroughly to consider. But taking it altogether, his portion of the work is admirably done.

We shall give a short abstract of it in order. As might be expected, great stress is laid on hygienic conditions, and the report on cholera is preceded by two or three pages, in which reference is made to the usual rate of mortality in country and town districts. Then passing to cholera,

Mr. Farr alludes to the outbreaks in India, and to the prevalence of summer cholera in England in 1846. He then passes to the epidemic of 1848-49 in London, and refers the first cases of the "new form" to the end of September. The weekly returns, and remarks on the spread of the epidemic throughout England, are then appended. And next we have sections on the following subjects :

1. *Influence of Sex on the Mortality from Cholera.*—Cholera in 1849 killed, in England and Wales, 26,108 males ; 27,185 females. Corrected for population (as there is an excess of females) one male died out of every 331 ; one female in every 333 ; so that, on the whole, males suffered slightly more than females. In some districts, however, the proportions were different. At the beginning of the epidemic the deaths of males were in excess, but at the height the deaths of females exceeded those of males.

2. *Influence of Age.*—Cholera was fatal at all ages, but the middle period of life suffered most. The diarrhœa, on the other hand, which Mr. Farr looks upon as "cholera with the striking but not essential symptoms suppressed," was more fatal to children and old persons. Classing the cholera and diarrhœa deaths together, it appears that men of the age of 25–35 were twice as liable in this cholera epidemic to die, as those 10 years younger (15–25). From this period the danger increases with age.

3. *Duration of Fatal Cases of Cholera.* — The mean duration of fatal cases was, in men, 49·44 hours ; in women, 50·44 hours. It appeared to diminish as life advances ; thus the duration in both sexes at the age of 15–35, was 50·904 hours ; at the age of 35–55, it was 46·896 hours ; and at the age of 55 and upwards, 47·352 hours. Fatal diarrhœa lasted much longer—viz., 16·044 days in males, and 16·692 days in females.

4. *Course of the Epidemics of* 1831-2 *and* 1848-9.—The two epidemics followed generally the same track. In London, Portsmouth, Bristol, Shrewsbury, Wigan, Liverpool, Leeds, Hull, and Merthyr-Tydfil, the mortality was greater in 1849 than in 1832. In Exeter, Plymouth, Gloucester, King's Lynn, Norwich, Nottingham, Sheffield, Carlisle, Newcastle-upon-Tyne, and Sunderland, the mortality was less in the last than in the first epidemic.

Each epidemic lasted 15 months, and commenced in October [end of September ?]. The epidemic of 1831–2 had a first eruption, lasting from October to May, when the mortality descended to the lowest point ; and during this time, out of 1000 deaths occurring in the whole epidemic, 174 took place, or 17 per cent. of the total deaths. From May to December was the second eruption, which reached its intensity in August ; and during it, 826 deaths occurred out of every 1000 distributed over the whole period of the epidemic. The epidemic of 1848-9 had a first eruption, which reached its height in the fourth month, and descended to the lowest point in May, 1849 ; it caused 29 per cent. of the total deaths. The second eruption reached its height in September, and caused 79 per cent. of the total deaths.

In Paris, also, each epidemic had two eruptions.

The following table will show at a glance the relative mortality in the different months of the two epidemics ; the numbers representing the percentages of the total mortality in each epidemic respectively.

Months.	1831–2.	1848–9.
October	?	·651
November	·314	·691
December	·912	·689
January	1·986	1·210
February	2·289	·682
March	4·912	·555
April	4·530	·197
May	2·419	·601
June	4·408	3·761
July	15·574	13·916
August	28·699	29·178
September	17·718	37·463
October	13·194	8·555
November	2·593	1·552
December	·453	·300

Influence of Temperature.—A table is given showing the temperature of London during the year 1849, and the deaths from cholera. In May, June, and July, the temperature and the mortality from cholera gradually rose; the former reached its acme in August, the latter in September. Mr. Farr evidently attaches great weight to the influence of temperature on cholera.

Influence of Locality.—The influence of locality on cholera was extraordinary, and is well illustrated by the fact, that 46,592 of the 53,293 deaths from cholera, in the year 1849, occurred in only 134 out of 623 districts. To put this in other words, 46,592 deaths occurred in an area of 7839 square miles, on which *four-tenths* of the population dwelt, while only 6701 deaths occurred on an area of 49,228 square miles, on which dwelt *six-tenths* of the population.

Eighty-five districts in England escaped the cholera altogether. These districts lie generally high, and are thinly peopled. Their population has scarcely augmented during the last 10 years; the average mortality in all during the 10 years has been very low—viz., 1·75 per cent.

In 73 other districts, there was no cholera, but some diarrhœa. In these districts, the average annual mortality for 10 years has been 1·905 per cent., and the population during that time has only augmented 3 per cent.

The cholera was three times more fatal on the coast than in the interior; and on further analysis it appeared that the fatality was greatest in the chief seaport districts. Thus 26,773 deaths, or *more than half* those of the whole country, occurred in London, Liverpool, Hull, Bristol, Plymouth, Portsmouth, Southampton, and Tynemouth. Setting aside London, the sea districts may be divided into three groups :—

(a.) *Including the Large Ports.*—Here a dense population is crowded on the low alluvial soils of the mouths of rivers; and the deaths from cholera were as 125 to 10,000 persons living.

(b.) *Including the Secondary Ports.*—The mortality was as 47 to 10,000 persons living.

(c.) *In the other Coast Districts, which included only small ports, often inaccessible to ships,* the mortality was only as 15 to 10,000 persons living.

On comparing more particularly sea-coast and inland districts, it is found that in 47 districts on the river and sea-margins, there were 85 deaths from cholera in every 10,000 inhabitants; while in 41 inland districts, comprising the large towns (except London), whose aggregate population and whose average mortality are both greater than those of the 47 coast districts, there were only 38 deaths to 10,000 persons living. The fatality in the two groups was in fact as 2¼ to 1.

In the rest of the kingdom, after the abstraction of these two groups, the mortality was only as 12 in every 10,000 inhabitants.

This great prevalence of cholera and mortality in the coast districts, has always been a strong argument for the contagionist's doctrine, which referred the disease to intercourse with other infected ports. It is, however, extremely probable, if not certain, that too much stress has been laid on this argument, and that prevalence of mortality in the great ports is due in great measure to the low level in which they stand, and to their position on alluvial soil. The influence of elevation on cholera will appear more fully in considering the causes of the mortality in London.

The *coal districts* suffered considerably; the deaths being 46 from cholera, and 13 from diarrhœa, to 10,000 inhabitants. The *marshy districts* of Lincolnshire and Cambridgeshire suffered very little; North Witchford, Whittlesey, and Wisbeach escaped, so also did some of the marsh districts of Essex and Kent. In the Romney marsh only one death occurred.

Geological formation appeared to influence it in some degree; the districts on the Granitic, Silurian, and Devonian systems nearly escaped. Herefordshire in the old red sandstone escaped, while Cornwall and part of Devonshire on the same formation suffered severely;—still other circumstances were in play here, and the influence of geographical formation cannot be very great.

Before alluding more particularly to the influence of locality, we pass on to the

Causes of the Mortality in London.—A very curious and interesting chart (plate iv.) is given in the appendix, by which the mean temperature of any week, and the mortality in London of the years 1840–9 inclusive, can be ascertained. Some curious relations which would be estimated with difficulty by figures, are brought out with great ease by this admirable plan, and the chart in question deserves very attentive study. Its chief fact is, that in London there are two healthy and unhealthy seasons; April, May, June, and the greater part of July, constitute the first healthy season—i.e., the deaths are under the average; then, in consequence apparently of the temperature remaining above 60° (Fahr.), August and September are unhealthy; in October and November the deaths are again below the average. In December, January, and February, if the temperature remains below 40°, the second and more unhealthy season occurs, and the deaths are considerably above the average. The cholera both in 1832 and 1849 caused a great increase of mortality in London, at a period of the year usually unhealthy—viz., in August and September. The first cases occurred south of the Thames, and throughout the epidemic the mortality was greatest in this district.

The chief elements which may be supposed to influence the mortality of an epidemic, and which we have the means of investigating, are, the water

supply, the drainage and elevation of soil, the density of population, and the poverty of the inhabitants.

(*a.*) *Water-Supply.*   The Thames supplies a great portion of the water; it collects streams from 6160 square miles of country, and receives the sewage of several millions of people.   From this vast and polluted river, a great amount of evaporation goes on, especially, of course, when the temperature is high. The amount of this evaporation is almost incredible; it is estimated by Mr. Glaisher as annually 1,523,242,991 gallons of water from a surface of water of 2245 acres in extent, which is the estimated surface of the Thames; or, in other words, 18,000 tons of water are raised from the Thames daily, and diffused more or less over the town.   This vapour carries up, it is presumed, organic matter, but the quantity of this is undetermined.

In the six districts supplied by water from the Thames at Kew and Hammersmith, the mortality was as 15 in 10,000 inhabitants.   In 20 districts, supplied from the Amwell, the Lea, and the Ravensbourne, 48 in 10,000 died of cholera.

In 12 districts, supplied from the Thames between Battersea and Waterloo-bridge, 123 in 10,000 died of cholera.

Of course this difference is not due solely, or even in any great degree, to the water-supply; because other more important elements have to be taken into consideration, and one of the most striking of these is the

(*b.*) *Elevation.* — Of all the causes influencing the spread and the mortality of cholera, none has so great an effect as elevation. This fact, known for a long time, has been worked out by Mr. Farr so perfectly, that it may be received like the solution of a mathematical problem.   We shall quote verbatim as much as possible of Mr. Farr's statements on this point, referring to the book itself for the tables, and for the various proofs and calculations on which they are based:

" The mortality [and therefore, presumably, the prevalence, *Rev.*] from cholera is in the inverse ratio of the elevation.   The mortality of the 19 highest districts was at the rate of 33 in 10,000; and of the 19 lowest districts, 100 in 10,000. The elevation in the two groups was as 71 to 10 feet above the high-water mark of the Thames, or as 7 to 1, while the mortality was as 1 to 3, or in the inverse ratio. ˏ In the two groups of the six districts supplied with the waters of the Thames at Kew and Hammersmith, the mean elevation was 35, and 175 feet, the mortality from cholera 19 and 11 in 10,000.   In the two groups of 12 districts supplied with the Thames water between the Waterloo and Battersea bridges, the mean elevations were ⅓ foot and 10 feet, the mortality 168 and 77 in 10,000.   In the two groups of 20 districts, supplied with the waters of the New River and the sea, the mean elevation was 24 and 59½ feet, the mortality from cholera was 59 and 37.   While the effects of the water and of the wealth of the districts are apparent, they do not in this analysis conceal the effects of elevation.   Cholera was excessively fatal in all the four districts which lie on a level with, or below, the Trinity high-water mark; it destroyed 144, 161, 164, and 205 in 10,000 inhabitants.   In the five districts which lie two to four feet higher, on an average, the mortality was at the rate of 68, 97, 120, 153, and 181 in 10,000. In 10 districts of an elevation of 50 feet and upwards, the mortality from cholera was not higher than 8, 8, 17, 19, 22, 22, 25, 35, 35, and 53, in 10,000.   The last mortality occurred in St. Giles, in which the beneficial effect of elevation was neutralized by other causes." (pp. 61–2.)

On further examination it is evident, that notwithstanding disturbing

causes, the mortality from cholera bore a constant relation to the elevation. Thus, on arranging the districts into 7 terraces, the first under 20 feet of elevation, the second under 40, the third under 60, and so on, it is found that the mortality on the ground under 20 feet being estimated by 1, the mortality of each successive terrace may be represented by $\frac{1}{2}, \frac{1}{3}, \frac{1}{4}, \frac{1}{5}, \frac{1}{6}$, or the mortality on each successive altitude of 20 feet, was $\frac{1}{2}, \frac{2}{3}, \frac{3}{4}, \frac{4}{5}, \frac{5}{6}$, &c., of the terrace immediately below it. The following table will make this point clearer. The highest terrace (340–360 feet) is Hampstead:

Elevation of Districts, in feet.	Number of terrace from bottom.	Deaths from cholera in 10,000 inhabitants.	Calculated series.
20	1	102	$\frac{102}{1} = 102$
20 — 40	2	65	$\frac{102}{2} = 51$
40 — 60	3	34	$\frac{102}{3} = 34$
60 — 80	4	27	$\frac{102}{4} = 26$
80 — 100	5	22	$\frac{102}{5} = 20$
100 — 120	6	17	$\frac{102}{6} = 17$
340 — 360	18	7	$\frac{108}{18} = 6$

The greatest discrepancy between the actual and the calculated mortality is at the mean elevation of 20–40 feet, and on examination this elevation is found to include the districts of Bethnal Green and Wandsworth, where other disturbing causes combined to heighten the mortality.

We have already seen that the effect of bad water almost disappears under the paramount influence of elevation. We shall now extract a portion of another table, to show that density of population and over-crowding, which exerts some influence over mortality, has also by the side of elevation comparatively little effect:

Elevation in feet above Trinity high-water mark.	Annual mortality to 10,000 persons living.		Number of persons to	
	Cholera, (1849.)	All causes, (1838-44.)	An acre.	A house.
Under 20 feet	102	251	74	6·8
20 — 40	65	237	105	7·6
40 — 60	34	235	184	8·5
60 — 80	27	236	152	8·8
80 — 100	22	211	44	7·7

The extraordinary effect of elevation appeared so important, that it was thought right to submit the principle to another test, by comparing the elevation and the mortality from cholera of each sub-district. The result entirely confirms the announced law.

This law of elevation is perhaps the most important practical point brought out in the Report, and is well worthy the attention of the authorities of the East India Company; for the fact, though long recognised, has never been so definitely shown before.

(c.) *Density of Population.*—Contrasted with elevation, density has little effect; yet when the effect of elevation is eliminated, density is found to have some influence. Thus in the 19 densest districts, where the inhabitants were 178 to an acre, the mortality was 71 in 10,000; whereas in the 19 least dense districts, where there were only 34 inhabitants to an acre, the mortality was 61 in 10,000.

(d.) *Wealth and Poverty.*—The influence of wealth and poverty cannot be fixed, and appears to be less considerable than was supposed. Elevation, however, interferes so much with the results, that it is unsafe to hazard any conclusions.

(e.) *Influence of General Insalubrity of District.*—There is a very constant relation between the mortality from ordinary causes, and the density of population. Places generally insalubrious suffered most from cholera. Thus of the 38 districts into which London is divided, 19 have a density of 155 persons to an acre, a mean elevation of 26 feet, and a mean annual mortality of 268 in 10,000; the mortality from cholera was 84 to 10,000. The 19 other districts have a density of 57 persons to an acre, an elevation of 55 feet, and a mean annual mortality of 212 in 10,000; the mortality from cholera was 48 in 10,000. The facts appear to be, that a low elevation contributes to general insalubrity, as well as to cholera; but that its effect is not so great on other diseases as density of population.

Inferences from these Facts.—If we apply these facts, derived from a careful study of the epidemic in London, to the country generally, it is found that the inferences drawn from them hold good everywhere. In every place, elevation exerted a paramount effect; and density of population and poverty were subsidiary influences. It remains, then, carefully to consider in what manner elevation acts.

"As we ascend," says Mr. Farr, "the pressure of the atmosphere diminishes; the temperature decreases, the fall of water increases, the vegetation varies, and successive families of plants and animals appear in different zones of elevation. The waters roll along the surface of the rocks, or filter through them and the porous strata of the earth, to burst out below—the sources of rivers or of tributaries, which carry disintegrated rocks with the remains and excretions of vegetables, animals, or men, in every stage of decomposition. The deposits in stagnant places, and at the estuaries, show the kind and quantity of mixed matter which the laden rivers carry down and deposit on the low margins of the sea at the tidal confluences of the fresh and salt waters. . . . .

"As the rivers descend, the fall of their beds often grows less, and the water creeps sluggishly along, or oozes and meanders through the alluvial soil. The drainage of the towns is difficult on the low ground, and the impurities lie on the surface or filter into the earth. The wells and all the waters are infected. Where the houses are built on hill-sides and elevations, as in London, the sewage of each successive terrace flows through the terrace below it, and the stream widens, the ground becomes more charged, every successive step of the descent, until it is completely saturated in the parts lying below the high-water mark.

"The river, the canals, the docks, and the soil of a port may be viewed as a large basin full of an almost infinite variety of organic matters, undergoing infusion and distillation at varying temperatures; and as the aqueous vapour which is given off

ascends, it will be impregnated with a quantity of the products of the chemical action going on below, variable in amount, but necessarily greatest in the lowest and foulest parts. . . . . The amount of organic matter, then, in the atmosphere we breathe, and in the water, will differ at different elevations, and the law which regulates its distribution will bear some resemblance to the law regulating the mortality from cholera at the various elevations. It has been seen how rapidly in London the mortality from cholera diminishes a few feet above the low ground on a level with the Thames, while several feet of elevation in higher regions produces no sensible effect. . . . .

"It is established by observation, that cholera is most fatal in the low towns and in the low parts of London, where, from various causes, the greatest quantity of organic matter is in a state of chemical action; and it may be admitted that cholera, varying in intensity with the quantity, is the result of some *change* in the *chemical action* of this matter. Further inquiry must determine whether in England that change is spontaneous, or the result of the introduction of a zymotic matter from beyond the seas; whether the poison enters the human frame in air or water, through the skin, the mucous membrane, or the air-cells of the lungs." (pp. 69–70.)

The readers of our journal need scarcely be reminded how frequently we have advocated views identical with these, and how we have over and over again pointed out, that all observers who have regarded cholera with an unprejudiced eye, from the days of Jameson downwards, have adopted opinions of a similar kind. Let us hope that this reiterated assertion—an assertion based on observations so numerous and so accurate, may at last have some weight with the rulers of this and other countries; and that we may at length commence in good earnest those works of sanitary improvement, the neglect of which is the opprobrium of the present generation, and the fatal legacy which it seems is to be inherited by the next.

Mr. Farr does not allude here to a *modus operandi* of a humid atmosphere already charged with organic exhalations, which seems to us very important—namely, its effect in causing the retention within the body of various organic excreta, which are given off especially by the lungs, and which require pure air in quantity to oxidize them to the degree requisite for elimination. It is probable that by their accumulation in the system, a predisposition is given to cholera as to so many other diseases; but to this subject we must devote some of our space on a future occasion.

The latter half of Mr. Farr's Report is not so good as the first. He quits his facts and figures, and ventures into the shadowy regions of "theories and analogies." The cause of cholera, whether from volcanic action, electricity, ozone, heat, fungi, bad water, is discussed shortly and imperfectly; then Dr. Snow's views, contagion, and spontaneous development, come in for a few pages. Subsequently, Mr. Farr, with the intention of showing that other zymotic diseases are governed by similar laws to those of cholera, discusses shortly the effect of elevation on ague, yellow fever, and plague. Various other speculations are then brought into view; such as the salubrity and unhealthiness of ancient and modern Rome and of Egypt, sanatory instinct, the effect of healthy places on animals, the effects of the earth on race, the degeneration of race in unhealthy places, &c. Many of these topics are handled with skill, and all of them will be read with interest, although they are fragmentary, and, we cannot but think, rather misplaced in the present Report. The latter part of the Report is indeed expanded into a kind of treatise on general hygiène, but it is a sketchy and imperfect treatise.

There are some points, too, on which we cannot agree with Mr. Farr. Thus, he has clearly underrated the great influence of long continued fatigue, as of long marches by troops, in giving a predisposition to cholera.   He says that such long marches only expose troops for a longer time to the causes of the disease.   But he has entirely overlooked the cases in which the march has been through a healthy country, and the attack has occurred in cantonments which had become infected subsequently to the entrance of the healthy troops : the way in which the eighty-sixth regiment suffered at Kurrachee is an example of this ;* and many others might be quoted.   It is likely, or at any rate very possible, that after long marches and great fatigue, the system is impregnated with the products of the used tissues which have been decomposed with unusual rapidity, and probably, therefore, not so perfectly as to allow their complete oxidation and excretion from the system.   Whether this be the case or not, the effect of long marches as a predisposing cause of cholera, is certain.

We will not, however, lessen the pleasure of our task by seeking out points on which we differ from Mr. Farr.   We would rather thank him gratefully for his most able and interesting Report.   It will always be a work of authority and reference ; and in its numerous tables and calculations it contains vast mines of facts, which we trust that future industrious observers will not be slow to explore.   The work is another proof, if such were needed, of the important results to medical science of this registration of deaths.   Among the greatest services which have been rendered by the registrar-general, is the light which his researches have thrown on the vital question of hygiène.   For what has already been done in this direction, we have in great measure to thank Mr. Farr.   We can safely say, that few men have done more good in their generation, and that no physician of this or any other time has been more instrumental in pointing out how the ravages of disease may be checked at the outset most speedily and efficiently.

We have numerous works on cholera before us, to which we hope soon to do justice.   Dr. John Taylor's excellent Report on Cholera in Huddersfield ; Dr. Bryson's and Dr. Snow's treatises, and almost innumerable German and French works, among which Schmidt's original and important treatise is pre-eminent, will be carefully examined as speedily as may be.

Art. V.

Des Vices Congenitaux de Conformation des Articulations.   Par Alph.
  Robert.
On Congenital Deformities of the Joints.   By A. Robert.—*Paris,* 1851.
  8vo, pp. 260.

This little work constituted the Thesis presented by M. Robert, during the concours which took place for the chair of Clinical Surgery in the Faculty of Medicine in 1851.   It presents a very intelligent summary of what is known upon the subject : but we should have expected that one enjoying so high a reputation, and possessed of so wide a field, as the surgeon of the Beaujon, would have been able to contribute more from his

* See British and Foreign Medico-Chirurgical Review, vol. ii. p. 85.

own resources than has been the case. We shall pass the various sections of his work briefly under review.

1. *Pathological Anatomy of Congenital Deformities of the Joints.*—The anatomical changes are of the most varied kinds, according to the form, degree, and primary cause of the deformity, the articulation that is the subject of it, and the length of time that has elapsed from birth when the parts are examined. As the majority of cases have only been examined in the adult condition, the pathological evolution of the changes consequent on congenital displacements is much less known than it would be, had they been examined in the various stages of such progress. In considering, with this reservation, the condition of the different parts forming the joint, we find that the *bones* may be absent entirely or in part, be separated from each other by unnatural intervals, or be abnormally fused together. They may be luxated, and they may become the subjects of organic lesions, as in the adult. In other cases their development is defective or arrested. As a general rule, atrophy is met with in all cases in which the movements of the limb have lost much of their extent. It is one of the best established laws in physiology, that diarthrodial surfaces owe the integrity of their forms and of their nutrition to the pressure they exert on each other in their normal condition; and when this ceases to prevail, the heads of the bones lose their geometrical forms, become rugous, and deprived of their articular cartilages, and even are changed into conical or filiform osseous prolongations, while the cavities become filled up so as to present irregular depressions, or even projections. If the bones have not entirely lost their relations, as in sub-luxations, the osseous surfaces rub against each other in new and abnormal spots, and reciprocally leave traces of wasting which are most seen on the least moveable bone. The cartilages become destroyed, and the surfaces eburnated by this compression and friction, which are among the chief causes of the deformed appearance of the heads and condyles of the bones, and of the formation of the new cavities for the reception of these. When, however, the displaced bone does not come into contact with any osseous surface, it still undergoes change, hollows itself out a receptacle in the muscular parts, and becoming surrounded by these, by ligamentous adhesions, and by an accidental synovial bursa, assumes some resemblance to a normal articulation.

The *ligaments* and fibrous capsules undergo modifications in form, length, and structure. At first, abnormal laxity of the ligaments is often the only change which the joints present. They are thinner and more distended, but their insertions into the bones have continued normal, their direction only being changed by the displacement of the articular surfaces. This relaxation continues to increase with the progress of the affection; but as fibrous structures yield very slowly to traction, they long strive against the displacements which muscular traction and the action of gravity are constantly tending to produce. In the deviations or sub-luxations of ginglymoid joints, while some of the ligaments are distended, those opposed to them become shortened, sometimes with and sometimes without an accompanying interstitial hypertrophy. The *synovial membranes* follow the displacement of the capsules, and may secrete more abundantly than in the normal condition.

The alterations in the *muscles* play an important part, these being both

numerous and varied, and capable of arrangement under various heads, accordingly as they affect the form, size, direction, or texture of these parts. When certain portions of the skeleton are wanting, the muscles that should be inserted into these may be also absent; and, in congenital anchylosis, many of the muscles fitted for moving the bony levers now confounded together, may be wanting—these last cases being, however, rare. Alterations in the length of muscles, in congenital deviations and dislocations, are far more common; such modifications being usually accompanied by changes in their direction.

" Consequences of the highest interest flow from the facts we have mentioned : 1. Changes in the direction of muscles, the increase or the diminution of the angle that their insertions form with the bony levers, may entirely change their physiological action. Thus, in very marked deviations of a limb, we may observe the flexors becoming extensors, and reciprocally; this depending upon the displacement they have undergone. M. Bouvier has related to us the case of an individual, the subject of extreme flexion of the knees, in whom the triceps femoris, at first tense, had luxated the patella outwardly, and had then slid over the external surface of the condyle so as to become a flexor of the leg upon the thigh.—2. The deviation of a limb, by rendering the insertion of a muscle more favourable for motion, multiplies its action, and causes it to predominate over that of its antagonist, in spite of the hypertrophied condition which the latter often presents. This fact is observed in club-foot.—3. Finally, it results from the property of tonicity, that when a muscle, the points of attachment of which have become approximated, has remained long in this position and become accommodated to it, it will become really shorter, and will oppose an energetic obstacle to the rectification of the deformity." (p. 16.)

The muscles in a great number of instances, and especially in the congenital deformities of new-born infants, are in the possession of all their structural and functional properties; but this is not always the case, for in other cases their structure may undergo change or their contractility may be lost. In some few cases the whole or part of a muscle may become hypertrophied, being thick, red, and larger than in the normal state; but in general it is found in quite an opposite state, flaccid, and less coloured. It may be in different degrees penetrated by adipose tissue, to the extent of almost entirely transforming it into fat. In other cases the red portion of the muscle may disappear without being replaced by fat, the aponeurotic and areolar frame-work of the organ acquiring a marked predominance over the fleshy substance, and what has been termed the fibrous transformation being produced. In the present state of science we are unable to point out the physical and physiological conditions by which these two results are determined. Fatty infiltration may invade a tense or a relaxed muscle, and in the same muscle two neighbouring fasciculi may present the two different changes. M. Guerin regards the fatty transformation as consecutive to paralysis, and the fibrous transformation as the ultimate stage of muscular retraction. M. Robert, however, quite agrees in the conclusions of M. Bouvier, who has dissected various muscles affected with old contractions, without ever observing transformation of fleshy into fibrous or tendinous structure. The tendons in such cases participate in the atrophy of the fleshy parts. M. Broca, prosector to the faculty, has also dissected 5 specimens of congenital club-foot, and about 15 other specimens, the congenital origin of which was doubtful; and in none of these has he met with this fibrous transformation.

2. *Classification.*—M. Robert distributes the various forms of congenital deformity of the joints into four groups, dependent upon anchylosis, diastasis, the absence of bony parts, and deviation or luxation.

(a.) *Deformities of the Joints dependent upon Congenital Anchylosis.*—This may be complete, so as to induce absolute immovability of a joint, or incomplete, so as only to considerably reduce the extent of its movements. M. Robert refers to a case of congenital union of the calcaneum and cuboid bones observed on both sides; and to another in which the calcaneum and the scaphoid formed on both sides but a single cartilaginous mass, having a distinct point of ossification for each bone. He also quotes at full length a remarkable case from the *Bull. de la Soc. Anat.*, 1836. A man died, aged 27, whose left arm was seven inches shorter and much smaller than the right, and was said to have been always so. Besides other changes in the skeleton, a flat or rather slightly convex surface was found in place of the glenoid cavity of the scapula. The humerus had no head, but possessed a slightly convex articular surface, stretching obliquely downwards and inwards, at the place of its usual neck. This bone was larger below than above, and was so absolutely united to the radius, that no trace of former articulation could be discovered. No trace of a cavity for the olecranon or coronoid process existed. The ulna was defective at its middle part, while its extremities were hardly recognisable. In the *Histoire de l'Acad. des Sciences*, 1716, Deslandes describes the case of an infant in whom no joints whatever existed, the body being as but one single bone. Naegele attributes the production of the oblique oval deformity of the pelvis to congenital anchylosis.

(b.) *Congenital Deformity of the Joints from Diastasis.*—Diastasis may be said to be present in a joint, when paralysis of the muscles allows the articular surfaces to separate more or less from each other, as in the case of the shoulder. But M. Robert only wishes to indicate the restricted group of cases, in which two bones naturally articulated remain separated from each other. Examples of this are met with only along the median line, and the cranial sutures. M. Guerin cites the case of a monster in which was observed a considerable separation of the symphysis pubis, together with a slight loosening of the sacro-iliac symphyses. Pubic diastasis is usually accompanied by extroversion of the bladder.

(c.) *Congenital Deformity from Absence of one of the Articular Extremities, or the whole of a Bone.*—In these cases, the articular extremity which remains has always undergone more or less marked modifications in its form, and more or less considerable changes in its relations. The deformities they give rise to may easily be mistaken for those produced by sub-luxations and luxations. They have been met with in almost all the joints, as the jaw, clavicle, forearm, &c. The radius is not unfrequently alone absent; but a case has never been met with, in which the ulna was alone absent, the radius persisting. In the Museum of the Faculty are several specimens in which the head and neck of the femur are absent, the great trochanter forming the upper extremity of the bone. A most conclusive example of the congenital nature of the affection is one in which these changes are observed on both sides. Duval relates the case of a child having two club-feet, both patellæ being absent; and in a case of double luxation of the knee, Vultzer was unable to find either patella. In several

cases the fibula has been absent while the tibia remained; but the reverse disposition has not been observed.

(d.) *Congenital Deformities with Deviation, Sub-luxation, or Luxation.*—This group embraces the most numerous and important deformities. It is separable into two sections, constituting different degrees. The first of these is little distinguished from the normal condition; changes existing in the articular surfaces, or their mode of union, which do not give rise to notable deformity in themselves, but which predispose to displacement from very slight traumatic causes, or from the mere operation of the will. M. Robert agrees with Boyer in considering that in instances of imperfect articular surfaces, the ligaments are always in a relaxed state. It is probable that this faulty conformation may explain the frequency of relapse sometimes observed after luxation. M. Robert quotes a case from the third volume of Malgaigne's *Journal de Chirurgie*, in which spontaneous luxation of both patellæ could be produced in consequence of the laxity of the femoro-rotular ligaments—the two knees being quite normal in their conformation. Portal relates a case in which the femur could be spontaneously luxated; and M. Humbert relates that of a surgeon who had from infancy possessed the power of luxating the femur and replacing it.

In the other section, the defective articular formation is exhibited by various deformities. There may be mere *deviation*, in which the direction of the articular surfaces may become changed, without their ceasing to remain in contact, or the ligaments becoming relaxed. In the knee, lateral deviation may be produced by one of the condyles being shorter or smaller than the other. Sandifort relates a curious example of a deviation simulating a luxation. The neck of each femur, instead of being inserted by its base, at the upper and inner part of the bone, adhered to its anterior surface, so that the two limbs, strongly rotated inwards, presented their patellar aspect to each other. Congenital shortness of the radius sometimes causes a deviation of the hand outwards. Deviations also arise when one or more of the short massive bones present a diminution of thickness at any part of their surface. Some slight cases of club-foot are due to this change alone. The articular surfaces have undergone a *sub-luxation*, when their normal points of relation have become diminished without having entirely ceased; and the luxation is *complete*, when the bones remain in contact at no part of their surfaces. The deviations are especially found in the ginglymoid joints. The sub-luxations are chiefly seen in those possessed of extensive plane surfaces. This is the case with the tarsus and spine, in which, by reason of the multiplicity of articulations united end to end, even slight sub-luxation will induce deformity. Complete luxations are most frequently met with in enarthroses, and the joints which approach these, as the radio-humeral and the astragalo-scaphoid, which in very bad club-foot is almost the only joint that is completely luxated. According to M. Guerin, complete congenital luxation can be produced at once; but such cases must be very rare, most passing through the stage of sub-luxation.

M. Robert passes in review the whole of the joints in which congenital luxation, in any of its degrees, is possible. Into this detail we cannot follow him; and this is the less to be regretted, as most of the cases he refers to have been already published; and we may observe, that no one among the authors he has quoted occupies a more honourable and conspicuous

position than Professor Smith, of Dublin.   We may, however, notice some of the observations respecting the principal joints.   Of the appearance of the shoulder in *Congenital Dislocation of the Humerus,* M. Robert says,

" These different alterations give rise to considerable changes in the form of the region, and in the attitude and functions of the limb.   The first thing that strikes the eye is the change in the configuration of the shoulder, which is considerably reduced in size, the corresponding side of the thorax participating in the atrophy. The arm has become small, the deformity looking the more conspicuous, inasmuch as the forearm frequently preserves an almost abnormal size.   In consequence of the absence of muscular projections, the prominence of the shoulder is pointed and angular.   The bones of the acromio-coracoidean arch project under the skin. The head is inclined towards the affected side.   The arm may be immovably fixed by muscular retraction; but generally it is, as it were, floating, hanging by the side of the body, as if only slightly attached to the scapula.   It has, in conse- quence of the atrophy of the humerus, lost in length, although its head is almost always lower than in the normal state, and more or less distant from the acro- mion.   The laxity of the capsule gives rise to an abnormal mobility of the joint, which allows of a more or less natural form to be temporarily given to the region, which disappears when the arm is left again to hang down." (p. 73.)

Congenital Dislocation of the Elbow.—M. Robert considers the con- genital origin of some of the cases that have been published very doubtful; especially as recent observations have shown that more or less complete traumatic luxation of the upper extremity of the radius, whether ante- riorly or posteriorly, may occur in very young children.   Such lesions are often mistaken or overlooked, until the deformity or difficulty of movement draws attention to them.   Nevertheless, among many of the facts recorded, there is a remarkable similarity; and certain pecu- liarities sometimes enable them to be distinguished from old unreduced traumatic dislocations.   These facts enable us to admit the following varieties :—1st. Complete luxation of the two bones backwards, a case of which is given by Chaussier.—2nd. Complete luxation of the upper end of the radius, forwards : Guerin has observed this on both sides in a girl, aged seven.—3rd. Complete luxation of the upper end of the radius, upwards and outwards: two conclusive cases of this are related by Adams and Deville.—4th. Luxation of the head of the radius, upwards and back- wards.   This is the only form that has occurred frequently enough to admit of any description at all general.   After referring to cases by Loir, Cruveilhier, and Smith, and describing a new one at length, M. Robert observes,

" In these cases there is generally to be noted deformity and smallness of the head and neck of the radius, coinciding with a notable elongation of these parts, which permits them to pass more or less upwards and backwards, beyond the lesser condyle of the humerus.   This last is rudimentary, while, on the other hand, the trochlea is expanded, in order to articulate by a broader surface with the upper end of the ulna, which is also more developed.   In four cases, a junction of the two bones of the forearm, at different points of their length, has been noted. The vestiges of the radial head are maintained by a ligamentous apparatus, more or less representing traces of the external, lateral, and the annular ligaments." (p. 85.)

While, as from the configuration of its articular surface might be expected, the radius quits its natural connexions so easily, it is very rare to find the *ulna* so doing.

Congenital Deformity of the Wrist.—Congenital permanent deviations of the hand (*main-bot*) originate either in displacement at the joint of the wrist, or in the atrophy or partial absence of the radius, and in certain cases of the carpus. The affection is a rare one, and ordinarily co-exists with club-foot. M. Robert arranges the various cases on record in accordance with the various movements of the joint. He terms them (1) *palmar*, when the palmar face of the hand is more or less flexed upon the anterior surface of the forearm. An interesting example of this is related by Davaine in the *Bull. de la Soc. de Biol.*, 1850. It was observed in a seven-months' fœtus, which presented nothing else abnormal. The radius of the left arm was absent. The humerus was a centimetre longer than the opposite one. The ulna was shorter, but more expanded than the other, especially at its upper extremity. The thumb and its metacarpus were absent; and the hand was articulated with the anterior surface of the lower extremity of the ulna, with which it formed a right angle. Cases are also quoted from Smith and Cruveilhier; and the autopsy of another by M. Follin. At the end of a wasted forearm, the hand, also atrophied, was found bent upon the arm, and incapable of being straightened beyond a right angle. Adipose tissue well developed was found, but no subcutaneous bursa. The palmar aponeurosis was not retracted, and the muscular masses of the arm and forearm had undergone neither fatty nor fibrous degeneration. The retraction affected almost exclusively the flexor carpi radialis and the flexores digitorum. These muscles were three centimetres shorter than in the normal state. The palmaris longus was only slightly retracted. The flexor carpi ulnaris and supinator longus were in their normal state. The muscles at the posterior surface of the forearm and hand were considerably elongated. The bones were notably diminished in size.—2nd. The *dorsal* face of the hand forms an angle with the dorsal surface of the forearm. Two cases are quoted from Smith, due to luxations of the carpus.—3rd. In the *ulnar*, the inner edge of the hand is more or less inclined towards the ulnar border of the forearm. M. Robert is only aware of one example of this, which he briefly refers to.—4th. In the *radial*, an analogous deviation takes place towards the radial edge of the arm. This is of more common occurrence than the other varieties, and is connected with a congenital shortness of the radius, absence of its lower extremity, or an entire absence of the bone. The thumb is also generally wanting. Davaine found in a seven-months' fœtus, that the two radii were absent, the two ulnas being shorter and broader than normal, especially at their upper ends. They articulated below with the carpi, the thumbs ànd their metacarpal bones being absent. Other portions of the skeleton offered anomalies, an absence of the left branch of the lower jaw being one of these. The above varieties may be combined with each other.

Congenital Luxation of the Hip-joint.—Female children are more often the subjects of this than male; and though it is met with single, it is oftener double. Several varieties have been observed. 1. Guerin has proved the existence of a *luxation directly upwards*, the head of the bone being placed immediately external to the anterior inferior spine of the ilium, corresponding to the anterior supra-cotyloidean traumatic dislocation, frequently described of late. 2. A *supra-pubic* dislocation, in which

the head of the bone, resting on the ileo-pubic eminence, forms a very distinct tumour in the groin.   3. Chaussier has described a luxation into the *foramen ovale.*   These varieties, though rare, having been observed in the fœtus, are indubitable.   4. M. Guerin describes a *sub-luxation backwards and upwards,* in which the head has not passed beyond the cotyloidean margin.   5. The immense majority of cases are examples of *luxation upwards and outwards,* the head of the bone lying on the external surface of the ilium.

In complete luxation, the head becomes conical and flattened in different directions, according to what part of its surface becomes applied against the ilium, so that its form and inequalities are very various.   The neck, too, is shorter, and may be faultily inserted into the body.   If the dislocation be incomplete, the cotyloid cavity becomes depressed at some part of its margin; but in complete luxation, whether a new receiving cavity be formed or not, the cotyloid cavity becomes filled up, either by the sinking down of its borders or the elevation of its centre.   Sometimes it becomes narrower, and sometimes broader, than normal, and sometimes it assumes a triangular form.   Although the cavity may long continue permeable, the deformities appertaining to it are more constant than are those of the head, and offer greater obstacles to reduction.   The round ligament may be absent, or traces of its rupture may be apparent.   In most cases it exists, and is lengthened out into a ribbon form, or is almost filamentous.   In complete luxation, without pseudarthrosis, the fibrous capsule still retains its points of insertion, though its direction is changed.   Bertin's ligament often becomes shortened and thickened, retaining the femur so that it cannot descend, and opposing a formidable obstacle to reduction.   In some cases, too, the elongated capsule becomes so narrowed in its middle as to assume an hour-glass form; but this change is slow of production, as Guerin and Sedillot have found it pervious at the twentieth or thirtieth year.   In *incomplete* luxation a new arthrodial surface is within a moderate time formed by an extension and deformation of the old articular surface; but in *complete* luxation the formation of an accidental cavity may be indefinitely adjourned, or may never take place—the entire capsule being continually opposed between the head of the bone and the ilium, which only presents here a slight depression.   In the case of a new joint being formed, the capsule, M. Sedillot observes, contracts adhesions to the ilium, and is converted into a kind of cartilaginous tissue.   This resistance of the capsule is why a new joint is so very much longer in forming after congenital than after traumatic luxation.   Guerin says, that it appears about the thirteenth or fourteenth year; but there is no general rule.   It is rare, when the congenital dislocation is double, for the new cavity to form on both sides; and at all events, one of the two is much more developed than the other.   The accidental cavity itself presents many varieties, as to depth, extent, and form; and sometimes there is a succession of such cavities.   The edges of the perforated capsule contract closer and closer connexions with the ilium, and the new cavity may become entirely circumscribed.   During this time, the obliteration of the normal cavity continues; while the head of the femur, recovering a portion of its functions, ceases to become further deformed, and if the subject is young resumes its growth.

The effect of this dislocation on the *pelvis* has been studied by Sedillot,

Vrolik, and Guerin ; and M. Lenoir has lucidly summed up what is known upon the subject. The effect varies accordingly as the luxation is double or single. When *double,* the diameters of all the pelvic apertures, as well as of the cavity itself, are equal to those of the normal pelvis, if not larger ; and delivery is rather facilitated than obstructed. At the superior strait the transverse diameter alone is narrowed, while the lower aperture is enlarged ten or twelve millimetres. The bones are slightly atrophied, and the general depth of the pelvis is diminished.

" In the *luxation on one side only* there is atrophy or arrest of development of the whole corresponding side of the pelvis. The ilium rises almost vertically, and no longer forms the iliac fossa. The ischium, on the other hand, projects outwards and forwards. The pubis, less raised than ordinary, is carried outwards, and its descending ramus is shorter, broader, and more vertical than that of the opposite side. The femur is also atrophied as compared with the other. The form of the sacrum is but little changed. The sacro-vertebral angle no longer corresponds with the pubic symphysis. The sacro-iliac articulations are not anchylosed. It results from all these facts, that the general direction of the pelvis has nearly undergone the same change as the oblique oval pelvis of Naegele, but in an opposite direction and less degree ; and delivery will be necessarily rendered more laborious." (p. 115.)

We have not space to notice M. Robert's description of congenital dislocation of the knee, and the varieties of club-foot.

Pathological Physiology.—The function of locomotion in the subjects of congenital dislocations acquires a peculiar physiognomy. The reciprocity, which, to a certain degree, connects the entire movements of the frame, indicates the extent of influence which each deformity will exert upon the neighbouring articulations. Thus, a congenital luxation of the hip disturbs the equilibrium of the entire skeleton, and modifies the greater part of the attitudes of the individual who is the subject of it. A kind of accommodation takes place, which restores to the patient, if not the whole, at least a certain portion of his locomotive functions. This we know occurs after acquired deformities ; but in the cases now under consideration, nature attains more advantageous results with slighter efforts, because these are more slow and unintermitting in their operation. We can easily understand that these compensatory modifications, of which the neighbouring joints are the seat, are accomplished without difficulty at a period when the bones and ligaments are so ductile, and obey without resistance the action of the surrounding agents. In this way, nature, in order to palliate the results of the original deformity, creates new deformities, which are incessantly increasing in proportion as the primary one augments.

Another law belonging to the general history of deformities, which merits attention, is the ease with which nature undoes her work, when the primary congenital deformity has been relieved. As long as the skeleton and its appendages continue flexible, they return to their normal configuration almost as easily as they departed from it. It is not in the very young child that the compensatory phenomena are most manifest, these appearing rather when the limbs are brought into action. It is only when locomotion commences, that the peculiar attitudes of a child suffering from club-foot or dislocated femur are revealed. It is only after several years, when atrophy has rendered a limb sensibly shorter, that the lameness induces consecutive

spinal deviation. It must not be supposed that the organism possesses the power of effacing the effects of a congenital deformity; for, save in some very rare cases, if this be not combated, it persists and becomes aggravated.

Independently of the obstacles which may have their origin in the malformations of the bones or ligaments, and which may be easily deduced from a knowledge of the anatomical changes, we have to take into consideration the condition of the muscles. These may be tense or relaxed, contracted, inextensible, hypertrophied, paralyzed, or atrophied, while their direction may become changed, and their relations and action so inverted as to turn a muscle that is normally a flexor into an extensor. But although locomotion is compromised by very numerous causes, yet in the majority of cases the function is in some mode accomplished. In the vicinity of paralyzed muscles, others are found which have become more powerful, and which, to a certain extent, supply the place of such as are defective. Near a joint rendered immovable by a luxation, by a union of substance, or by muscular retraction, other articulations become more lax, and their movements more extended; and practice in the new motions sometimes imparts to the patients a remarkable dexterity in employing their defective limbs.

Etiology.—The first thing that strikes the observer is the influence of *hereditariness.* A remarkable case is given of hereditary absence of a phalanx of the fingers and toes. The grandfather of the subject of it presented the same defect. He had three children, who all inherited it. The eldest of these had three male children, who all exhibited it. The second, a female, had five children, two girls, who had the three phalanges, and three boys, who had only two. The third child, the father in the present case, had eleven children, of which number five girls had the normal number of phalanges, and six boys all wanted a phalanx on each finger and toe.

In an etiological point of view, we may separate congenital deformities of the joints into two groups; one of these consisting of those which are primary, original, or essential; and the others being purely symptomatic or consecutive to diseases which have occurred during intra-uterine life. The fœtal affections again which are capable of giving rise to these deformities of the joints, may be divided (1) into those which are seated in the articulations themselves; and (2) others which do not primarily influence the joints, but affect the bones, the fibrous tissues, or the muscles.

1. *Concerning the Fœtal Arthropathies* we have, as yet, but few illustrative cases, and these only apply to the *hip-joint.* Paletta and Parise each relate a case, in which the dislocation seemed to be due to a large increase of the fatty substance contained in the cotyloid cavity. In M. Guerin's collection, the dislocated head of a femur presents manifest signs of old inflammatory action. M. Parise recognises *hydarthrosis* as a cause of congenital luxation. Among 332 new-born infants, whose articulations he examined, he found articular dropsy in three; in two, there was a single dislocated hip; and in the third case the dislocation was double.

2. *Fœtal Diseases not affecting the Articular Surfaces.*—Diseased conditions of the *skeleton,* as seen in rickets, &c., may determine secondarily deviations or serious malformations of the joints. Others may arise from a contraction of the *ligaments* or *aponeuroses.* M. Robert has several times observed congenital lateral deviations of the fingers or great toe

combined with a shortened condition of the lateral ligaments. All observers have insisted upon the existence of the contraction of *muscles.* But while most of these have regarded this action of the muscles as primary and causative, they have done so with very different views. Guerin, studying congenital deformities on a large scale in correlation with monstrosities of the nervous system and convulsive affections of the fœtus, lays it down as a fundamental fact, that the convulsive muscular retraction or continued tension of the muscles is due to a lesion of the nervous system. Once established, this muscular retraction becomes permanent. At first the texture of the muscles affected by it undergoes no change, but continues as red, firm, and voluminous as in the normal state. This first stage is called by Guerin, *contraction.* But at a later period the muscles gradually undergo alteration, and what is called fibrous transformation takes place, constituting his second stage, or that of muscular *retraction* properly so called. Restricted to the explanation of some deformities, this theory seems to be justified by the due interpretation of facts; but it cannot be generalized without overlooking the import of other etiological circumstances. There are cases in which the deformity is due, not to muscular retraction, but to a true paralysis, which permits the action of those muscles to predominate, the motor power of which has been preserved. The doctrine of the influence of the pressure exerted upon the fœtus by the uterus in the production of deformities, admitted by Hippocrates, had become forgotten when M. Martin revived it in 1836. He regards clubfoot as produced by such compression brought into action by reason of the paucity of the liq. amnii—a doctrine evidently only applicable to a limited number of cases. M. Robert cannot agree with Cruveilhier in his explanation of the origin of such deformities by the compression exerted by certain parts of the fœtus on other parts of its own frame.

Diagnosis.—In a given deformity we have first to determine whether it be congenital, and next to establish the anatomical condition of the intra- and extra-articular elements. It is only when these points are determined, that we can declare our prognosis and establish our treatment.

1. The determination of the *congenital nature of the deformity* is arrived at, from the consideration of various circumstances—viz., (a.) *Its Age.* Easy as it is shortly after birth to determine this, it becomes more and more difficult as we depart from this point; both because the characters of the congenital nature of the affection become modified, and those which at first marked a deformity accidentally produced become lost. In certain cases, it is almost impossible to determine in an adult or old person, whether a certain deformity dates from intra-uterine life, or early infancy. Some traumatic lesions are, however, hardly ever met with in early life: and thus in a child presenting a luxation of the hip, we only hesitate in determining whether this is congenital, or the product of hip-joint disease, this last affection usually leaving more or less permanent traces.—(b.) *History.* If several members of the same family present the deformity, or even different anomalies, we have the strongest presumption of its congenital nature. The personal history of the patient himself is usually defective. A strong disposition prevails to conceal the original character of an infirmity, and to refer it to subsequent accidental causes; and when the deformity is old, and the indication false, error is often unavoidable.—

(c.) *Number.* Congenital deformities have a great tendency to appear in homological joints. In the majority of cases, when two club-feet or a double luxation of the femur exist, the affection is congenital. In rare cases the convulsive diseases of the young may give rise to these multiple deformities. Although this multiple character is a strong presumption of their being congenital, we cannot conclude inversely, that when the deformity is single it is rarely congenital, as facts would contradict the assertion.—(d.) *Co-existence of other anomalies of different organs.* One of the most convincing proofs of the congenital character of the deformity, is its coincidence with intra-uterine disease and fœtal anomalies. Thus the congenital character of a single club-foot was at once determined in a young man, fifteen years of age, by the co-existence of a slight spina bifida. In various of the cases quoted in the course of this work, deformities have been found in individuals who have presented shortening or junction of the phalanges, absence of bones, imperforate rectum, &c.—(e.) *Relative frequency and seat of the affection.* Congenital deformities are met with in much greater frequency in some joints than in others. Club-foot is met with more frequently than the deformities of all the other joints put together; and then comes luxation of the hip-joint. Next follow luxations or deviations of the shoulder, elbow, wrist, and knee, without our being able to determine their exact relative frequency. Finally, we possess a very small number of examples of deformities of the clavicle, the fingers, and the jaw. We know how rarely spinal deviation is met with in the uterus, and how often it occurs in youth; while with regard to dislocation of the femur the reverse of this is the truth.—(f.) *Varieties.* These are much more numerous in accidental than in congenital dislocation. Thus, there are at least six varieties of accidental luxation of the femur, while any other than that of outward and upward is scarcely ever observed in the congenital form. At the elbow, dislocation of the head of the radius is that which is almost alone met with. Among the varieties of club-foot, there are some which are very frequently congenital, as varus and talus, while others are more frequently acquired, as equinus and valgus.—(g.) *The external characters* offer little aid in distinguishing a congenital from an acquired luxation. The difficulty of diagnosis in some of these cases is well illustrated by a case which occurred to M. Nelaton, which he took for one of congenital luxation of the humerus, so closely did the symptoms resemble those described by Smith. At the autopsy it was found that no luxation at all was present, the appearances resulting from paralysis of the muscles of the shoulder and consecutive atrophy.—(h.) *Progress.* This in congenital deformity is usually slow, and a spontaneous cure is rare, especially after the first few years of life are passed. The condition of the functions is rarely advantageously modified, but oftener deteriorated. Organic or traumatic affections capable of inducing deformity are usually more rapid in their progress; and sometimes they are susceptible of an increasing amelioration, as is seen in unreduced traumatic luxation. Sometimes, on the other hand, the affection becomes rapidly worse, as is seen in certain spontaneous luxations, or deformities, consecutive to diseases of the joints.

2. *The anatomical diagnostic marks.*—The condition of both soft and hard parts differs so much, with respect to the age of the subject and indi-

vidual circumstances, that it is almost impossible to foresee the anatomical
changes that may be met with in particular cases. Diagnosis, too, may
be rendered difficult or even impossible by various circumstances, as the
small size of the bones, their being very deeply-seated, the interposition of
thick masses of soft parts, &c. Exploration is thus, *cæteris paribus,* ren-
dered more difficult in children than in adults, in persons who are stout
than in those who are thin, and in joints which are deep-seated and pos-
sessed of little mobility than in those which are in the opposite condition.
It would lead us too far to follow M. Robert in his detailed account of
the various parts composing the deformed articulation.

Prognosis.—This should be considered under three points of view, the
amount of functional lesion, the accidents likely to supervene, and the
chances of curability. Congenital deformities of the joints have occa-
sionally disappeared spontaneously. Fleischmann relates a case of gibbositas
which did so after six months' employment of the horizontal posture; and
even very well-marked club-foot has thus got well. Laugier relates a case
of a child suffering from talus, and M. Bouvier has seen recovery both in
talus and varus. Lateral deviations of the knee sometimes disappear in
the same manner. Such cases are, however, quite exceptional; and we
may lay it down as a general rule, that congenital deformities of the joints
left to themselves are persistent.

In considering the cases in which it is proper for surgery to interfere,
we must be guided by the pathological anatomy. In complete *anchylosis,*
the bold operations performed for the relief of the accidental anchylosis
of parts supplied with all their muscles, are inadmissible. Incomplete an-
chyloses are too little understood to admit of any formal opinion being given;
and M. Robert believes that they are often really false anchyloses, dependent
upon the retraction of muscles and ligaments. *Diastasis,* due to the incom-
plete development of two bones intended to be contiguous, and the *absence*
of the whole or part of a bone, must be considered as incurable conditions.
Deviations and dislocations offer the greatest chance of cure, the nearer the
period of birth they are taken in hand, when the bones are cartilaginous,
the ligaments flexible, and the muscles little altered in structure; and then,
if the displacement be not very considerable, we may always, with the
exception of some forms of these, expect either to cure, or very notably
relieve, the deformity. Articular deformities, at first easy of cure, may,
however, if too long neglected, at last prove refractory to all resources.
It is impossible to indicate the exact period at which such a transforma-
tion is accomplished; but it may be laid down as a general rule, that arti-
cular deformities become more speedily incurable when they are congenital,
than when they have been produced after birth, the changes in the parts
being more complete and more intimate. Thus, while a well-marked
example of varus with sub-luxation of the astragalus, is often incurable, if
congenital, at twenty or twenty-two; the same deformity accidentally pro-
duced may be advantageously treated at fifty, and beyond. In judging of
the curability, we have also to take into account the dynamic condition of
the muscles, as well as the extent and form of the displacement of the
bones. The muscle may be in a state of retraction or paralysis. In the
former condition, although in old cases it becomes atrophied, it does not,
except in very rare examples, undergo fibrous transformation; and there-

fore, when the obstacle due to the shortening of the muscles is removed, we may expect that it will recover its function. But in the case of paralysis we have no such hope; for we may act upon the shortened antagonist, and obtain the rectification of the limb; but this will remain motionless, and being left to itself, it will gradually return to its former state. So in regard to the form and extent of the displacement. Where this is a mere deviation in the arthrodia, or a slight sub-luxation, it is almost always curable; while complete luxation, which has been slowly produced, and is attended with extensive displacement, is generally beyond our resources.

The enarthroses receive separate consideration. While congenital dislocation of the *shoulder* has been too rarely observed to admit of any general conclusion being drawn, it is much otherwise with regard to that of the *hip*, which engages M. Robert's attention at considerable length. In discussing the question of its curability, he considers it in reference to the obstacles presented by the anatomical changes of the parts, and the clinical proofs adduced by M. Pravaz of its having been accomplished. The former of these he believes to be quite insuperable, preventing— even supposing the reduction could be obtained—any permanent effect resulting. The cases adduced by M. Pravaz are regarded by both M. Gerdy and M. Robert, as not being, in fact, reduction of the dislocation at all; while the degree of amelioration obtained was only producible by a long and painful procedure, and proved but temporary.

Treatment.—The statements already made show the importance of early treatment; but M. Robert agrees with Dupuytren and Bouvier, that this should be either undertaken shortly after birth, or delayed until after the completion of the first dentition. In treating these cases, our object is to re-establish the normal relations of the articular surfaces, by surmounting the various resistances offered by the forms or positions of the bones, and the rigidity of the muscular and fibrous tissues. To these we oppose either the action of pressure, or traction, or surgical operations. We cannot follow M. Robert in his judicious rules for the employment of these means, which, indeed, have nothing peculiar in them. He objects to the employment of *tenotomy* in any other manner than as an adjuvant to orthopædic procedures, and after these have been fully tried. In the case of several muscles suffering from retraction, he does not approve of the practice of those surgeons who recommend multiple divisions. Experience has amply shown that it often suffices to remove the resistance of the most powerful muscle or muscles, whilst the others eventually yield to orthopædic procedures. It is the more desirable to act thus, inasmuch as these last are usually the most deep-seated, the nearest the joints, and the most difficult to reach. In cases in which orthopædic apparatus is borne with difficulty, multiple divisions of the muscles may be required. But such cases are quite exceptional in early childhood, when the section of a small number of muscles is almost always sufficient.

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Art. VI.

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1. *On the Diseases of the Bladder and Prostate Gland.* By William
   Coulson, Surgeon to St. Mary's Hospital, &c.   Fourth Edition,
   revised and enlarged.—*London*, 1852.   8vo, pp. 485.

2. *A Practical Treatise on the Diseases and Injuries of the Urinary
   Bladder, the Prostate Gland, and the Urethra.*   By S. D. Gross, M.D.,
   Professor of Surgery in the University of Louisville, &c. &c.—*Phila-
   delphia*, 1851.   8vo, pp. 726.

Few persons who ever read a newspaper, can fail to know that Mr. Coulson
has written a book on ' The Diseases of the Prostate Gland and Bladder.' The
announcement, for years past, has rivalled that of Courtenay on ' Strictures
of the Urethra,' in the frequency with which it has met our eye in the
various journals—daily, weekly, quarterly. This book has now reached a
fourth edition; partly owing to the systematic manner in which it has
been advertised, partly owing to its intrinsic merits, but more than
either in consequence of the remarkable deficiency in this department of
our surgical literature. Dr. Gross is quite correct in asserting that all the
treatises on this subject as yet published in the English language, are
mere outlines, which no one has attempted to render at all worthy com-
panions to such works as those of Lawrence on ' Hernia,' Mackenzie on
the ' Diseases of the Eye,' Budd on the ' Liver,' and Curling on the
'Testis.' It has remained for an American writer to wipe away this reproach;
and so completely has the task been fulfilled, that we venture to predict
for Dr. Gross's treatise a permanent place in the literature of surgery,
worthy to rank with the best works of the present age. Not merely is
the matter good, but the getting-up of the volume is most creditable to
Trans-atlantic enterprise; the paper and print would do credit to a first-
rate London establishment; and the numerous wood-cuts which illustrate
it, demonstrate that America is making rapid advances in this department
of art. We have, indeed, unfeigned pleasure in congratulating all con-
cerned in this publication, on the result of their labours; and experience a
feeling something like what might animate a long-expectant husbandman,
who, oftentimes disappointed by the produce of a favourite field, is at last
agreeably surprised by a stately crop which may bear comparison with any
of its former rivals. The grounds of our high appreciation of the work
will be obvious as we proceed; and we doubt not that the present faci-
lities for obtaining American books will induce many of our readers to
verify our recommendation by their own perusal of it.

Professor Gross enters at great length, and with much clearness, into
the anatomy of the perinæum, bladder, prostate, and urethra; and also
adds a short chapter on the urine. Mr. Coulson's sketch of the normal
and abnormal states of the urine is larger and more elaborate, extending
to eighty-three pages; and exhibits care and attention in its composition.
We are compelled to remark, however, that the degree of attention
bestowed upon the different departments of the subject bears but little
relation to their relative importance; and that certain misprints of
proper names—such as Lecann for Lecanu, and Pattenkofer for Petten-

kofer, obtrude themselves disagreeably upon our notice. We have no inducement, however, to dwell on these introductory portions of either work; as they more properly belong to the departments of anatomy and of animal chemistry, and are almost entirely compilations from familiar treatises.

The first chapter in the division, "Diseases of the Urinary Organs," of the work of Professor Gross, treats of *malformations and imperfections* of the bladder; a term which we prefer to the one used by Rokitansky and by Mr. Coulson, "Abnormities of the Bladder." Mr. Coulson's chapter, "Abnormities of the Bladder," follows after those relating to the diseases of the viscus; but as the other arrangement seems the most natural, and is certainly the most convenient, we shall follow it on the present occasion.

Professor Gross considers the subject of malformations and imperfections of the bladder under the heads :—I. Absence of the Bladder ;—II. Bilobation or Multiplication of the Organ ;—III. Extrophy* or Congenital Eversion. Mr. Coulson, on the other hand, adopts no division whatever; but professing to include both congenital and acquired abnormities, he notices the sacculated bladder, an effect of disease, whilst he makes no mention of those interesting curiosities in whom the bladder has been found to be congenitally divided into different lobes or pouches. Complete *absence of the bladder* is very rare; but that there is no foundation for Mr. Coulson's statement, derived apparently from Rokitansky, that such a condition "must be accompanied with imperfect development of the kidneys, absence of the urethra, and defective development of other organs," is proved by the examination of the body of Abraham Clef, the account of which Professor Gross quotes from Binninger. In that instance, the bladder was totally wanting, the urine flowing from the kidneys directly through the ureters into the urethra; and although it is not stated whether the urine was discharged involuntarily and constantly, it is evident that the inconvenience was not extreme, as the patient had managed to conceal the infirmity both from his physician and his friends. Several curious cases of *congenital subdivision* of the bladder into two or more compartments, are brought together by Professor Gross; and he cites from the Philadelphia 'Medical Examiner,' for July 1850, a case recorded by Professor Johnson, in which a child, which died when eight weeks old, was found to have a sort of supernumerary bladder, in the form of a pouch, filled with urine, arising from the lower and back part of the bladder, at the place usually occupied by the right seminal vesicle, and attached by a narrow pedicle. *Exstrophy* or *extroversion* of the bladder is, in a practical point of view, the most interesting, and, at the same time, the commonest, of the malformations of this viscus. Deficiency in the anterior wall of the bladder, always associated with corresponding deficiency in the abdominal wall, and, where the fissure is at all extensive, with separation or partial absence of the pubic bones, is a condition productive of extreme discomfort and misery. Not only does the patient

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* We are somewhat perplexed with this term, which we suppose to be intended to represent the *Ecstrophe*, or *Exstrophia* (ἐξ, 'out of,' and στροφη, 'turning'), of M. Chaussier, who, we believe, was the inventor of it. In the form given to it by Prof. Gross, it would look as if derived from ἐξ, 'out of,' and τροφη, 'nourishment ;' which, we presume, is not his meaning.

suffer from the inconveniences of incontinence of urine, but also from the exposure of the vascular and sensitive surface of the protruding bladder, and from the loss of that support in progression which is afforded by the junction of the pubic bones in front. It is no wonder, then, that many propositions have been made for the purpose of relieving this distressing condition, and that operations of a very hazardous nature have been performed by surgeons at various times. For those who will not be content with the admirable apparatus devised by Mr. Earle, and described in Mr. M'Whinnie's interesting paper in the ' Medical Gazette,' there is the operation of establishing an artificial passage by seton from the bladder into the rectum. As far as we are aware, this has only been attempted twice; once by Mr. Lloyd, of St. Bartholomew's, when it proved fatal from inducing peritonitis; and once by Mr. Simon, of St. Thomas's, when it succeeded, the patient narrowly escaping with his life.

Wounds and Injuries of the Bladder.—Solutions of continuity in the bladder, from punctured or gun-shot wounds, are almost invariably fatal by the supervention of peritonitis; but a considerable period sometimes elapses before the symptoms set in. Dr. Gross proposes in such cases to open the abdomen and sponge-out the extravasated urine; a plan which, however hazardous, would, he conceives, be preferable to leaving the patient to die from the inevitable consequences of the presence of the irritating fluid in the peritoneal cavity. In civil life, we see the bladder punctured occasionally for the relief of retention, and with such results as demonstrate that there is no great hazard in a mere incision into the organ where it is uncovered by peritoneum. The most common form of rupture of the bladder, however, is from external violence, when the viscus is distended with urine. Such an accident is not necessarily fatal, as the readers of this journal are aware, from our report of Mr. Rynd's case in a recent number; but as it almost always occurs where the serous membrane covers it, the issue is generally unfavourable. Mr. Hird related an interesting case at the Medical Society of London, which is quoted by Mr. Coulson, to show that a patient may walk several miles after complete rupture, and for a time exhibit no symptom which attracts more than ordinary attention. Cases of spontaneous rupture from over-distension are rather examples of sloughing than of anything else; the openings being generally numerous and small, and the texture of the bladder softened. Rupture during labour may occur from manual violence, or from the pressure of the child's head, and is, on the whole, not a very uncommon accident; such cases ranking amongst the most distressing conditions with which we have to do.

The treatment of *vesical fistulæ* legitimately comes under this division of the subject, and is dwelt upon with considerable ability by Mr. Coulson. Professor Gross, however, is silent on this point; an omission of importance, which we hope hereafter to see supplied. The treatment is either palliative or radical. The former consists simply in the adaptation of a caoutchouc bottle to receive the urine, or in the introduction of a piece of sponge into the vagina; this last practice being often successful, as we can testify, in giving relief when all other means have failed. The radical and permanent cure of vesical fistula, is confessedly amongst the most difficult of surgical operations. All that can be said on the subject is thus summed up by Mr. Coulson:

"When the accident happens from laceration, without loss of substance, it has been stated to be cured by a catheter constantly retained in the urethra, with a bladder attached to its lower end, provided the treatment is commenced soon after delivery. By this contrivance Mr. Gaitskell effected a cure in a few weeks; but I cannot say from my own experience that I have ever known a case of spontaneous cure when the communication is once established. The constant passage of the urine constitutes a serious obstacle to its ever closing without surgical interference; and when a case comes before me in which there exists a simple fissure, I always recommend the performance of the operation of parting the edges and uniting them by some kind of suture, as soon as possible, before the parts have become accustomed to their altered state; before, too, the edges of the fissure have grown hard and callous. The chance of success is, under these circumstances, far better than when months have been allowed to pass in attempts which always proved unavailing.

"For my own part, I am convinced that with the assistance of chloroform, the anæsthetic properties of which place the patient in a condition of insensibility which materially aids the surgeon in the performance of this tedious and difficult operation, many patients may be cured of a calamity which otherwise renders life insupportable. No suffering is too severe, no peril too great, for a woman anxious again to be placed in a condition of health and comfort." (pp. 249, 250.)

We feel it due to American surgeons to state, that this subject has received more attention among them than it has in this country. Drs. Hayward, Pancoast, and Mettauer, have severally published methods of operating, of which Mr. Coulson takes no notice; and a very elaborate paper on the subject, by Dr. Sims, has just appeared in the 'American Journal of Medical Sciences' (Jan. 1852).

Acute Inflammation of the Bladder is, fortunately, a very rare disease; but rare as it is, Professor Gross has devoted a most instructive chapter to its consideration. Mr. Coulson discusses the subject in two imperfect chapters, respectively entitled " Acute Inflammation of the Mucous Membrane," and " Acute Inflammation of the Muscular Structure of the Bladder." Such a division of the subject appears to us unnecessary, and s₁ far unnatural, that it is one which can never be carried out in practice; and it is particularly worthy of note, that the authorities upon whom Mr. Coulson seems most to rely, and from whose works he quotes, are nearly unanimous in their conclusion that Cystitis is never confined exclusively to one coat of the bladder. We admire and appreciate, however, the subdivision adopted by Dr. Gross—viz., I. General Observations; II. Fibrinous Exudation of the Bladder; III. Suppuration and Abscess of the Bladder; IV. Gangrene of the Bladder; V. Ulceration of the Bladder.

It is rare for the whole bladder to be violently inflamed; and this becomes a matter of congratulation, when we consider the severity of the symptoms attendant upon an inflammation of even a circumscribed patch. The disease, most frequent in the neck and base of the bladder, takes the same course as enteritis; affecting the mucous membrane first, then the submucous tissue, and, finally, the muscular substance, rarely spreading to the peritoneal covering. The symptoms by which it announces its presence can hardly be mistaken, and must be actively met; for, according to Professor Gross, the inflammation rarely proceeds " beyond the sixth or eighth day without terminating in resolution, tending to suppuration, passing into gangrene, or assuming a chronic type." The treatment must, of course, vary with the exciting cause of the disease; but in the simple idiopathic affection, arising, for instance, from prolonged retention of urine,

Mr. Coulson, whilst recommending the abstraction of blood from the hypogastric region by cupping, or the application of leeches, says that commonly the loss of much blood cannot be borne.   In his subsequent remarks, we cannot help being struck with the fact, that although writing of *acute inflammation,* he seems to have had in his mind the *chronic* form of the disease.   Of the use of calomel, the only information that we can find is this : " Excepting at its commencement, mercury is not of use in this form of inflammation." (p. 150.)   " At the early stage," he says (p. 149), " the most valuable remedy is morphine, or opium (I prefer the former), given in sufficient doses to allay the pain about the bladder and along the urethra, as well as the frequent desire to pass urine.   These are the most distressing symptoms ; and if unmitigated, they soon wear out the strength of the patient ; but if even a few hours' intermission be obtained in the day, some chance may exist of recovery."   And then follows praise of anodyne injections and suppositories, condemnation of the practice of injecting oil and opium into the bladder, a caution against the unnecessary use of the catheter, and other vague statements, such as, " It is advantageous to employ counter-irritation above the pubes ; and the hip-bath at night will be found very serviceable."   " Infusion of diosma in the proportion of an ounce to a pint of water, small doses of copaiba and essential oil of cubebs, infusion of hops and the alkalies, will, all in their turn, be found useful."   Then a few words about diet, and then this sentence : " The prognosis of these cases is very unfavourable, if the ulcerative stage once sets in."   We can hardly suppose that, in the present state of our knowledge, our readers will consider such generalities as having the least claim to their attention, or the writer of them the smallest right to be considered an authority as to the treatment of acute cystitis.

In Professor Gross's hands the abstraction of blood meets with the highest approbation.   " I have repeatedly," says he (p. 125), " cut short by the lancet alone, attacks of the disease so severe as to leave the patient no rest, and so threatening as to induce the worst apprehension for his ultimate recovery."   If the patient be old and enfeebled, the lancet is to be used cautiously, and recourse must be had to cupping or leeches.   But, he continues,—

" Where the remedy is applicable, it should be employed, not only early in the disease, but to as great an extent as the system will bear.   In a word, we bleed here, as in other violent inflammatory affections, for effect, and not for ounces. As soon as the patient feels faint, the arm is tied up, to be reopened, in urgent cases, as soon as any tendency is perceived to a renewal of the original symptoms.

" But I would not restrict the employment of the lancet to the more severe forms of cystitis, or to such cases only as are accompanied by symptomatic excitement.   To do so would be to deprive the patient, in many instances, of a most powerful agent in combating what may be considered as the milder cases of this complaint.   There is a variety of cystitis, properly denominated acute, as it respects the local distress, in which there is an entire absence of constitutional disturbance, and yet the suffering is exceedingly severe.   In these cases there is no remedy, according to my experience, which is followed by such prompt and permanent relief as copious bleeding at the arm.   The operation rarely requires to be repeated, and is generally sufficient, with the aid of a gentle laxative and a dose of Dover's powder, to effect a cure in thirty-six or forty-eight hours ; sometimes, indeed, much sooner." (pp. 125, 126.)

When there is no biliary derangement, castor-oil or sulphate of magnesia, aided by an emollient enema, form the best means of clearing out the bowels; but we are cautioned to be exceedingly careful not to employ any drug which may have a tendency to irritate the lower bowel, and through it the urinary bladder. Of diaphoretics, the following combination is highly spoken of—namely, three grains of tartrate of antimony, forty or sixty drops of laudanum, one ounce and a half of Epsom salts, and eight ounces of water, with a sufficient quantity of white sugar to disguise the taste of the ingredients; a table spoonful is to be administered every three or four hours. When the cystitis depends on cold, gout, rheumatism, or irritation of the bowels, ten or fifteen grains of Dover's powder, three or four times in twenty-four hours, is very useful. In the use of demulcent drinks, care is to be taken, that while we allow a sufficient quantity to dilute the urine, we do not give so large a quantity as to necessitate frequent micturition. Diuretics are strictly to be avoided, unless the urine be very acrid and high-coloured, in which case a small quantity of nitrate of potassa or weak spirit of nitre may be employed; and in the gouty and rheumatic form of the malady, colchicum is sometimes beneficial. In the latter stages of the disease, Dr. Gross speaks favourably of an infusion of uva ursi and hops, in the proportion of one ounce of the former and half an ounce of the latter, to a quart of water; a wine-glassful of this, with or without fifteen or twenty grains of bicarbonate of soda, may be given five or six times a day.

Of the local remedies, besides leeches, cupping, and in severe cases the application of a large blister, followed by an emollient poultice, suppositories and opiate injections by the rectum are to be employed. A good pewter syringe, with a long nozzle, is far preferable, in Professor Gross's opinion, to all the patent contrivances for administering such enemata, of which he has any knowledge. Fomentations and hot bathing are eminently serviceable; but we are cautioned not to employ transient bathing, which only excites the patient, and does more harm than good. The immersion should be continued from twenty minutes to an hour.

When the cystitis depends on the presence of a calculus, no effort should be made to extract this, much less to break it up. The organ may for a time be overpowered by the intruder; but being accustomed to its presence, the irritation will at length subside. "To cut out the stone under such circumstances might prove fatal; to crush it, would be certain to be so." (p. 130.) When, however, the foreign body has been introduced from without, the rule, of course, is just the other way.

Cystitis caused by cantharides requires peculiar treatment. Dr. Gross says that a decoction of parsley-root and water-melon seeds is a popular remedy of great value; and mentions that Dr. Mulock, of Dublin, has derived great benefit from the administration of thirty drops of liquor potassæ every hour.

Cystitis, from gonorrhœa, is often benefited by the use of copaiba.

In gouty or rheumatic inflammation of the bladder, Professor Gross believes that a full dose of colchicum given at bed-time is preferable to small ones frequently repeated. In obstinate cases of this variety of cystitis, he considers calomel to be not merely useful, but almost indispensable. With respect to blisters, Professor Gross supports the opinion

of Desbois de Rochefort and others, that they are more useful than any other remedy, when the attack depends upon retrocedent gout or rheumatism. And when induced by the sudden repulsion of some cutaneous disease, as tetter, urticaria, or erysipelas, the same observation applies.

When stricture of the urethra has occasioned the inflammatory attack, we are to take every means of relieving the pre-existent affection; and as a general rule in cystitis, we are above all things to prevent the bladder from becoming distended. Finally, Professor Gross agrees with Mr. Coulson in condemning direct medication, as the injection of oil, and so on, into the bladder; and believes the practice likely to do more harm than good in acute cases.

Before dismissing the subject, it may be well to mention that the extracts from Mr. Coulson's treatise, which we have placed before our readers, are derived from the first of his chapters on Cystitis, that upon Inflammation of the Mucous Membrane of the Bladder. With regard to the acute inflammation of the Muscular coat, for which he makes a separate chapter, he confesses that "it is seldom, perhaps never, exclusively the seat of inflammation, if we except those cases in which abscesses form between the serous and muscular coverings" (p. 180); and of these he says, further on, that "perhaps it would be more correct to regard the areolar tissue, and not the muscular fibres, as the seat of inflammation; in short, to class the affection as we should class an abscess arising in any other situation." (p. 181.) After concluding his observations on chronic inflammation of the muscular structure of the bladder, Mr. Coulson also adds a few pages on inflammation of the peritoneal coat of the bladder, and of the subjacent cellular tissue; with the effect, among other things, of bringing the clumsy and inapplicable arrangement which he has adopted, through attempting to subdivide an indivisible subject, into prominent relief. He says that this inflammation, often the close of a fatal disease of the bladder, is seldom confined to the peritoneal coat and subjacent cellular tissue, but extends over the whole peritoneum. That form, however, of inflammation of the external covering of the bladder, which is connected with inflammation of the adjoining lining of the pelvis, requires a different plan of treatment, and is aggravated by abstraction of blood. Many of the symptoms attending it are periodic in their character, resembling a prolonged fit of ague. "The febrile symptoms, commonly so called, cease during the day, return with violence at night, and in the morning terminate with sweating." (p. 198.) The treatment at first consists in gentle local depletion by cupping or leeching, the mild employment of mercury, and the use of enemata and fomentations. When hectic symptoms show themselves, wine, strong broths, quinine with acid, bark, and similar tonics, must be resorted to.

Catarrh of the Bladder is also considered by Mr. Coulson under the heads, "Chronic Inflammation of the Mucous Membrane of the Bladder," and "Chronic Inflammation of the Muscular Structure of the Bladder;" but we cannot say that, in our opinion, any greater advantage here arises from this artificial distinction, than in the former case. Sir B. Brodie has written so well on this subject, that it would be unreasonable to expect any great degree of novelty in Dr. Gross's treatment of it; and although the chapter he has devoted to it is ably and completely written, we shall

confine ourselves entirely to the remedial suggestions which he makes. With Dr. Gross, as with many others, the pareira brava has not answered those expectations which Sir B. Brodie's encomiums would lead us to form. When given in infusion, he has found it to create nausea; but as an aqueous extract, combined with opium, morphia, or lupuline, it has proved more efficacious. Mr. Coulson seems to think it valuable when there is much pain and irritability of the bladder. Nearly the same observations apply to the buchu; but the uva ursi, in the form of infusion, meets with much praise from both the writers we are at present considering. Benzoic acid with Professor Gross, and the compound tincture of benzoin with Mr. Coulson, have often proved most valuable medicines; and the former states benzoic acid to be far more efficacious when combined with the balsam of copaiba—a drug which, as every one knows, is highly extolled, and often very beneficial, in many inflammatory affections of the urinary passages. Both our authors speak favourably of injections into the bladder; and in addition to the usual substances employed in such injections, Mr. Coulson makes mention of the injection of the balsam of copaiba, as advocated by Dr. Devergie; but he adds no personal testimony either for or against the practice. Cauterization with the solid nitrate of silver has been employed by Professor Gross, but not with the same results as have followed its employment by Lallemand; and in this particular he bears out our own experience as to cauterization for spermatorrhœa. Yet we cannot help thinking that in both instances the plan of this treatment is correct; and that the disappointment consists, not so much in any failure in its principle, as in the difficulty of its effectual execution. It must be a very difficult thing to apply the caustic to the whole internal surface of the bladder; and probably this is the explanation of the superior success which has attended its employment, in cases where the inflammation was chiefly confined to the neck of the viscus.

Dr. Gross narrates the history of a case in which Dr. Parker, of New York, made a lateral incision into the bladder, similar to that in the operation of lithotomy, the object being to afford a free passage from the secreted mucus, and " to put the organ thereby into a state of comparative repose." The case terminated fatally, although the patient derived considerable relief as to many of his symptoms.

Nervous Affections of the Bladder.—Irritability and neuralgia of the bladder have so many points in common, that few authors devote a distinct chapter to the latter affection; but Dr. Gross appears to have met with the disease so frequently in some of the malarious districts of America, as to render his remarks exceedingly valuable. Neuralgic affections appear to be common in the western states of America—occurring in the eye, stomach, bowels, uterus, spinal cord, testicle, urethra, and urinary bladder. Like neuralgia elsewhere, that of the bladder commences with undefined sensation of pain in the neighbourhood of the affected organ. Sometimes this pain is sharp and paroxysmal, sometimes dull and aching; but in whatever manner it displays itself, the pain is seldom confined to one spot, but extends to the rectum, anus, urethra, perineum, and middle of the thighs, and especially to the sacral and lumbar regions. It often comes on at a particular hour, gradually increasing, and then declining in intensity for a space of from two to six hours; and leaving behind it a distressing

sensation of soreness in the urinary passages. There is frequent desire to make water, and great difficulty in voiding it, the urine being thrown out in jets and in small quantity. Very often the symptoms are so like those attending stone, that the patient is repeatedly sounded, and sometimes cut. When death ensues, as it may do from the mere excess of suffering, the cause of the disease is left in the same obscurity as in other cases of neuralgia. The memory of the patient's sufferings is the only evidence of the disease—the pain has left no trace behind it—no guide for the future —no explanation of the past. It is only from the effects of treatment, that we can derive instruction or warning.

When accompanied with inflammatory symptoms, the treatment is obvious; and besides bloodletting, Dr. Gross thus speaks of the use of purgatives :

" From abundant experience in the treatment of neuralgic affections, as occurring in different parts of the body, I am convinced that a systematic course of purgation is not only unequivocally beneficial, but absolutely indispensable to a speedy and permanent cure. When the disease is complicated with amenorrhœa, the cathartic medicine should be combined with aloetic and emmenagogue preparations, with a view to their specific effects on the uterus." (p. 211.)

Carbonate of iron has been extensively employed by Dr. Gross, and with some success ; but he expresses his belief, that when neuralgia arises from malaria, quinine and arsenic are vastly superior to all steel preparations. After proper purgative medicine, with or without general bleeding, quinine is to be administered in four-grain doses, every three hours, until fifteen or twenty grains have been taken. It is then to be discontinued until the next day, when it is to be renewed, and the same quantity is to be taken in the twenty-four hours, when, in all probability, the paroxysms will have either ceased, or else abated very much in violence.

Larger and more frequent doses of quinine are objectionable, on account of the distress which is apt to be produced in the head. Dr. Gross then goes on to say,

" When the disease has been thus moderated or subdued, the best medicines to eradicate it are arsenic, strychnine, and aconite, in union with opium. The formula which I have been in the habit of using for many years past, both in vesical and other forms of neuralgia, is the following :

> ℞   Acid. arseniosi, gr. ij.
>   Strychninæ, gr. j.
>   Ext. aconite, gr. viij.
>   Pulv. opii, gr. v.—M.

" These ingredients should be incorporated with each other with the greatest care, and be divided into sixteen pills of equal size, of which one is to be administered every six hours, or four in the twenty-four hours. In some instances, the opium may be advantageously increased, or where it disagrees with the patient, it may be replaced by lupuline or hyoscyamus. When nausea ensues, the pills are to be used less frequently, or instead of giving one pill at a dose, only one-half or two-thirds of one should be employed at a time. Attention to this point is a matter of paramount importance, as it respects the benefit to be derived from this combination. Another rule is, not to continue the exhibition of the pills longer than a week or ten days at a time, to allow the stomach a short recess, when they are to be resumed, and taken as before. When administered with these precautions, arsenic, strychnine, and aconite seldom fail to produce a most favourable impression, and are often, of themselves, sufficient to effect a cure.

Where this is not the case, they should be employed along with other remedies, of which purgatives and the warm bath are amongst the most efficacious. With Fowler's solution of arsenic, so much vaunted in the treatment of this affection, I have but little experience; but I have given it sufficiently often to satisfy me that it is far inferior, in every respect, to arsenic in substance. It is more liable to nauseate, and does not exert the same controlling influence in arresting the disease.

" To moderate the violence of the paroxysm, large doses of narcotics are frequently indispensable. Of these the best are the salts of morphia, either alone or in combination with nauseants, according to the state of the vascular system. In some instances, where other means have failed to afford relief, I have derived the greatest benefit from the steady, persistent use of narcotics. I might, indeed, cite several cases in which the protracted exhibition of this class of remedies resulted in a radical cure. My rule is to give narcotics in full and sustained doses, taking care always previously to clear out the bowels, and restore the secretions. Where one article, or mode of exhibition, is found to disagree, another should be substituted. When the pain is very violent, or when narcotics cannot be taken by the mouth, opiate injections or suppositories should be used.

" An emetic of ipecacuanha, or tartrate of antimony, at the approach of the paroxysm, will sometimes have the effect of cutting it short, or materially abridging it. The remedy is particularly indicated when the disease is associated, as it often is in malarious districts, with gastric and biliary disorder.

" Much benefit may also accrue, in many cases, from the warm bath, or the application of steam to the affected part. This can be readily effected by connecting one end of a gum-elastic tube with the spout of a tea-kettle, filled with hot water, and placing the other under the bed-clothes. Fomentations with hops, opium, or laudanum will also be highly serviceable." (pp. 213, 214.)

The remaining observations on this interesting and, in this country at least, rare affection, though important, are too lengthy for transference to these pages. Four well detailed cases illustrate the preceding remarks. Of these we shall quote the first two; one of which will show the occasional obstinacy of this affection in its most severe form, whilst the other affords the more pleasing example of its cure.

" CASE 1.—One of the most interesting cases of neuralgia of the bladder of which I have any knowledge, occurred in my private practice in 1844. The patient was a coloured man about twenty-eight years of age, the property of Mr. McGruder, of the Pond Settlement, in the neighbourhood of Louisville. He had generally enjoyed good health until about two years prior to my visit to him. The prominent symptoms were, a frequent desire to pass his urine, and severe pain in the bladder, darting about in different directions, frequently paroxysmal in its character, and aggravated by exercise, the erect posture, and exposure to cold. His sufferings became at length so severe that he was obliged to abandon all out-of-door exercise, and confine himself strictly to the house. When I first saw him in the spring of 1844, he was compelled to void his urine every twenty or thirty minutes; the neuralgic pains were exceedingly violent, especially in the evening; his appetite was bad; the tongue was coated; the bowels were constipated; and his nights were usually spent without sleep. Although his sufferings were almost constant, he retained a good deal of flesh, and his countenance did not exhibit much trace of the local distress. The urine was generally somewhat acid, and of a light pale colour, with a slight increase of mucus. There was usually more or less scalding during micturition, and the discharge of the last drops of urine was always attended with spasm and tenesmus. The pains extended frequently along the spermatic cord, as far as the sacro-lumbar region, down the thighs, the perinæum, and even the testes, which were usually retracted, and exquisitely tender on pressure. The patient had never had gonorrhœa, syphilis, gout, or rheumatism;

and his habits had always been regular. He had been brought up in a malarious district, but had never suffered much from intermittent fever, nor has he ever had neuralgia in any other part of the body.

"Supposing the patient might have stone, I deemed it my duty, before putting him upon the use of anti-neuralgic remedies, to sound him; but, after the most careful search, found nothing. The operation was subsequently repeated several times with the same result. Giving him a dose of calomel, rhubarb, and jalap, to open his bowels freely, I enjoined a light diet, and ordered him to use, three times daily, a pill composed of the eighth of a grain of arsenious acid, one half that quantity of strychnine, and half a grain of extract of aconite, with three grains of quinine. The prescription was continued for a week, when it was temporarily omitted, on account of the disordered condition of the stomach. Meanwhile little impression was made upon the disease. As soon as the nausea had subsided, the use of the medicine was resumed, but in smaller quantity. In this way another week elapsed, and still the disease went on. Colchicum and morphia were now substituted, and under this combination, aided by the daily use of the hot bath and bicarbonate of soda and hop-tea, the symptoms improved, the patient began to have some appetite and sleep, and the micturition diminished considerably in frequency. The amelioration, however, was of short duration. In eight or ten days the symptoms were as violent as ever. The copaiba mixture was now directed, with the addition to each dose of three to five grains of benzoic acid. Under this prescription, which was continued for several weeks, no amendment was produced. Morphia, in large doses, both by the mouth and the rectum, was next tried, but with no other than transient relief. The original prescription was now resumed, and again used for several weeks with an occasional intermission. A large issue was also established in the sacro-lumbar region with the actual cautery, and the effect of the bougie, as recommended by Civiale, was tried. No relief followed. Finally, anodyne injections were thrown, at first, once, and afterwards twice a day, into the bladder; suppositories were also employed morning and evening; and, as a tonic, the patient was directed to take, three times a day, fifteen drops of the muriated tincture of iron in combination with a drachm of elixir of paregoric. In short, the treatment was varied in every possible form for five or six months, without the slightest permanent, or, in fact, even any decided temporary benefit. Becoming discouraged, the patient finally went home, where, after lingering for six or eight months longer, he sank under the effects of his ailments. No examination of the body was permitted.

"CASE 2.—A married woman, twenty-nine years of age, the mother of three children, visited me in 1846, for neuralgia of the bladder, under which she had laboured upwards of four years. She was tall and slender, with a pale, sallow complexion, and was of a nervous, excitable disposition. Soon after her second confinement, she observed a small swelling on the left nympha, a short distance from the orifice of the urethra, which, in time, became exquisitely tender, and gradually acquired the bulk of a pigeon's egg; it was of a red, florid colour, and of great firmness. About five months after it was first noticed, it began to pain her, especially late in the evening, so that she was unable to rest well at night; her appetite also declined, the bowels were costive, and she was obliged to void her urine six or eight times in the twenty-four hours. Micturition was attended with a scalding sensation in the urethra and the neck of the bladder, followed, in a few months, by dull, heavy, aching pains, which, at intervals, darted through these parts in different directions, as well as through the thighs, the groins, and perinæum. She also suffered severely in her back, as well as in the thighs and legs; and the little tumour was a source of incessant annoyance. During her last pregnancy, which was terminated about six months previously to her visit to me, her neuralgic pains were greatly augmented, both in frequency and violence; nor did her confinement bring with it any decided or permanent relief. She had formerly suffered from neuralgia of the facial nerves.

"Believing, from the history of the case, that the vesical affection was owing

to the tumour above described, I at once excised it, and then placed the patient upon a course of constitutional treatment, under which she rapidly recovered. Indeed, she had hardly any severe neuralgic pains after the operation. The remedies directed for her consisted chiefly of the compound calomel pill, and of the internal use, three times daily, of quinine and the aromatic wine of the citrate of iron, in the proportion of three grains of the former to a drachm and a half of the latter. Her strength and colour rapidly improved, the circulation of the extremities was restored, her spirits, which had been previously much depressed, became remarkably buoyant, and when she left Louisville, after a sojourn of three weeks, she seemed to be the most happy and delighted being in the world." (pp. 216—219.)

Paralysis of the Bladder.—We beg to present our readers with the following two quotations from Mr. Coulson's observations on this subject. Speaking of the consequences of retention of urine to other organs, he says:

"If, however, the bladder be allowed to remain distended, unnoticed by the surgeon, the urine, unable to find a free escape, is thrown back along the ureters against the kidneys, excites in the latter organs a kind of inflammatory disturbance of a sub-acute character, by which the cortical or secreting substance becomes granulated and adherent to the investing fibrous capsule, while the organ itself is partially absorbed. The elements of the blood mix with the urine, and hence we find albumen upon chemical analysis, and blood-discs upon microscopic investigation, at first in inconsiderable, and afterwards in very large quantity." (p. 113.)

This is a series of pathological phenomena so new to us, that we cannot but suspect it to exist only in Mr. Coulson's imagination; the fertility of which is also apparent in the far-fetched explanations contained in the latter part of the following extract:

"Authors have described paralysis of the bladder as a result of venereal excesses and of masturbation. The general debility which may result from such habits will affect this organ as well as other parts of the body; but, as far as my own professional experience goes, I should say that the usual result would be irritability of the bladder, dependent upon a morbidly sensitive condition of the mucous membrane of the urethra; for patients commonly complain of a tingling sensation at the glans penis, and of a constant desire to pass water, which is voided in very small quantities. Indeed, the smarting sensation at the end of the urethra becomes intolerable when a small quantity of urine is collected in the bladder. The only way in which excesses of the above description could affect the expulsive power of the bladder would be by indirectly favouring the development of some disease, such as tuberculosis; which, attacking the spinal cord, would excite in it changes similar to those with which we are familiar in the lungs—namely, destruction of the tissue by softening, or by inflammation of the surrounding parts and of the membranes which connect it with the vertebral canal." (p. 120.)

A clear and practical distinction can be established between paralysis of the neck of the bladder, and paralysis of the body of that organ; the first form being attended with incontinence, the second with retention. But we are apt to believe that this difference is hardly sufficiently attended to by many surgeons.

After an able division of the various forms of paralysis of the bladder, Dr. Gross goes on to speak of the treatment. He cautions the surgeon against evacuating the whole of the urine, accumulated in an over-distended bladder, at one time; as he is satisfied that he has seen several patients die from the severe depression induced by the sudden removal of the distension. In all such instances, he allows a small quantity of urine to remain in the bladder; and also applies a bandage to the abdomen, as after tapping

or parturition.   Cathartics he considers to be of the utmost service in all
descriptions of paralysis of the bladder; and of these, calomel, with castor
oil and turpentine, is his favourite.   Emetics also are not to be neglected.
Of remedies acting especially on the nervous system, strychnine, can-
tharides, and arnica, are the most potent, and are best given in the following
formula:—Strychnine $\frac{1}{16}$ of a grain, cantharides $\frac{1}{8}$ of a grain, arnica from
3 to 5 grains three times in the 24 hours.   Of arnica in paralysis of the
bladder from fever, masturbation, and general exhaustion, Dr. Gross speaks
very favourably.   He prefers the tincture in doses of from 40 to 60 drops
thrice daily.   The ergot of rye, much commended by some, has not sup-
ported its reputation in Dr. Gross's hands.   Counter-irritation, such as
a succession of blisters over the dorso-lumbar region, is useful in almost all
but the inflammatory form of vesical paralysis.   The actual cautery is also
a proper remedy.

The whole of this chapter of Dr. Gross's treatise is extremely valuable,
and bears the stamp of practical experience.

We are compelled by want of space to pass over the next hundred pages
of Professor Gross's work; and in so far as Mr. Coulson considers the
subjects in question, we must refrain from noticing his observations also.
It may give some idea, however, of Dr. Gross's comprehensive labours, if
we here enumerate the important subjects which we are thus obliged to
omit: Heterologous Formations of the Bladder: Scirrhus, Encephaloid,
Colloid and Melanosis—Tubercular Disease of the Bladder; Fungous,
Erectile, and other morbid growths of the Bladder—Worms of the Bladder,
Serous Cysts, and Hydatids—Fœtal Remains in the Bladder—Hair in the
Bladder—Air in the Bladder—Hæmorrhage of the Bladder—Retention of
Urine (an exceedingly well-written chapter)—Incontinence of Urine—
Hernia of the Bladder—and Urinary Deposits.   A large proportion of
these are either unnoticed by Mr. Coulson, or only very cursorily treated.
We should have taken the opportunity of pausing on the question of punc-
ture of the bladder; but that the recent discussion at the Royal Medico-
Chirurgical Society, and the approaching publication of Mr. Cock's paper,
will give us occasion to recur to it hereafter.

Mr. Coulson's chapter, entitled "The Chemistry of Urinary Concre-
tions," is succinct, able, and interesting; affording in the first place a good
sketch of our historical knowledge on the subject, and in the second place,
an account of the most recent investigations by Dr. Bence Jones and
others.   It is also exceedingly pleasing to us to state, that his observations
on stone in the bladder go a considerable way towards compensating for the
deficiencies we have felt it our duty to point out in the earlier part of the
treatise; and though they are defective in arrangement and completeness,
especially when compared with the labours of his American rival, they are
yet considerably above the average, and might pass unchallenged were it
not for Dr. Gross's publication.   It is needless to dwell upon the familiar
subject of calculus vesicæ; and we shall therefore content ourselves with
bringing into relief some of the more interesting peculiarities of Mr. Coul-
son's and Professor Gross's views.

Mr. Coulson points out, as favouring the formation of urinary concre-
tions round foreign bodies in the bladder, the irritation which they cause,
occasioning an increased secretion of mucus and agglutinating matter from

the walls of the viscus; and he enters at some length into the chemical questions involved in the changes of the urine occasioned by varying conditions of the general health.   He takes occasion to controvert some of Liebig's assertions, such as that rich living has no connexion with stone in the bladder; and makes the shrewd remark, that Liebig is more of a speculative than of a practical writer, and that it is impossible to adopt an opinion which is at variance with the general experience of the medical profession: and this gives rise to the thought in our minds, how little we really do know of the true causes of stone, notwithstanding the labour that has been bestowed upon the subject; and how constantly we find experience contradicting the most captivating theories.

This uncertainty is in great measure occasioned by the disgraceful condition of hospital statistics; and it appears to us, that it is quite useless to expect any rapid advance in our knowledge of this and other analogous subjects, until some general system of case-registration is instituted.   We have never been able to understand why the enormously rich royal hospitals of London do not devote a considerable department of their forces to the promotion of these inquiries; which cannot be considered complete, until they include a sort of registry of patients, as well as of cases.   It would not be a very difficult or expensive matter to establish regular communications with patients who have undergone important operations, whereby their subsequent history might be traced, and many doubtful points cleared up.   As things at present stand, almost every surgeon trusts to his own experience, no matter how limited, and rightly trusts it too, guided by the floating knowledge of the day, to determine disputed points. One man has met with unexpected success in performing the operation of tracheotomy for croup; another has done it a couple of times, and has failed; and henceforth he entertains the worst opinions of what the other pronounces safe and nearly unfailing. A surgeon accustomed to lithotomize, is persuaded to crush a stone, and from some untoward accident his patient dies; and forthwith the operation is concluded to be more tedious and dangerous than lithotomy. A hospital surgeon has secondary hæmorrhage after a flap-amputation—a circumstance " that in the whole course of his experience he never met with when he amputated by the circular method;" of course the vessels of the limb have been cut obliquely; and this being incidental to the operation, he never performs it again.   His younger colleague, however, likes the celerity of the flap, and whips off limbs with marvellous dexterity, until in an evil hour he wants to see if he cannot make as good a stump by the older and slower method.   Unfortunately, he has forgotten to raise the limb when he saws through the bone; and some months afterwards his patient comes back to him with a conical stump.   From this time the knot of pupils who follow his teachings turn up their noses at the very thought of a circular amputation; and so far as they are concerned, the question is settled for ever.   Examples such as these might be produced by the dozen, and will be continued to be produced, until some plan shall be concerted, whereby the after-history of patients may be traced when they have left the hospital and returned to their ordinary occupations.

Dr. Gross enumerates certain districts of America in which stone is much more common than in other parts; but remarks:

" The causes of these differences have not been ascertained; attempts have been made to trace them to the effects of climate, and to the influence of the water, food, and habits of the people, but without success. . . . . . .

" Coloured persons," says he, " appear to be remarkably exempt from calculous complaints. Whether this is the case in all countries where the negro resides I am not informed, but it is certain that the circumstance obtains, in an eminent degree, in the black population of the south-west. During a residence of ten years in Kentucky, I do not recollect to have met with a solitary example of gravel or stone in a coloured person. My impression is, that Dr. Dudley, in his large calculous practice, has never cut more than two or three individuals of this description. To what this immunity is due, our knowledge does not enable us to determine. The circumstance is so much the more surprising when it is remembered that the coloured people of that region are constantly exposed to hard labour, and that their fare is often of the coarsest character." (p. 343.)

He points out the employment of certain kinds of food as predisposing to stone in an indirect manner, and especially to the inordinate consumption of hot bread, biscuit, and pastry in the south-western states ; and refers to the practice of "bolting" the food in an almost unmasticated state, which travellers in America lead us to believe is one of the national peculiarities —we suppose we must not say *vices* of anything done in that enlightened republic.

Sounding occupies a deservedly conspicuous place in Professor Gross's treatise ; and the directions given are full and complete. Several years ago, we had several sounds constructed, which were much thinner and slighter near the handle than in the remainder of the instrument ; by which simple contrivance, the sound was capable of being moved about with much greater facility when in the bladder, than when of the same thickness throughout. A contracted " orificium urethræ " is a great obstacle to a thorough exploration of the bladder. Of the difficulties attending the discovery of a calculus, both our authors narrate several examples ; Professor Gross's remarks and cases being especially valuable, on account of the methodical arrangement which distinguishes his volume.

Chloroform is of great value in assisting the diagnosis ; and, in our opinion, should always be employed in children ; and even in adults, where there is much pain and irritability. Some surgeons object to the administration of anæsthetics, on the plea of the injury that may be done to the bladder without our patient giving any sign ; but this reasoning can only apply to surgeons who cannot trust their own tact and judgment, and require a monitor constantly whispering " care," " care." The case is different where the operation is that of crushing ; and here we agree with Dr. Gross, that, except in the case of children, it is better not to give chloroform.

" It is," says he, " of no little importance that the patient's mind should be perfectly clear, in order that he may promptly inform the surgeon of his suffering, should any arise, whether from too rough a manipulation, too great a size of the stone, or the seizure and inclusion of the mucous membrane of the bladder." (p. 418.)

In America lithotripsy is still in its infancy, few surgeons having made it a special object of study. Dr. Gross, however, not unfrequently performs it, and apparently prefers Jacobson's instrument, modified by Velpeau, to any other. Mr. Coulson's experience seems to be larger on this subject ; and his chapter is proportionally fuller and more complete.

Lithotomy.—The following are the chief points on which the opinions of the authors of these volumes seem to differ from those commonly entertained by the profession.

Mr. Coulson prefers to introduce the staff before tying up the patient; as it is much more easily done then, than when the thighs are flexed upon the abdomen. He also likes to entrust the staff entirely to his assistant, which can be done by depressing the handle a little, and to leave the operator free to protect the rectum and to guide the knife with the fore-finger of the left hand. This plan does not necessitate the altering of the position of the staff at all, after it has once been introduced; and although the groove of the staff is not so prominent in the perineum when the handle is inclined towards the ground, yet those who do not require the staff as a guide to the first incision, find no inconvenience from it. We quite agree with Mr. Coulson that the staff ought not to be allowed to alter one single natural relation of the parts to each other, which it must do if forcibly made prominent in the perineum; and we also agree in his recommendation to commence the first incision low, so as to avoid the artery of the bulb. After opening the membranous parts of the urethra, Mr. Coulson prefers to complete the operation " with a long straight knife, with a knob at its point,"—for what reason it would be difficult to say, except that it may glide easily along the groove of the staff.

Dr. Gross, on the other hand, is decidedly averse to the introduction of the staff before tying up the patient, " because it is productive of serious annoyance to the patient, and is almost sure to be followed by a premature escape of the urine.

" My rule," he observes, " therefore, always is, to tie the patient first, and immediately after to introduce the staff; taking care to confide it to a good intelligent assistant, one who is thoroughly acquainted with the anatomy of the pelvis, and the different steps of the operation. A poor staff-holder is a great curse; for he often excessively embarrasses the surgeon, and makes him commit blunders he might otherwise avoid. During the operation, the instrument is to be held perpendicularly, with the handle nearly at a right angle with the trunk, and inclined *slightly* towards the right side. The curved portion, securely lodged in the bladder, is hooked up closely against the pubic symphysis. The object of this advice is to prevent the instrument from pressing upon the rectum, which would thus be in danger of being wounded. By inclining the handle of the staff a little towards the right groin, the curved portion is made to bear against the left side of the perinæum, with the effect of rendering it somewhat prominent, and thereby facilitating the division of the membranous portion of the urethra." (pp. 428, 429.)

He also completes the operation with one instrument, such as is sometimes called a Liston-knife; and does not interfere with the staff after it has once been given to his assistant.

On the completion of the operation, Mr. Coulson says, that when the patient is put to bed, " the knees should always be kept raised, and the scrotum supported"; but Dr. Gross speaks as follows on this point:

" My experience has taught me that it matters little, if any, what posture the patient assumes after he has been put to bed. I usually, however, request him to lie on his right side for the first five or six hours, to afford the lips of the wound an opportunity of becoming glazed with lymph before he is obliged to urinate. At the end of this period, and, indeed, often much earlier, I permit him to rest upon his back, or upon either side, as may be most agreeable to him. Young subjects, unless they are incessantly watched, will seldom remain in the same posture beyond

a few minutes, and I must confess I have yet to see a case in which any detriment resulted from this source." (p. 465.)

He also gives minute practical directions for the proper construction of the bed, and of contrivances for keeping the patient dry and comfortable. The expedient of keeping a tube in the bladder for the urine to flow through, he condemns in strong terms; observing, that it can only be necessary in cases where the incisions, through design or inadvertence, have been unusually extensive; and that the contact of the tube with the surface of the bladder must necessarily tend to excite inflammation of the mucous membrane.

We do not consider it necessary to enter into any consideration of the modifications of the lateral operation of lithotomy. Of the quadrilateral operation, Dr. Gross merely observes, that it is one of those singular novelties of which there seems to be so much fondness in French surgeons, and that there is no probability that the operation will attract serious attention anywhere. As a sort of appendix to his chapter on stone in the bladder, he narrates, in a very pleasing, short, and practical style, the histories of 24 cases of lithotomy, which he hopes may "prove interesting, not only to young lithotomists, but perhaps, also, to those of riper experience." (p. 472.) We are tempted to extract the subjoined graphic account of a difficult case of the lateral operation, which may both interest and instruct the reader.

"The following case illustrates some of the difficulties which sometimes occur in lithotomy, when the stone is of great size. I may mention that the operator was young and inexperienced, and that the calculus might possibly have been extracted through the perinæum, without recourse to the supra-pubic incision. The case was doubtless well calculated to perplex an older surgeon; it was a first one, and may be compared, in its effects upon the mind, to a first case of labour, with a breech, arm, or shoulder presentation. The young gentleman, in announcing his troubles, writes, 'I am in a 'bad box,' the worst kind of a 'bad box;' I have operated recently for stone, through the perinæum, passed the scalpel into the bladder, with little difficulty. The knife, on entering the viscus, struck against the calculus. I now introduced my finger, and touched the stone, which I found to be of enormous size, filling up the whole cavity of the bladder, which was firmly and spasmodically contracted upon the foreign body. Finding the incision in the neck of the bladder too small, I enlarged it to nearly two inches. I then introduced the forceps, but could not carry them into the bladder, much less expand them over the stone. I made many attempts to move the stone and change its position, but it was so large, and so exceedingly rough, that this could not be done. I now enlarged the wound in all directions to the greatest possible extent, and then renewed my efforts at extraction, but was again completely foiled. Whenever I touched the stone, it produced the most severe vesical spasm and bearing-down pains, similar to those of parturition. Finding all attempts to remove the stone of no avail, I was indeed greatly at a loss. The case was now just this. I had cut down to the stone, but found it impossible to extract it. The awful condition of the wretched man was before me with all its horrors. I would have given half my existence for a consultation.'

"In this quandary, the operator, rather than abandon his patient to his fate, proceeded to open the bladder above the pubes. Having reached the organ, he placed his finger upon it, and found it hard, and firmly contracted upon the stone. An incision, upwards of two inches in length, was then made into it, when, introducing a finger, he succeeded, though not without difficulty, in raising the small end of the stone into the wound. 'I now,' says he, 'seized it with the forceps, and attempted to extract it, but failed. I then tried to break it, but in this also I

failed. Finally, I tried again with my finger, and after some difficulty succeeded in removing a calculus of a pyriform figure, weighing eight ounces, and measuring nine inches and a quarter in its greater, and seven inches and a half in its lesser circumference, by three inches and a half in diameter.'

" Notwithstanding two such large wounds, and the violence used in extracting the stone, the patient did remarkably well, and finally recovered. On the eighteenth day after the operation, he was able to walk about his room, and was gaining flesh and strength, both wounds healing kindly and even rapidly. The only inconvenience which he experienced was from a communication between the bladder and the rectum, caused by carrying the knife too far back in the attempt to enlarge the incision in the neck of the bladder. My correspondent closes his letter in the following expressive words : ' Mr. W—— is a very stout, tough kind of a man; I do not believe that thunder and lightning could kill him.' The experience thus bought has not been without benefit ; instead of intimidating my friend, he has resolutely pursued his course, and has acquired no little reputation in his neighbourhood as a lithotomist. The only error, perhaps, which he committed, was, that he did not attempt to break the calculus, and extract it piecemeal. Such an attempt, however, might have proved fruitless, when we reflect upon the fact, previously adverted to, that he found it impossible, in consequence of the large size of the foreign body, to carry the forceps into the bladder, and to expand the blades over the calculus." (pp. 444—446.)

DISEASES AND INJURIES OF THE PROSTATE GLAND.

Acute Inflammation.—Amongst the exciting causes of prostatitis, it is a disputed point whether the various gum-resins employed in the treatment of gonorrhœa have or have not the power of causing the gland to inflame. Upon this question Mr. Coulson only makes the following statement:— " The internal use of the gum-resins and astringent injections employed in the treatment of gonorrhœa are among the exciting causes of prostatitis." (p. 419.) Professor Gross, on the other hand, holds a different opinion.

" Velpeau asserts that stimulating diuretics, as copaiba and cubebs, employed in the treatment of gonorrhœa, are apt to produce it. My own opinion is, that such a result rarely, if ever, follows the exhibition of these medicines; I have constantly used them for the last twenty years, in large as well as in small doses, and in every stage of the malady, and yet I cannot recall to my recollection a solitary instance in which they appeared to exert any pernicious effect upon the prostate. I am not aware, moreover, that the statement of the French surgeon has been verified by the experience of others equally entitled to credence. That the internal use of cantharides, which act specifically upon the neck of the bladder, stimulating injections, and caustic applications, may occasionally produce inflammation of this gland, is indisputable." (p. 526.)

Be this, however, as it may, our authors agree in the appropriate treatment to be employed, and dwell particularly on the necessity of copious depletion. We have before expressed, in our review of Mr. Adam's treatise on the ' Diseases of the Prostate Gland,' our entire dissent from this the ordinary method of treating the disease; and as we then stated the reasons for our opinion, we shall not now dwell upon the point. With respect to local depletion, a proceeding to our mind very like that of leeching an inflamed cervical gland, Mr. Coulson observes :

"Mr. William Craig (' Lancet,' vol. ii., Session 1840–41, pp. 299, 300) relates a case of diseased prostate in which he employed a particular method of applying leeches directly to the gland, differing both from those of Henderson and Bejin. It consists of a tube into which is fitted a piece of wood, with a handle at one end,

while the other terminates conically in a blunt point, for the purpose of gradually dilating the rectum. When this is effected, the wooden dilator is to be withdrawn, and a box of proper size to fit the tube, and capable of holding three or four leeches, with a piece of wire to form a handle fixed into the bottom of it, is to be pushed through the tube.

"It is necessary to lubricate the wood and the tube with oil previous to using it, and in introducing it, it should be directed towards the rectum in order to prevent its coming in contact with the tender prostate." (p. 422.)

He also speaks of the use of the warm bath thus: "Hot bathing generally, as well as locally, gives relief." (p. 423.) Professor Gross enters more at length into the subject, and subjoins the following sensible observations:

"The kind of bath is an object of no little importance in the management of this disease. The hip-bath is the one usually recommended; but I am satisfied that its beneficial effects are frequently more than counterbalanced by the inconvenience which attends its administration. To be at all efficacious in relaxing the cutaneous exhalants, it is necessary that the immersion should be continued at least from twenty-five to forty-five minutes; a period which must inevitably lead to great fatigue, to say nothing of the afflux of blood that is likely to take place to the inflamed organ from the peculiar position of the trunk. It is for these reasons that I seldom resort to this agent in the treatment of acute disease either of the prostate or the urinary bladder. All the good effects that can be desired in such cases may be readily obtained from the steam-bath, prepared either by conducting the vapour of hot water to the body of the patient from a tea-kettle, or by placing near him, under the bed-clothes, a few hot bricks, wrapped up in flannels previously moistened with vinegar-and-water. By either contrivance, free diaphoresis may generally be induced in a few minutes." (pp. 528, 529.)

With respect to abscess of the prostate, Professor Gross truly observes, that there is nothing in the affection to exempt it from the ordinary treatment of phlegmonous abscess; and recommends an early evacuation of the matter through an incision with a long straight knife in the perinæum. Mr. Coulson concurs in the same recommendation, but expresses himself much more cursorily and unsatisfactorily. As we could hardly select any portion of the two works which more strikingly illustrates the style of the respective authors,—Professor Gross's painstaking, minute, and thoroughly practical instructions, and Mr. Coulson's slip-shod, vague assertions,—we shall cite from each the paragraphs relating to this.

"The second indication to be fulfilled is the opening of the abscess; and the question therefore arises, are we justifiable in doing this? Not a few practitioners are of opinion that such collections should always be permitted to pursue their own course, on account of the uncertainty of distinguishing them and the difficulty of reaching them with the knife. I cannot agree in the propriety of this advice. As long as the matter is pent up, the part is unrelieved, and the abscess has a tendency to increase and produce further mischief; nay, its contents may burrow extensively among the adjacent structures, doing great injury not only to them but also to the prostate, and finally, perhaps, escaping into the pelvic cavity; an event certain to be followed by fatal peritonitis. It is absurd to look upon an abscess of the prostate as a peculiar affection, for it does not differ from a phlegmonous abscess in any other part of the body, except by its situation, and there is nothing in this, I conceive, that should exempt it from ordinary treatment. The rule, therefore, which should be adopted in all cases of this disease, is to anticipate nature by an artificial opening, instead of allowing her to pursue her own wayward course; a course which is frequently tedious, ill-directed, and inadequate.

"It has been already stated that the most favourable route for the escape of the

matter is through the perinæum, and hence, whenever it points in this direction, no time should be lost in furnishing it an outlet. For this purpose a long, straight, narrow-pointed bistoury is much preferable to an abscess lancet, which is not only unsteady in the handle but too short in the blade, even if it be pushed up as far as its shoulders. The incision should be made in the most prominent part of the swelling, and care should be taken, on the one hand, to avoid the rectum, and, on the other, the urethra and urinary bladder. It must be quite free, and made as dependent as possible. A small tent may be retained in the track for a few days to prevent premature closure.

" When the abscess points in the rectum, as will be indicated by the large size and fluctuating character of the swelling, it may be readily reached with a curved trocar, four or five inches long. The patient is placed as in the operation of lithotomy, and the left index and middle fingers, well oiled, are carried up the bowel until they come in contact with the most prominent part of the abscess. The trocar, concealed within its canula, is then placed in the groove formed by the junction of the two fingers, and as soon as it has reached its destination, it is thrust into the swelling, and immediately withdrawn, at the same time that the canula is pushed farther in. When the matter is discharged, the instrument is removed, and the case is treated upon general principles. For some days after the operation the lower bowel should be kept as quiescent as possible.

" When the abscess bulges inwards towards the urethra and the neck of the bladder, it may be punctured with a common silver catheter, carried down in the usual way, and moved about in different directions, as in searching for a urinary calculus. Or, instead of this, a sound with a conical beak and a small curve, may be used, and this, on the whole, is preferable, inasmuch as it can be made to pierce the abscess with more facility. The slightest pressure frequently suffices to effect our object. When the abscess is not yet completely matured, and the local suffering is such as to render delay improper, the operation may be executed with the lancetted stylet used for dividing strictures of the urethra. When, by any of these procedures, the matter has been evacuated, the urine should be frequently drawn off with the catheter, to prevent its entrance and sojourn in the interior of the sac ; an occurrence not only productive of exquisite pain and spasm, but liable to be followed by the worst consequences as it respects the recovery of the affected gland. When the parts are tolerant of the presence of the instrument, it may be permanently retained in the urinary passages, until all danger from the above cause is past.

" When the abscess is of a scrofulous character, as indicated by the nature of the pus, the system should be subjected to the influence of iodine and tonics." (Professor Gross, pp. 536, 537.)

" If an abscess forms, it is desirable that the matter should find its way neither into the rectum, the urethra, nor behind the bladder, which may be fatal, but should pass to the surface, in order to discharge its contents more freely, and heal more readily. We must not, therefore, hesitate, when fluctuation can be discovered, to make a puncture with a lancet, without waiting for the matter to present at the surface.

" Should the disease have anticipated the operator, and the abscess have opened into the perinæum or the rectum, nothing can be done beyond maintaining the general health. Should the abscess have burst into the urethra, or at the neck of the bladder, it might be better to allow a flexible catheter to be retained in the canal, until there is reason to believe the abscess is healed, than to practise its frequent introduction. Most commonly the matter gradually diminishes in quantity, and ceases after a time. In other instances the discharge disappears for a time, and then comes on, and this may occur several times before the patient completely recovers." (Mr. Coulson, p. 424.)

Professor Gross also devotes a chapter to the topic subject of *Ulceration of the Prostate Gland.* The affection, however, which is most

important to be rightly understood, is of course the *Chronic Enlargement of the Gland,* and to that subject we shall at once pass. With respect to the seat of the disease, Dr. Gross denies that the left lobe is more frequently affected than the right; and says, that no reason, anatomical, physiological, or pathological, can be assigned for the occurrence, supposing it to exist. We incline, however, to agree with Mr. Coulson in the correctness of Sir Everard Home's observation; in further illustration of which we may remark, that many diseases are more common on the left than on the right side; as, for instance, hernia. Neither of our authors countenances the idea, that chronic enlargement of the prostate is an invariable accompaniment of advancing years. On the contrary, Mr. Coulson says that the condition in question is truly a diseased one, and that if no morbid change be going on, the prostate gland shares in the rule affecting other organs in old men, and decreases in size. Dr. Gross says, however, that the prostate gland is alone an exception to this rule; and that it commonly manifests a tendency to exceed the limits assigned to it by nature. He sensibly remarks, however, that "old" is a relative term; one man being old, and exhibiting all the signs of age, at forty, and another being still young at sixty.

Remedies influence the hypertrophied prostate very little. Mr. Coulson has found cicuta useful, but then he combines it with the iodide of potassium. Dr. Gross recommends a trial of the hydrochlorate of ammonia, either alone in doses of fifteen or twenty grains, three or four times a day, or combined with a small portion of tartrate of antimony. He also speaks of the proposition of excising the prostate gland.

"*Excision* of the prostate has been recommended. The operation is spoken of by some of the older surgeons, but has not, so far as I know, received the sanction of any of the moderns. It does not appear that any one has really ever had the hardihood or folly to perform it, and this is no doubt the best thing that can be said in commendation of it. The idea of extirpating the entire gland is, indeed, too absurd to be seriously entertained. Such an operation, even supposing it were practicable, and that the patient could survive it, would be far worse than the disease; for it would inevitably lead to the formation of an incurable fistula, rendering life utterly miserable. Excision of the middle lobe would be less objectionable, and might, in fact, be resorted to with a fair prospect of success, in all cases in which this body forms a permanent obstacle to the passage of the urine. When it is attached by a narrow footstalk, the operation could hardly fail, and might afford the only chance of relief. I should certainly myself prefer it, under such circumstances, to the operation of 'crushing,' recommended by some of the French surgeons, and to the puncture of the bladder above the pubes. I should not even expect much difficulty in the execution of it. The position of the patient and the incisions in the perinæum would have to be the same as in the lateral operation of lithotomy. The enlarged lobe might be easily cut off at its base with a pair of stout, probe-pointed scissors, curved on the flat; or it might be twisted off with a polypus-forceps." (p. 573.)

We do not find anything particularly worthy of remark in the subsequent consideration given in either treatise to the more uncommon affections of the prostate gland—as cancer, tubercle, atrophy, and so on; or even in the chapter, "Calculi of the Prostate."

Here we part company with Mr. Coulson, whose work does not extend to the subject of diseases of the urethra; and we shall also reserve our remarks upon that part of Dr. Gross's treatise which treats of the "Urethra,"

as we shall be better enabled to consider it in conjunction with one or two other works which we have lately received. We have already expressed our high appreciation of Professor Gross's treatise; and it gives us much regret to feel compelled to state that the comparison of the two is, on almost every point, disadvantageous to Mr. Coulson. We acknowledge that we have tried his work by a high standard; but we think that we are fully justified in so doing, considering how long its author has been before the public as a surgical writer, and how high a position he evidently desires to attain. There is scarcely any point on which his work is not inferior to Dr. Gross's; and we have been continually led to feel, in our examination of it, how seriously defective it is, alike in method and in completeness; so that its perusal has left us with the conviction, that scarcely any one subject has been satisfactorily treated, scarcely any point of pathology or practice fairly and fully disposed of. We can assure Mr. Coulson that we state this with regret, and with no other motive than that which arises from our desire to discharge our critical duty faithfully and impartially. That our verdict will be confirmed by any competent judge, who may take the same pains that we have done in the comparison of the two works before us, we have not the smallest doubt; and we have even a sufficiently good opinion of Mr. Coulson's own candour, to believe, that if he will examine Professor Gross's treatise for himself, he will admit our case to be so strong, that he will not feel it necessary to impute to us any bias to his disadvantage.

ART. VII.

Neuralgia; its various Forms, Pathology, and Treatment: being the Jacksonian Prize Essay of the Royal College of Surgeons for 1850, *with some Additions.* By C. TOOGOOD DOWNING, M.D., M.R.C.S.— *London,* 1851. 8vo, pp. 375.

A PRIZE-ESSAY always excites large expectations of completeness. The critic looks for at least a thorough analytical review of the literature of the subject, and a collocation and arrangement of the facts accumulated therein; and if it be a purely *scientific* question, he anticipates also a weighing of modern views, and a trial of them by experimental research; whilst if it be a *practical* question, he, with reason, hopes to have it thoroughly elucidated, not only by bibliographical inquiries into accumulated experience, and by scientific research, but also by enlightened observation, based on a thorough knowledge of anatomy and physiology. It is, indeed, by the latter alone, that we can arrive at any sound advance in practical knowledge, whether as to the pathology, diagnosis, or treatment of disease. Impressed with this conviction, at a very early period of his life, Sir B. C. Brodie (to whom this prize-essay is dedicated " by express permission") enriched physiological science by his researches, and, at the same time, so disciplined his own mind, as to lay the foundation for the very honourable and successful professional career he has completed. Now, we put it to Sir B. C. Brodie to say, whether this essay is such an one as we have indicated that a prize-essay ought to be, and as his own example shows that it should be? We ask Sir Benjamin, whether the range of reading

displayed in this volume is in *any* degree commensurate with the literature of the subject?—whether there are any proofs that the author has analysed that literature, and meditated on its facts?—whether the anatomical and physiological knowledge of the nervous system therein displayed is such as he thinks creditable to the Royal College of Surgeons of England?—whether the cases recorded by the prize-man are recorded as they would have been by an observer thoroughly imbued with a knowledge of the literature, the pathology, and the physiology of his subject?—and whether, finally, the literary execution of the work, as regards the grammatical construction of the sentences, the selection of words and phrases, and the general arrangement of the whole, is such as he can approve?

Our readers will sympathise with us in the painful position in which we are placed; for we have either to express and establish opinions as to the merits of this essay, altogether opposed to the opinion virtually expressed by Sir B. C. Brodie and the adjudicators named by the Council of the Royal College of Surgeons of England, or we have to let it go forth to the professional public abroad as a true reflex of the science and literary acquirements of a large section of the medical profession in England—the large body of English surgeons. In the one case we have to act in most painful contrariety to our feelings and inclination; in the other, in opposition to our convictions, and to our sense of duty to our professional brethren and to English medical literature. We cannot hesitate, however, as to the course of conduct we ought to adopt. Our duty must be preferred to our own peace, or to the gratification we should have felt in adding our commendations to those of the adjudicators of the prize; but we withhold our approval with the greatest reluctance, and shall feel it incumbent on us to enter largely into the reasons which have influenced us to call in question the *imprimatur* of so eminent an authority as the Council of the *Royal College of Surgeons of England.*

Neuralgia, as its name implies, is a disease of a nerve, the principal or leading symptom of which is agonizing pain—*pain* emphatically. Nerves vary, however, in their endowments; of those which are sensitive to painful impressions, all are not to the same, or even to similar impressions. Some are nerves which influence the secretions, and when these are affected with neuralgia, increased secretion results; others are excitor, and, when the seat of neuralgia, increased or irregular muscular acts take place. These results differ also, as the affected nerves thus act upon muscles of voluntary or of involuntary motion, or are nerves of common or of special sensation. The property of common sensation is placed in the nerves as an aid to the *vis conservatrix.* Pain is the sentinel of the organism, its occurrence indicates danger to the well-being of the latter, and it excites to efforts which are defensive and conservative. Impressions caused by injurious agencies excite certain changes in the molecular structure of the extreme point of the nerve-fibril, which are transmitted from molecule to molecule along the whole track of the fibril, in its course to the sensorium; reaching the latter, the sensation termed pain is felt, and a re-action in the central or cerebro-spinal axis follows thereupon, adapted to remove the injurious agency, or the impressions caused thereby. Each nerve having its own proper sensibility to special impressions, and reacting only within fixed natural limits, cannot be said to be affected with neuralgia so long as it

responds in no other way to impressions than normally, and to those impressions alone to which it is appropriate; but if, from any cause, whether known or unknown, the nerve-fibril in any part of its course, or that part of the sensorium in which it ends, undergoes a change such that impressions ordinarily painless excite pain, or such that the molecular change is induced which follows upon painful impressions, although no perceptible impression has been made on the sensorial end of the nerve-fibril, then the phenomena are said to be neuralgic, and the morbid condition upon which they depend is termed neuralgia.

In general pathology, *pain* is a leading phenomenon, because very few morbid changes take place in the tissues, which do not involve the sensorial end of the nerve-fibrils. These morbid changes are, however, never considered as the causes of neuralgia, nor is the pain termed neuralgic; in this way Dr. Downing's observation is correct, that " the term is now generally understood to apply to all those painful disorders which are apparently unconnected with inflammation or recognisable lesion of a part;" and if his nosology and pathology had been duly kept in accordance with this generally-acknowledged view, we should have had no critical remarks to make upon his definition and arrangement. It is evident, however, that he has had no fixed idea in his own mind, as to what is or is not neuralgia. In the same opening paragraph from which we have just quoted, neuralgia is, in one sentence, described " as a more or less violent thrilling agony, occurring in paroxysms, and shooting along the course of a nerve at variable intervals;" and then, in the next, it is stated to be (the italicized words are the author's) " a disorder or *pain of a well-known nerve.*" What meaning is attached to the word *well-known* by Dr. Downing is not obvious; but if we adopt the definition with the meaning ordinarily attached to the words, we are at a loss to understand why Dr. Downing labours to prove that *rheumatic* neuralgia is a " spurious" form; or why " *spasmodic* neuralgia" is the genuine form, any more than the " *hysterical* neuralgia." Be this as it may, he arranges the neuralgiæ into these three classes, the rheumatic, hysterical, and spasmodic, observing, regarding them,

" These are the principal classes of neuralgic affections, to one or other of which the cases met with in practice may generally be referred. There may possibly be other varieties, determined by constitutional causes, as the character of *these* is mainly dependent upon the prevailing diathesis.

" But it would be erroneous to suppose that every case can be placed, clearly and distinctly, under one or other of these heads. Sometimes neuralgic affections are of a mixed character, and partake so much of the qualities of the different species, that it is difficult to say to which they belong. It is possible, also, that they may co-exist, or gradually merge from one into the other. This will readily be conceded, when it is borne in mind, that the symptoms vary continually, both in type and character, at different periods. The advantage of attending to these distinctions cannot be too much insisted on. Not only do we thus gain a clue to the solution of many difficulties connected with the subject, but can understand why remedies should be so powerful in some cases, but totally inoperative in others. By the greater number of writers, not excepting the continental, all instances have been confounded together under one common title, and treated accordingly." (pp. 26, 27.)

One cannot but admire the easy style of condemnation with which Dr. Downing criticises " the greater number of writers, not excepting the

continental," when there is abundant internal evidence that he has hardly read half a dozen. Valleix's and Rowland's books have evidently supplied Dr. Downing with references, and helped him in the arrangement of his subject; he has also drawn largely upon Dr. Rowland's work for facts, and he might have very well added, that Dr. Rowland distinguished the rheumatic form. On looking into other writers he will find how mistaken he is. Dr. Elliotson very plainly remarks, " But there is a kind of neuralgia which is decidedly a rheumatic pain in the nerves, which arises clearly from cold, and is nothing more than rheumatism affecting the nerves. . . . . . There is a kind of rheumatism which affects the nerves, therefore there is a kind of neuralgia which is rheumatic."* As explicitly, also, Dr. Watson; we add M. Sandras on the European continent,† who notices the *gouty* also, with numerous other writers, as Romberg, of Berlin,‡ and our own Dr. Copland, who particularly mentions as a practical point, that "the connexion of these affections with *gout, rheumatism,* hysteria, &c., should not be overlooked." § The *gouty* is a form of neuralgia evidently not at all comprehended by Dr. Downing. Or if we pass to the American continent, we might quote Dr. George B. Wood, of the University of Pennsylvania, who observes, " But probably the most frequent of these general sources of neuralgia is the gouty or rheumatic tendency; and cures may very often be effected by remedies applied in accordance with this view. The physician should never allow a case of neuralgia to pass from under his observation without having satisfactorily investigated its relations to gout and rheumatism." ||

If we turn to the literature of neuralgia, with reference to the *hysterical* form, we shall find that there is hardly a writer on hysteria who does not most distinctly recognise the difference between the hysterical neuralgiæ and the spasmodic, or true tic douloureux. We need not, however, dwell on this point, as we have amply demonstrated the incorrectness of his literary assertion; but would remark, that when Dr. Downing proceeds to consider, in his next chapter, the *causes* of neuralgia, he limits his inquiry solely to the *spasmodic* form. Under this head we find nothing whatever worth critical notice, unless a weak hypothesis as to the proximate cause should be considered such. The usual causes are assigned, in an imperfect and slovenly manner, principally from Valleix's and Rowland's monographs. Age, sex, temperament and constitution, habits and mode of life, hereditary transmission, seasonal changes, and debilitating influences, are enumerated as predisposing causes, and all treated of in the most superficial manner in eleven widely-printed pages. The exciting causes are considered just as superficially, with only the slightest possible reference to modern neurology or pathology; the only references made being such as plainly indicate that the writer has little practical acquaintance with either. A favourite exciting cause is derangement of the "*primæ viæ.*"— "The much respected Mr. Abernethy was fully impressed with the validity of this source of disturbance," &c. Then Sir Charles Bell is quoted; next, "Sir B. Brodie has written somewhat to the same effect, although he by

■ Lectures. Edited by Cook and Thompson, p. 349.
† Traité Pratique des Maladies Nerveuses, vol. ii. p. 323.
‡ Lehrbuch des Nerven-krankheiten der Menschen.
§ Dictionary of Practical Medicine, vol. ii. p. 880.
|| Treatise on the Practice of Medicine, by George B. Wood, M.D., &c. &c., vol. ii. p. 799.

no means assigns the alimentary canal as the universal habitat of neuralgic irritation"—as if such unmitigated " universal" nonsense could possibly have a " habitat" in his mind. The laxity and want of precision which the author's style displays is shown in the subjoined:

" Other writers have detailed cases where [in which] neuralgia was closely connected with hæmorrhoids and fæcal accumulations in the rectum and sigmoid flexure of the colon [Copland, p. 886], or alternated with functional derangement and pains in the liver. Most medical practitioners must have met with similar instances. I have noticed several—among others, that of Mr. T., the editor of a weekly newspaper, who is subject to a kind of facial neuralgia, whenever the stomach is deranged; although other causes, such as foul air, a heated theatre, &c., will equally excite it." (pp. 45, 46.)

If the reader will refer to Dr. Copland's ' Dictionary,' he will find that the facts referred to by Dr. Downing are quoted in illustration of the application of a special physiological principle to pathology for which Dr. Copland has distinguished himself. Dr. Copland does not refer to mere dyspepsia or indigestion as such, but to the *physical* irritation of the nerves on the intestinal mucous surface which occurs in certain forms of disease, and in which the irritation is transmitted to the cerebro-spinal axis, to be *reflected* upon other nerve-fibrils therein. It is, in short, the doctrine of morbid *reflex sensorial* action which he advocates and applies. Now, of all this, Dr. Downing appears in unconscious ignorance; otherwise he would have surely written something definite on so important a point. Nearly half a century in arrear in his neurological knowledge, he is writing of " dyspepsia" and " disorder of the primæ viæ," while Dr. Copland refers him to his article, " Irritation," for what we cannot but characterize as a most able and concise explanation of the exciting cause of a large number of neuralgic, spasmodic, and cerebral phenomena. Even if Dr. Downing had not read Copland or Müller, the views of Dr. Marshall Hall cannot surely have escaped him; yet intimately related as these are to his whole subject, Dr. Downing never once refers to them !

The easy way in which our author passes over others of the more important and obscure points in the etiology of neuralgia, must not be unnoticed. Heat and cold, osseous disease, disease of the brain, caries of the teeth, and malaria, are dismissed in eleven of the widely-printed pages full of such platitudes, as " we may suspect (!) the neuralgia to arise from this cause [malaria], when we find it associated with ague, or the patient has clearly been exposed to miasmic vapours in a low, marshy district," the section ending with the following:

" A few words must suffice for the remaining exciting causes of neuralgia. Some of them are doubtless important, but at the same time obscure. It has not been my fortune to trace the disease distinctly to either of them, although one or other has been occasionally suspected. They should be enumerated, if only for the purpose of reference in doubtful cases. Thus, in addition to rheumatism and hysteria, which have already been considered, some writers insist upon acute or chronic neuritis, syphilis, epilepsy, and hypochondriasis. Certain French authors allude to derangement of the catamenial function, others to anæmia, while some English authorities rely almost exclusively upon spinal irritation." (pp. 60, 61.)

Dr. Downing is very much puzzled, of course, by the conflicting "authorities." Who are the " certain French authors," that dwell so specifically upon catamenial derangement and anæmia as the exciting causes of neur-

algia to the exclusion of the English? or who are the Englishmen who
"rely" so devotedly upon spinal irritation? Of these matters we are not
informed. Dr. Downing, however, thinks cold is "the chief excitant,"
closely followed by two causes "almost ignored by our continental neigh-
bours;" but in by far the larger number of cases it is, he thinks, "a most
mysterious infliction or dispensation of Providence"! As our readers may
think we are jesting on so grave a topic, we must really, in self-defence,
quote Dr. Downing's own words:

"In reviewing this long list of the known or supposed exciting causes of neur-
algia, we may naturally inquire which of them most frequently gives origin to the
disease?—which is the most ordinary disturber of the nervous equilibrium? These
questions scarcely admit of a satisfactory answer, in consequence of the *local
causes* of disturbance to which each form is distinctly liable. Yet cold may be
considered as the chief excitant, closely followed by dyspeptic irritation and
malaria—two causes which are almost ignored by our continental neighbours.
But it is impossible to arrive at anything like an accurate calculation on this
point. In an immense number, by far the larger proportion, of cases, no pro-
bable cause can be assigned for the first onset of the malady. We can gather
nothing satisfactory from our patient. He knows not *how* or *why*, but only *when*,
it came. It is often a most mysterious infliction or dispensation of Providence
on those who are not, at least particularly, distinguished for vice or folly. No act
of imprudence can be alleged as the offensive agent. Often will neuralgia select
for its victim the temperate, the prudent, and the good, without the slightest
warning or premonitory symptom." (pp. 61, 62.)

This is a correct example of the etiology of a prize-essay. In this way
questions of so great importance in the diagnosis and treatment of neuralgia
are—we were going to write discussed—but, in fact, slurred over. Take,
for example, the class of causes whose seat is in the blood, of which rheuma-
tism and anæmia are examples. How large an addition might have been
made to the general pathology of the nervous system, as well as to the
pathology of neuralgia, by a systematic investigation of the important
influences exercised by a morbidly constituted blood on the nerve-fibrils,
and particularly on the nerves in relation to the sensorium. The retention
of biliary, urinary, intestinal, and cutaneous excreta, is often the remote
cause of diseases of the nervous system, as well of the neuralgic as of the
spasmodic and maniacal groups. A few additional facts—nay, one solitary
case,—investigated in reference to these morbid conditions of the blood,
would have constituted a valuable addition to our knowledge; or some
of the morbid conditions of the blood, caused by poisonous elements
derived from without, might have been investigated in their relations to
neuralgia. No more valuable or interesting point could have been taken
up, than the operation of minute quantities of lead upon the sensitive nerves,
and the sensorial portion of the cerebro-spinal axis. An excellent basis
for such an inquiry would have been afforded to Dr. Downing by the
standard work of Tanquerel.* Of 1217 cases of lead-colic, 755 had also
lead-neuralgia, or, as Tanquerel terms it, *arthralgia*, by which term he means
—not *articular* pains, but—"acute pains in the limbs, unattended by
redness and swelling, not following the exact course of the nerves, con-
tinuous, but becoming more acute in paroxysms, *diminished by pressure*,
augmented by motion, and accompanied with disturbance of the motive

* Traité des Maladies de Plomb ou Saturnines. Paris, 1839.

functions, such as cramps, hardness, and tension of the pained parts."* It is by no means improbable that in persons of the neuralgic diathesis, or with a predisposition in some of the sensitive nerves to exaltation of sensibility, or with a susceptibility to take on morbid action, minute doses of lead might induce spasmodic neuralgia, although not sufficiently large to excite lead-colic, or *mesenteric* neuralgia, as it has been termed.† So grievously ignorant of his subject, however, is our author, that he never once refers to this action of lead, not even for diagnosis!

There is a case in an interesting medical annual (published in the United States), which is so instructive on this point, that we make special reference to it here. It is a case of disease of the nervous system, "extending over four years," reported by Dr. Murphy, of New Harmony, Indiana, and caused by lead. The following was the condition of the patient, as regards the neuralgic phenomena, when he was at the worst:

"The slightest touch of the skin over the umbilicus, and indeed over other parts of the body, produced such terrific pain as almost to throw him into convulsions, producing all the effects of an electric shock; while the greatest pressure over the same place gave him no uneasiness, but rather relief. . . . . From the balls of both thumbs, which were much atrophied, excruciating pains would arise, shooting with great severity up to his arms and shoulder, to the back of his neck and head; the shoulders were affected with constant pain, especially the deltoid muscles, which also were slightly paralyzed. The pain in the lower extremities was also very severe, commencing in the soles of his feet, which were so sore that he dreaded to touch the floor with them, and shooting up the limbs to the lumbar region with dreadful suffering. There was also at this time a new source of suffering—shooting pain through the testicles, of such severity, as almost to produce fainting; indeed, to see him in this suffering was the most heartrending sight I ever witnessed."‡

In this instance no exposure to lead-poison could be traced, until it was discovered that the patient had been in the habit for many years of chewing lead, always putting a bullet or shot into his mouth when out hunting; chewing the lead-lining of tea-boxes, &c. A course of mercury cured him. The reader cannot fail to perceive how accurately as to their character the pains correspond with those of neuralgia.—Those morbid states of the blood, in which the relative number or the condition of the blood-corpuscles has undergone change, have important relations to neuralgic as well as to other affections of the nervous system. The operation of minute doses of febrific poisons in developing the spanæmic, anæmic, or chlorotic cachexia, has an important bearing on the *modus operandi* of malaria, as an exciting cause of neuralgia. A zealous and industrious inquiry on these points would not have failed to elucidate some acknowledged difficulties in the *treatment* of these most intractable diseases. Such an inquiry might be reasonably expected from a prize-essayist; but in this, as in other points requiring scientific research based on a thorough knowledge of modern physiology and pathology, Dr. Downing is wholly wanting. Again, rheumatism and neuritis might have been considered in conjoint relation to neuralgic diseases—the neurilemma being referred theoretically to the class of sero-fibrous tissues; or inflammation of the true sero-fibrous membranes of the nerves and cerebro-spinal centres might have been so considered, and much light thereby thrown on the therapeutic diagnosis and the general pathology. Here, again, Dr. Downing

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* Op. cit., vol. i. p. 493.   † Romberg, op. cit.
‡ Southern Medical Reports. Edited by E. D. Fenner, M.D., of New Orleans, vol. ii. (1850), p. 276.

is also wholly defective. The intimate connexion between hysteria and hypochondriasis, on the one hand, and the neuralgic diathesis and neuralgia, on the other, suggests a searching inquiry into the natural history and proximate cause of that congenital or acquired state of the organism which constitutes the predisposition to suffer from diseases of the nervous system, with reference, not merely to the curative or palliative, but also to the prophylactic treatment; as to all this, Dr. Downing is silent beyond the bare mention of the most commonly observed and most obvious facts, as to its hereditariness, &c. We might comment on numerous other omissions, such as all mention of the influence on neuralgia of electrical, magnetical, and barometrical changes; of the variation in the winds; of the hygrometrical condition of the atmosphere; of the seasonal changes, &c.; but we are weary of the task.

Dr. Downing next proceeds to consider what he terms " the *local* causes," meaning the pathological anatomy of neuralgia. Every student knows that structural disease or lesion of a sensitive nerve is the most common and most certain cause of neuralgic pains. We quote Dr. Downing's introductory remarks to this section, principally to illustrate the faults of his style.

"One other very interesting question remains to be considered. Whether neuralgic pains seated in a particular nerve or spot are not due to some local determinant cause? I am firmly of opinion that they are. In most of those cases where [in which] the pains are persistent—not flying and erratic, but fixed for years to one place—some such cause exists, although it may be impossible always to demonstrate it. Why should otherwise the pain be determined to one particular part? Why should one nerve, or branch of a nerve, be selected (so to speak) to bear the anguish? Possibly it is often but a trivial agent that gives the pain localization, when the neuralgic diathesis is established, as the slightest chill or draught of cold air is sufficient to direct rheumatism to the shoulder, the back, or the scalp, when the blood is inflamed. Often may the local predisposition be traced to an accident that has occurred long before, all recollection of which is forgotten, until brought to mind by close inquiry and questioning. The ample testimony which Dr. Rowland has collected on this point is deserving of close attention. He says," &c. (p. 64.)

It seems scarcely credible that Dr. Downing should have here so closely approached the recognition of a disordered state of the *blood* as a cause of neuralgia, and yet not have made it. By the way, what does he here mean by " inflamed blood"?

Dr. Downing mentions as local causes, contusions, blows, wounds, foreign bodies in a nerve, cicatrices, tumours, spiculæ [*sic*] of bone penetrating or pressing on nerves, and neuroma of stumps. These local causes, so thoroughly well known to every practitioner, are considered subsequently together, under the term *traumatic* neuralgia, in which a few cases quoted from authors are thrown together, and one or two added from the author's experience. In the whole section there is nothing, absolutely nothing, but what may be found in many other and better treatises, except one case by Mr. Gay, and two by the author; yet Dr. Downing can not only say of his book generally, that " he believes it ranges over a wider field than its predecessors," but of this particular section, that it " is quite novel!" How imperfect this part of the etiology of the essay is, may be easily understood, when we assure the reader that *centric* local disease of the medulla spinalis, or ganglionic disorganization, is never once referred to, and it has evidently never occurred to Dr. Downing to name it as a local cause of

neuralgia. Nothing can more strikingly show Dr. Downing's singular ignorance of the physiology and pathology of the nervous system, than this omission. No writer in the least acquainted with modern views could possibly have overlooked this important class of local causes. It is quite true that Dr. Downing mentions osseous deposit within the *cranium,* and other organic lesions there, amongst the reputed *exciting* causes; but it is principally to show that they are rather the *effects,* than the causes of neuralgia.

The proximate cause of neuralgia presented considerable difficulties to the earlier writers. Modern pathologists are better able to rest contented with the explanation, that it is simply such a change in the functional activity of the sensitive nerve-fibril, either at its commencement, or along its course, or at its termination in the sensorium, that its faculty to communicate to the mind the sensation of pain is indefinitely exalted. For this reason the general term *hyperæsthesia* has been given to this group of disorders. Dr. Downing combats the notion that it is not neuritis—as the greater number of writers have done before him. The proposition is obviously true, unless it be argued that inflammation of a nerve arises whenever pain is felt by its agency. The same remark is applicable to hyperæmia and to local structural change. Nevertheless, these may all be the immediately exciting causes. Dr. Downing gravely argues that the negative evidence of *post-mortem* examinations should be set against the positive evidence, and infers, that because certain eminent men have not been able to discover local structural change in the nerves affected with neuralgia during life, the disease may exist independently of structural change. The conclusion, as an abstract proposition, we are not prepared to deny; but the argument is obviously unsound, for there is nothing to show that the observers referred to examined the central axis, or the nerve-fibril in its course through the spinal cord, or the medulla oblongata, or the sensory ganglia at the base of the brain. Dr. Downing advances an hypothesis; he thinks that there is a spasm in the nerves themselves, or rather, a spasm of the neurilemma. We subjoin his own words:

" It cannot be supposed that the neurile matter itself is possessed of contractility. The power must reside, if at all, in the neurilemma, the structureless membrane enclosing it, identical with the sarcolemma of unstriped muscular fibre. This tissue, enveloping each fibrilla, must constitute a considerable part of the bulk of each nerve. Its properties have been little investigated. It is known to possess elasticity. Contractility is the most universal principle of the living system. It resides in most of the tissues, and is capable of being roused into activity by appropriate stimuli. It may exist in the neurilemma, although ordinarily quiescent. No great liberty is taken, therefore, in supposing that this tissue is susceptible of extraordinary excitement under disease, and, by compressing and extending the neurile matter, giving rise to the severest local pains." (pp. 89, 90.)

Dr. Downing's style of argument in favour of his hypothesis is well illustrated by the following:

" We may reasonably infer that other nerves are capable of this contraction by [from a consideration of] our feelings when those of the leg are irritated. The sensations are, I should say, exactly similar to those in spasmodic neuralgia, except that the latter are attended by more pain. For the purpose of observing the phenomena, I often, as the saying is, *send my leg to sleep,* by allowing the limb to rest on a hard substance placed in the ham. Upon removing it, I find the well-known tingling sensation to ensue, with alternate heats and chills. After a

time these feelings pass off, and then the limb remains with scarcely a thrill, while in a quiescent state; but the instant it is moved and the ankle or even toe joint bent, the whole leg and foot seem suddenly encircled by tight-drawn cords and wires. . . . . . The sensation conveyed to the mind is as if the *nerves* were suddenly called into spasmodic affection. You can scarcely be deceived in the matter. Their whole course and distribution are plainly revealed to the mind's eye, and you feel them distinctly drawn up and contracted." (p. 90.)

Dr. Downing's doctrine, in short, is this: that *the nerves themselves are contracted* in paroxysms of neuralgia. He attributes its first promulgation to Sauvages and Swan, but labours hard to establish it as a novelty. He thinks the clicking or peculiar sensation heard or felt by persons suffering from facial neuralgia, may be due to this spasmodic action of the nerve itself, and considers the word "tic" very appropriate. "The word *tic* signifies a convulsive movement or spasm, and therefore André, who suggested it, considered it very applicable, more especially to those cases where [in which] there was trepidation or convulsive twitching of the skin." (p. 190.) A curious style of argument, certainly, but quite without foundation. There are unstriped muscular fibres in the skin itself (as Kölliker has shown very lately), independently of the true subcutaneous muscles, such as are on the face. It is probably these that contract in certain forms of spasmodic neuralgia. Besides, the word tic is medically synonymous with trismus;* tic douloureux is simply trismus dolorificus—*agonizing* trismus; and a case is quoted by Sauvages from André's 'Observations' to illustrate the definition. Under the head of Trismus Occipitalis, Sauvages quotes another case, recorded by the same author, which should evidently be classed as a cervico-occipital neuralgia. This is only one, however, of many instances, which prove that Dr. Downing has taken his quotations at second-hand from the half-dozen treatises, out of which he has culled the principal materials of his essay.

All that Dr. Downing states as to the *treatment* of neuralgia, has been better said by Valleix, Rowland, Copland, Romberg, and systematic writers in general. A chapter is devoted to a "New Method of Treatment," which is simply the topical use of the vapours given off by narcotic vegetables in a state of combustion. This is effected by an instrument, which Dr. Downing terms the Aneuralgicon. He appears to have relieved and even cured several very intractable cases by this method, but this class of affections is remarkable for invaluable remedies. The history of neuralgia contains not a few examples of similar cures by every possible variety of means, from mesmerism to metallic tractors and electro-galvanic chains. Dr. Downing uses mixtures of the leaves, slender stalks, and seeds of various narcotic plants, as "belladonna, henbane, cannabis indica, tobacco, hops, aconite, stramonium, hemlock, digitalis, &c. The seeds of various herbs have also been added under certain circumstances." The reader is referred to a "brochure," already published, for a full description and engraving of the instrument, both which should certainly have been given in this volume. We presume we must seek in the same "brochure" for an account of the "certain circumstances" which regulate the "mixtures" of the plants, and the addition of the seeds of various herbs, as also for the names of the latter. We have looked carefully through this prize-essay for these particulars of treatment; we have specially examined the details

* Sauvages, Nosol., vol. i. p. 530, ed. 1768.

of all the cases in which the aneuralgicon was successfully used by Dr. Downing, but we can find no clue to a selection of " the plants," and " seeds of various herbs."   In *one* case (of crural neuralgia), it is true, we are told that the aneuralgicon was " charged with belladonna and henbane," but why these were selected in preference to aconite, stramonium, cannabis indica, &c., we are not told.   Dr. Downing must, we think, be well aware that honourable discoverers of new methods of cure are not generally so costive of information as he appears to be, and that it is scarcely fair to refer the readers of what professes to be a complete monograph on Neuralgia, to another publication, for details of the treatment recommended.

In discussing the various special forms of neuralgia, Dr. Downing adopts the ordinary arrangement, beginning with that form known emphatically as tic douloureux.   We here find the same lax, indefinite notions as in the earlier part of the essay.   A few well-known facts as to the anatomy and physiology of the nerves of the face are stated, with no distinctness of application, and with little effect in elucidating doubtful points as to the etiology, pathological anatomy, and therapeutics of facial neuralgia. We are invited to remember that the nerves are numerously distributed to the face—that there is a " peculiar connexion" between the three kinds of nerves—that they have also special anatomical relations to the bones, muscles, and tendons—that the face being much exposed to the weather, they are "more liable to be affected by atmospheric influences and malaria;" that it is by virtue of their relations to the sympathetic ganglia, that they " sympathize with the *vital actions* of the abdominal viscera," &c.; but a philosophical analysis of the varied phenomena and of the special forms and varieties of facial neuralgia, in reference to their anatomy and physiology, and physiological relations to the nerves, is nowhere afforded.

The fundamental principle, that irritation of a nerve-fibril at any part of its course developes its functional activity, should be the basis of the nosology of this class of diseases.   It has long been well known that irritation upon one class of afferent nerves—e. g., the gastric or intestinal, although not *felt*, will reach the cerebro-spinal centre, and develope therein such changes that infinitely varied phenomena, sensorial and motorial, will result. Sometimes the cerebrum will manifest the effects of distant or afferent irritation, and just according to the portion of the cerebrum predisposed to diseases will be the phenomena produced: in one it may be epilepsy; in another hypochondriasis; in another hysteria; in another vertigo; in another mania.   Sometimes the medulla oblongata or the medulla spinalis manifests the influence of the irritant, and various forms of visceral or musculo-cutaneous spasm or neuralgia are excited.   In this way the trigeminus and facial nerves may suffer from changes within the medulla oblongata—limited to them, and specially directed to them—induced by local afferent irritation on some other and distant nerve-fibril.   Nothing is more common than this circumstance in disease of the pelvic viscera, as, e. g., when in hæmorrhoids, uterine congestion, cholera, or dysentery, neuralgic pains and spasms are excited in the musculo-cutaneous nerves of the leg and thigh.   This point in the etiology of painful diseases is the characteristic of a large class—viz., those arising from excentric irritation.

But the fundamental nosological principle we have laid down, taken in

combination with the last mentioned, leads us to another large class—namely, those in which there is local centric irritation, independently of the operation of distant irritants, and induced by causes acting directly on the nerves themselves. These causes are very numerous, and would of themselves constitute a most interesting subject for original investigation. To this class we must refer those neuralgiæ which are excited by *excessive use* of the implicated nerves. A long march will thus induce sciatic neuralgia. A very interesting example of this kind is the *spasmus scriptorius*—a muscular neuralgia altogether unnoticed by Dr. Downing. It attacks those who are compelled to write continuously for a long time, and was not all unfrequent during the heat of the railway-mania. Artisans and others, who use their eyes much on minute objects, or in reading, &c., are liable to suffer from ciliary neuralgia, excited in this way. Dr. Downing also omits to mention this form, although it is practically of considerable importance; there are, however, two or three instances of it detailed in his book. The case of the needlewoman (p. 214), and of the attendant in the library of the British Museum (p. 212), both termed ophthalmic neuralgia, are examples of this kind; so also the case of " tic douloureux" in a tailor (p. 177).

Allied to this cause is the operation of a constant mechanical irritant of a nerve. This head comprises all those cases in which exposure to extremes of temperature, blows, wounds of nerves, cicatrices involving nerve-twigs, neuromatous growths, &c., induce neuralgia. Less obvious, but not less important, are those cases in which over-use or excessive irritation induces a change in the nerve-centre as well as in the nerve-root; or, in other words, in which the irritation extends from the fibril affected to the cerebro-spinal centre, and *within* it to others in its immediate locality. Thus *spasmus scriptorius*, or neuralgia ciliaris, will excite neuralgia of the contiguous brachial or facial nerves. Irritation of this kind may spread through the cerebro-spinal axis, and involve it in morbid action to a considerable extent.

Causes like these may not operate with sufficient vigour to excite neuralgia, but they may induce such a sensibility to impressions, that trifling exciting causes of *another* kind are sufficient. Thus distal afferent irritation may render a person susceptible of the influence of cold or of changes of weather, but it is only when these come into action that the paroxysm is excited; or it may be *vice versâ;* excessive use, the application of cold, or mechanical irritation, has rendered a nerve susceptible, and then distal afferent irritation (such as the gastro-intestinal, uterine, urethral, &c.) or even any *emotional* excitement, will induce neuralgia. In this way excessive use of the perceptive organs, as by artists, experimental observers, &c., developes a *general* susceptibility, to be localized when the appropriate local exciting cause is applied.

It is when there is this induced local susceptibility, that *morbid elements in the blood* act most obviously in inducing neuralgia. Malaria may be present therein, yet remain latent and harmless, until this state occurs. So also the *materies morbi* of rheumatism or gout may " fly about," to use Dr. Downing's inelegant phraseology, until it is specially manifested in some locality, rendered more susceptible by the predisposing causes we have specified. This seems to select specially the serous or sero-fibrous tissues; and it may be observed, that poisons in general have a special elective affinity for certain portions of the nervous system; lead, in the first instance,

at least, acts upon the mesenteric plexus. This poison has, however, a local as well as a general action, and is very probably the undiscovered cause of many intractable neuralgic and spasmodic diseases.

So much for the nerve-centre, and for neuralgiæ of *centric* origin. *Local* disease of the nerve may be situated either, 1. At its exit from the centre, or between that point and the ganglion which is formed upon the roots of all the nerves of common sensation; 2. In the ganglion itself; 3. In the trunk, or portion of the trunk of the nerve. In neuralgiæ of the facial nerves, the diagnosis is especially complicated as to these points by their anatomy. If we were to take Dr. Downing as an authority, we should be led to believe that the Gasserian ganglion is never the seat of disease in tic douloureux, for he mentions no such contingency. Yet nothing is more certain than that some of the most intractable cases of tic douloureux have depended upon diseased structure of that ganglion. The particulars of a highly instructive case of this kind, of twenty-six years' duration, are related at very great length by Romberg, together with the results of a *post-mortem* examination.* It is a model-case, to which we refer Dr. Downing, for we are sure its perusal (if he is able to read it) will convince him of the imperfections of his own histories, and indeed, of his whole essay.

Dr. Downing's handling of the musculo-cutaneous neuralgiæ is sadly defective, but his treatment of the visceral neuralgia is worse. Some are hardly mentioned; others are *only* mentioned; *all* are treated in *eight* pages! He terms them "neuralgiæ of the internal organs." We subjoin *excerpts* from this portion of the *prize*-essay.

" Although a treatise on neuralgia could scarcely be considered complete without some allusion being made to the disease when seated in the great cavities of the body; yet, on the present occasion, I should judge it would be unnecessary to give more than a rapid sketch of the symptoms, on account of the great obscurity of the subject, and the great length to which this essay has been already carried.

" Neuralgia of the viscera is now universally admitted by physicians, and enables them to account for phenomena which are otherwise inexplicable, and to supply a rational and frequently successful method of treatment. Many facts, such as the following, may be adduced in proof of its existence. Pain, without perceptible organic change, is felt in an internal organ—the liver, for example. This suddenly ceases, and immediately makes its appearance, with all the characteristic symptoms of neuralgia, in the face or foot; once more it vanishes from thence, and is instantly perceived in the side, as before." (p. 345.)

It would be difficult to excel Dr. Downing in the composition of *rigmarole*, but surely the prize was not awarded to him for excelling in that! And yet we have a strong suspicion that the judges have inclined to the prize-man for his feats in this respect; else how is it, that a prize being offered for the best essay on a given subject, that it may be thereby more elucidated, one half of it is left untouched in the prize-essay, " on account of the great obscurity of the subject"?—the obscurity being most palpably, most unmistakably, in the writer himself. " The greater part, I should judge," (a favourite expression of the author, when he is puzzled by his ignorance of his subject,) "are of the hysterical kind"— the common form of metallic colic, especially lead-colic, for example! "They are produced by the same causes as hysteria. . . . . There is no ground

* See Romberg's Lehrbuch des Nervenkrankheiten, vol. i. We are glad to learn that a translation of this valuable work has been determined on by the council of the Sydenham Society.

for believing them rheumatic." Yet both cardiac and cœliac neuralgia are intimately associated with gout, and very rarely occur even in the hysterical unassociated with the arthritic diathesis. Angina pectoris (cardiac neuralgia) is a remarkable illustration of the connexion between the gouty habit and painful nervous affections ; we would refer Dr. Downing to Dr. Forbes's essay, in the ' Cyclopædia of Practical Medicine,' for some facts that will be quite new to him. The notice of nephralgia is a curious compound of all the bad points in Dr. Downing's book.

" NEPHRALGIA.—There can be but little doubt [!] that the larger proportion of the painful sensations, experienced in the loins by young women, consist of hysterical neuralgia of the kidneys. The greatly-increased secretion of urine, during the paroxysms of hysteria, indicates considerable nervous irritation in these glands at such times. Neuralgic pains of the spasmodic variety are often caused by the passage of a renal calculus, or other local stimulant. It will be observed that the pain in these cases is not constant, but is paroxysmal in character, and takes the course of the ureters, or spermatic cord. It is distinguished from nephritis by the absence of fever, by its intermittent nature, and there being no retraction of the testicle." (p. 349.)

This is the whole of the paragraph which Dr. Downing devotes to so obscure and important a disease as neuralgia of the kidneys; the history, pathology, diagnosis, prognosis, and treatment, are all comprised in what we have quoted. But saturnine neuralgia is treated even more absurdly, for all these points are comprised in one line and a half, which we quote, as a curiosity in medical literature : " The colicky pains engendered by lead are instances of the same affection in the bowels." (p. 349.) We must not omit, however, to state, that Dr. Downing has a case of " *ovarian* neuralgia," which is simply a case of neuralgia of the anterior crural nerve, the paroxysms occurring most usually at the menstrual period. Just now, however, everything is uterine or ovarian, and Dr. Downing must be in the fashion.

Such is the critical analysis of this curious professional work, which we have thought it right to place before our readers, to whom it is obvious we owe an ample explanation for noticing it at all. We allege as our warranty, that it professes to be the Jacksonian prize-essay of the Royal College of Surgeons of England. If this be so, it is obvious that there must have been something extremely faulty in the adjudication of the prize. The inference inevitably follows from the premises. Now this is an important fact; for it very nearly concerns the honour and credit of the fellows and members of the College at large, that they should prevent the repetition of a proceeding which so seriously compromises its dignity. A prize essay, issued under its sanction, ought surely not to be of a character to degrade it— even if it should not elevate it—in the estimation of our Continental and American rivals; and we think that we have given ample proof, not only that the author has added little or nothing to our knowledge of the subject, but that his work must be regarded as exhibiting a degree of ignorance of what has been done by others, such as even an ordinary compiler might be ashamed of. We cannot believe for a moment, that the adjudicators acted under any undue bias; but we must consider, either that they have been incompetent, or that the prize was awarded to Dr. Downing simply because there was no worthier competitor. If the latter be the explanation, we must hope that the Council, when the time comes for the

next adjudication, will withhold the prize—according to the custom of many continental academies—unless some essay should be found which can be placed before the public as a creditable specimen of the scientific and literary attainments possessed by the members of the College. We feel sure, moreover, that Sir B. Brodie could never have consented to accept the dedication of the essay, had he not taken its merits on trust; for in no other way can we explain the singular incongruity which is manifested between Sir Benjamin's own researches into neuralgia, and those to which he has lent the sanction of his name.

We owe a duty to medical literature, too, which would be very incompletely discharged were we to allow a book like this to pass unchallenged. The labours of men like Tanquerel, Valleix, Swan, Romberg, Copland, Hall, Müller, and the great advances made in neurological pathology of late years, by numerous inquirers, are practically ignored by a writer who is in his acquirements at least half a century in arrear; all these would have abundant reason to complain if we had allowed such a work to be ushered into the world under such distinguished sanction, without a hearty protest.

Art. VIII.

Maladies de l'Algerie. Par le Docteur A. Haspel.
On the Diseases of Algeria. Vol. I. *The General Causes of Disease in Algeria, and Diseases of the Liver.* By Augustus Haspel, M.D.— *Paris*, 1850. 8vo, pp. 390.

Among other ill effects of a conquest which all true friends of France must wish that it had never undertaken, the occupation of Algeria has given rise to an enormous amount of disease and mortality among the military forces engaged in its accomplishment. Dragged from their homes by the tyranny of the conscription, exposed to the inhalation of poisonous miasmata, devoid of the few comforts a soldier's life is susceptible of, very insufficiently supplied with food, lodging, and raiment, and constantly harassed by the untiring activity of a nomadic foe, whose sufferings and resistance excited the sympathies of Europe, and whose subjection was unattended by glory—the French soldiers, with their physical energies depressed and their *morale* unsustained, rapidly fell victims to climatic influences, which even the robust and well-fed are not always able to resist. Of late years this mortality, though still very great, has, by the observation of the laws of military hygiène, become much lessened. These scenes of privation, suffering, activity, and rigid discipline, favoured the production of a hardy and energetic race of military generals, who will yet probably influence the fate of Europe; and brought into play the acumen and powers of observation of several distinguished medical officers, whose labours have imparted to France the same description of valuable information on the diseases of warm climates, as that which our Indian practitioners have conferred upon our own country.

Among these M. Haspel has already made himself favourably known by several publications in the *Recueil de Mémoires de Médecine Militaire,* (some of which were noticed in the 'Med.-Chir. Rev.,' Jan. 1846), and other periodicals; and in the present work he furnishes the results of his

more extended experience, and more matured reflection. His observations apply chiefly to the province of Oran, in which he exercised his medical functions for several years. Although this province offers great diversities of soil, surface, elevation, and geological structure, some particulars of which are furnished, we need not advert to these, inasmuch as the author states that certain common causes of insalubrity everywhere impress an identical character on the diseases that are induced, wherever these may be observed. The chief among these causes is the accumulation, during the rainy season, of vast plains of water, hemmed in by elevated land, and insufficiently provided with issue. The evaporation of these commences in May; and the heats of summer and autumn give rise to abundant poisonous exhalations, due to the decomposition of the organic matters held in suspension. The main indication here is evidently to procure the discharge of these waters by the enlargement of natural channels, and the excavation of artificial ones; and in proportion as such works have been accomplished, has the health of the soldiery improved.

The Medical Constitution of Oran.—The varying forms which diseases assume in the different seasons may be briefly adverted to. In this point of view, M. Haspel divides the year into four periods, distinguished by certain pathological peculiarities, which, he says, may be designated, if we adopt the nomenclature of the ancients, as the *nervous* type in the spring, the *bilious* in summer, the *putrid-bilious* in autumn, and the *putrid-catarrhal* in winter. The *first* of these periods commences in March and terminates in June, and constitutes, with its delicious temperature (a mean of 66°), and absence of morbific agencies, the most favourable season for Europeans. Diseases which then occur are simple in their nature, and are attended neither with tedious convalescence nor obstinate engorgements. The *second* period comprises July, August, and September, during which the intense heat (giving a mean of 84°) from an unclouded sun, and an entire want of rain, convert the country into the appearance of a desert. Towards the end of this period, the heated blast of the simoon exerts its withering influence on man, animals, and plants. Diseases become rapidly worse, intermittents yielding with difficulty, and undergoing complication with congestions of the internal organs. The succession of symptoms and of periods is disturbed and confused. Hyperæmia of the liver, under the guise of a trifling ailment, lays the foundation of serious or incurable disease; and the simple diarrhœas or dysenteries of the former period take on what has been termed the bilious or hepatic form, and become much more dangerous diseases, and much more difficult of cure. It is worthy of remark, that notwithstanding the high temperature, and the frequency of violent vomiting, the brain is seldom affected during this period; and that when delirium is present, it rarely indicates meningitis. Gastric inflammations are also rare.

The *third* period (from October to December) is characterized by sudden and frequent alternations of heat and cold, and the presence of occasional rains and an abiding moisture. The mean temperature varies from 73° to 77°. A state of general atony or defective power of re-action prevails. Diseases become more complicated and severe—quotidian or simple remittent fever being replaced by tertian, quartan, irregular, and erratic forms, which yield with difficulty, and re-appear with great obstinacy. They pass

into the chronic state, are sometimes hardly to be distinguished from typhoid fever, and become complicated with great visceral engorgement. Diarrhœa and dysentery are both more frequent and more severe; and though still bilious at the commencement of the epoch, they manifest a strong disposition to the adynamic or putrid state towards the end— simultaneously with the typhoid degeneration of intermittent fever. It is in this season that chronic dysentery attacks exhausted subjects; and hyperæmia of the liver is now of frequent occurrence.

The *fourth* period extends from the latter half of December to the beginning of March, and is especially characterized by the abundance of rain. The thermometer often sinks below the freezing-point, but fine and warm days are of frequent occurrence, so that great alternations of temperature prevail. The fevers, in place of intermittent, manifest pernicious or adynamic forms, with severe cerebral symptoms, arising from chronic meningo-cephalitis. In some years, and especially when there is excessive rain, symptoms of scorbutic degeneration or of gangrene appear, and abundant evidences of the dissolved state of the blood are made visible at the autopsies. In this season, chronic and insidious hyperæmia of the liver, leading to abscess or ramollissement of the organ, is common. Pneumonia, which is not common in Algeria, now prevails, and is of a low type, not admitting of depletion. Convalescence from any disease is very tardy, relapse frequent, and mortality great, especially in relation to the much fewer numbers of sick at this period of the year as compared to the preceding.

Etiology of the Diseases of Algeria.—Before adverting to the causes which he regards as supplementary or secondary, such as the regimen and habits of the soldier, M. Haspel dwells at some length upon the *miasmatic intoxication of the atmosphere,* which he considers as the primary cause of the endemic and epidemic diseases of the country. The pathological picture, he observes, so graphically drawn by Pringle, amidst the marshy plains of the Low Countries, is faithfully reproduced in Algeria, situated in so different a parallel, and possessed of so different a climate. The climate of Holland has undergone but little alteration since Pringle wrote; but the eye of science and the hand of man have been busy in designing and executing those works of improvement, whose extension is always followed by a proportionate amelioration of the health of a community. On the other hand, localities once flourishing and salubrious are now, in their decayed and neglected condition, the hot-beds of disease and death. The French medical officers now serving in Italy have the opportunity of observing epidemics, with whose characteristics their previous Algerian experience had rendered them familiar.

"In traversing the globe, what a prodigious difference of climate, what a variety of soil, what an inequality of elevation, are observed, and yet what a pathological unity exists. The same scene of morbid phenomena is reproduced in a regular and constant manner in a thousand places at once, in spite of climate or the brilliancy of the atmosphere, wherever the same causes of insalubrity prevail—becoming only in certain localities more intense and more prolonged.

"What a contrast, as far as the comparison of climatic influences is concerned, prevails between the icy seas of the polar regions and the burning sands of the desert—between the Siberian, with his hut planted on the very confines of living nature; the Mongol, traversing his vast saline steppes; or the inhabitant of Kamts-

chatka, dwelling amidst his ice and his extensive marshes, and the nomad tribes of the parched regions of Africa, who raise their tents amidst the burning sands. And yet amongst all these what a morbid unity, what an identity in their annual epidemics are observable; so that we may see the same causes in operation in climates separated from each other by, so to say, insurmountable barriers, approximating by the identity of their effects the most opposite portions of the globe." (p. 44.)

Although, however, such marshy emanations constitute the grand primary cause of the sickness encountered in Algeria, other causes exert great influence as predisposing ones. To some of these we may briefly refer.

Food of the Soldier.—All medical observers, familiar with his condition, have pronounced the diet of the soldier serving in Algeria to be insufficient. At first it consisted of but 550 grammes of biscuit while on march; and the rations which have been substituted, though a great improvement, are not sufficiently ample. They consist of biscuit, 643 grammes; meat, 300; rice, 60; coffee, 12; sugar, 12; and salt, $16\frac{1}{2}$ grammes. The meat is weighed uncooked, and the head, spleen, liver, &c., form portions of the quantity allotted. During the hot season it becomes of a very impoverished quality, even when not positively deleterious; for the animals then rapidly die fearfully emaciated, and lesions of the viscera are constantly observed, the lungs, liver, and spleen being loaded with dark-coloured blood, or exhibiting tubercular or hydatid productions. Abscess of the liver is frequent. The soldier's bread or biscuit is ill-made, having undergone little or no fermentation; and even of this the quantity he obtains is so insufficient, that he expends any extra pay he may receive in purchasing additional portions at very high prices. While on march, no fresh vegetables can be obtained; and the same dull uniformity of diet often prevails from year's end to year's end. As a natural consequence, when the opportunity presents itself, as in a razzia, the soldier gives way to the extremest excesses. As in part exemplifying the effects of this defective diet, the disproportionate mortality of the soldier as compared with the officer may be cited; the army losing one soldier in every 12·8, while it loses an officer only in every 54·4—although the deaths on the field of battle are relatively greater amongst the officers than amongst the men. So, too, the artillery, engineers, and commissariat, who receive better pay, are often in the enjoyment of health, while the rest of the army is decimated by fever.

Drinks.—Besides this defect in the amount and kind of food, Dr. Haspel regards the bad condition of the potable waters in Oran as a fertile source of the prevalent dysentery and liver-disease. It is of the most filthy description, and yet, owing to the excessive thirst engendered by the heat, is drunk in large quantities. Nevertheless, in some localities in which the water is pure these diseases prevail. M. Haspel believes that the occasional intemperance of soldiers, upon their return from expeditions, during which they have obtained no access to spirituous drinks, has been too hastily set down as a cause of dysentery—the temperate by no means obtaining the exemption which this view would lead us to expect. He regards, too, the absence of wine from the soldier's diet-table as a great deficiency, which the coffee does not supply. During expeditions, at all events, he believes that one or two rations of brandy should be daily distributed, in order to aid the flagging digestive powers, and support the strength under extraordinary

fatigue. Ten years' experience in Algeria has convinced him that too much influence in the production of disease has been assigned to the employment of stimuli, while its genesis from exactly opposite sources has been too much overlooked.

Fatiguing Duties.—The young soldiers summoned from France before growth is entirely completed, and submitted to this insufficient diet, are also during the expeditions subjected to inconceivable hardships. Harassed by constant forced marches through districts devoid of every means of sustaining life, loaded with the necessaries of existence to such an extent as to be called by the Arabs, in derision, "the camel-soldiers," unable to consume their wretched repasts in peace, and reposing either without shelter or in most overcrowded receptacles—these unfortunate beings rapidly become victims to fatal disease. It is, however, consolatory to add, that this state of things is gradually undergoing amelioration; for notwithstanding the activity of the warfare in the province of Oran from 1840 to 1847, sickness and mortality kept continually decreasing. This has been due to the organization of a better commissariat, the amelioration of the abodes, the establishment of additional camps, the return of numerous invalided subjects to France, the progress of medical science, and above all to the increase of agricultural enterprises.

The Action of High Temperatures.—It is not during the hot months, that the Algerian diseases put on their severest epidemic forms. In the colder and damper season of autumn, the frame, which has been over-excited during the prevalence of the high temperature, becomes prostrated and more liable to disease. It is then that the paludal miasmata exert their influence with most effect, converting simple fevers into pernicious ones, and giving rise to the development of severe epidemics of dysentery and hepatic disease. Elevation of temperature does not, then, exert any direct influence in the production of these diseases; for, frequent as they are in tropical regions, they are also found prevalent in northern countries, where other causes than heat must play the part of adjuvants. More importance is to be attached, even as an occasional cause, to *vicissitudes of temperature*, which in Algeria are often, towards the end of summer and in autumn, sudden and considerable. The influence of combined cold and moisture in the induction of dysentery, has been noticed by numerous writers.

Predisposing Conditions of Individuals.—Upon this point it is difficult to speak with any exactitude. It is in warm climates that we can observe how deceptive appearances sometimes prove—the most robust men, who seem most likely to resist the attacks of disease, being often those first affected. Nevertheless, among the predisposing individual conditions may be noted the lymphatic temperament, "the fatty and humid constitution," a northern origin, exhaustion from excesses of various kinds, inebriety, depressing emotions, or inordinate physical exertion. In reference to temperament, the author makes the following remarks on animals:

"Among animals, temperament exerts a great influence in determining the acute or chronic form of hepatitis. The acute form is met with in those of the arterial sanguineous temperament. In the Arab horse, which may be taken as the type of this, the affection of the liver proceeds with the utmost energy, while in the ox or sheep such acute form is rarely met with. The animals which have appeared to

us most exposed to chronic hepatitis, are those of the venous-sanguineous temperament, of which the ox is pre-eminently the type, and then those of a lymphatic temperament, as the sheep. In the space of five months we met with at Mascara more than one hundred cases of more or less complicated chronic hepatitis in oxen; sheep only presented a twentieth part, and in the same space of time not a single horse was so affected, although the disease is by no means rare among these animals in Algeria." (p. 77.)

The experience of the foreign legion in Algeria proved that the inhabitants of the south, as Spaniards, Italians, and Provençals, resist disease infinitely better than do those of the north, as the Germans, Flemish, and Alsatians, in whom diarrhœa and dysentery are easily induced, and assume the chronic form. This consideration becomes of importance in determining the departments whence recruits should be drawn. Another fact, already noted by Lind, and by Thevenot, in Senegal, is the danger of drawing troops from the marshy districts of Europe. Of fifteen men arriving in Algeria from Rochefort, scarcely convalescent from fever, all but three were rendered unfit for service after six months' residence during the best part of the year. Five died of dysentery, and two suffered from very severe hepatitis. In reference to the great comparative exemption of the *Arabs* from forms of disease which prove so formidable to Europeans, notwithstanding their exposure to many circumstances which are hygienically adverse, and their privation of so many things we consider essential, M. Haspel observes, that while allowing for the inuring effects of habit, we must not omit to notice the very large proportion of their children that perish, the most vigorous being, in fact, alone able to resist the mischievous influences they are subjected to. The nomadic mode of life of the indigenous inhabitants is doubtless favourable to this immunity; for during the prevalence of the hot weather, they migrate from the plains into the mountains, redescending these again on the approach of winter.

Acclimatization does not exert any beneficial influence, inasmuch as Europeans who have inhabited Algeria for ten or more years are as liable to dysentery as those who have just disembarked, while affections of the liver are more frequent and more severe in proportion to the length of residence. Those of tender age are very liable to disease, and on this account the bringing-up of young children in Africa is very difficult. Notwithstanding their greater weakness and susceptibility, European women are less liable to become diseased than men; although the Arab women, whether from the effects of climate, or the wretched and laborious lives they lead, undergo at a very early period a premature decay both in health and appearance. Notwithstanding that it is at an advanced period of life that the lower portion of the intestinal tube is most liable to become diseased, yet in Algeria men suffer more even from this cause at twenty than at forty; and, in fact, after thirty the endemo-epidemic affections undergo a sensible decrease in intensity.

Diseases of Algeria.—Although M. Haspel necessarily describes the various endemic affections—disease of the liver, dysentery, and intermittent fever—separately, yet he is most emphatic in maintaining the connexion of these various morbid phenomena as mere different forms of manifestation of the effects of the same morbific cause. This connexion has, indeed, been insisted upon by other writers on the diseases of Algeria, and

by various Indian practitioners; but we do not recollect having seen it more forcibly dwelt upon than in the pages of the present work. We quote a few passages, which, though forming rather a long extract, are full of interest, and exhibit a generalizing and philosophic spirit.

"All portions making up the pathology of Algeria are inter-dependent, the chapter which follows being only a continuation of that which preceded it. Here diseases are not superimposed, they are congeners, forming the interlaced links of the same chain. They intermingle with each other, are co-existent, and successive. Dividing the pathology of this country, as has been done, into a thousand distinct groups, which, though coming into contact, have no relationship with each other—devoting a volume, for example, to this disease, and a volume to that one—is only separating links from this long chain of maladies, breaking through the tie that unites them, and exhibiting but a poor comprehension of the true progress of events. . . . .

"In this piecemeal mode of description, the divisions are often arbitrary, the various influences unperceived, and the pathological genesis uncomprehended. Disease after disease is described in mathematical order, by a machine-like process, which, seeming regular, is only mechanical; and just as if the wave which precedes can be separated from that which follows, dysentery, intermittent fever, and hepatitis, are successively treated of with scarcely any notice of the tie that binds them to each other, and in ignorance that diseases, so different in appearance, constitute a whole, whose origin and influence do not admit of separation. In order duly to comprehend them, science should not isolate, but combine these elements. She must not limit herself to separate and purely material descriptions, as does the naturalist, but consider these complex morbid conditions as they present themselves for observation in the pathological picture, following them out in their progress, and studying them in their relations as a whole. This was the method pursued by Hippocrates. Physicians of all periods have more or less conformed to it; and it is to this we are indebted for the imperishable portraitures of diseases which Pringle, Sydenham, Stall, Hildenbrand, and others, have handed down to us. This was one of the special characteristics of their talent, which is at the present day, so to speak, unknown, amidst the infinity of details with which science is encumbered and oppressed. It was this artistic feeling which enabled them to construct with different figures an homogeneous indivisible whole, from which no one part could be abstracted with impunity. They so animated their portraits, and so well connected the varied diseases of man with the air he breathed, and the soil that fed and supported him, that the products of their pencil seemed possessed of but one soul, and to live a life in common—a single pathological harvest, sown at the same time, springing up in the same soil, gilded by the same sun, and watered by the same dews. In default of a more distinct and comprehensive term, I would designate this mode of procedure of the ancients as a *pathological sympsyche*—a term that may serve to indicate that indissoluble parentage which indicates certain morbid actions, of however different appearance, as branches of the same stem. . . .

"On observing the diseases of the province of Oran summing themselves up, so to say, in three well-defined pathological expressions, co-existing or confounding themselves with each other, alternating or transforming themselves into each other, following a progressively increasing course from spring-time to autumn, presenting, though differing in form, remarkable analogies in respect to the suddenness and violence of morbid congestions, their mobility, and relapse, and finally, all three engendering a special cachexy, I was struck both by the contrast they presented in their symptomatic physiognomy and general expression, and the relations which nevertheless seemed to unite and attach them to a common cause; merely modified in its intensity and by the reaction of the organism . . . .

"Although it is true that these diseases may be distinguished by marked characters, they nevertheless preserve a number of attributes in common, and are all at bottom marked by the ineffaceable seal of malarious influence. The successive trans-

formation of simple into malignant dysenteries, their frequent coincidence with pernicious fevers, of which they sometimes form the starting point; the asthenic character, invasion, type, duration, disappearance, and certain epidemic return of these last, declare the analogy of the origin of these different diseases with all the certainty of which medicine is capable. Moreover, as every one must have observed, there are years much more fruitful in fever than in dysentery, and *vice versâ;* but what is especially remarkable is, that when one of these diseases predominates, the other is of much rarer occurrence. Fevers, dysentery, and diseases of the liver, may also sometimes mingle and combine in the same individual, succeeding, expelling, or replacing each other in turn. To complete the analogy, we have to bear in mind the frequency of relapse without obvious cause daily observed in fever, dysentery, and diseases of the liver, and the identity of organic alterations, as ascites, engorgements, and chronic or acute inflammations of the abdominal viscera, which so frequently result from them. These are no separate portions, but an indivisible whole under the dominion of a single cause; and those who deny this truth, and endeavour to explain diseases so stable in their characters by ever-changing individual circumstances, are either misled by theoretical prejudices, or will not make use of their eyes." (pp. 9, 11, 39.)

Ideas of this kind, M. Haspel believes are especially required to be impressed upon those coming from the schools of France, where the localization of disease has been taught to an extravagant degree. We may, however, assert that a great change has taken place in that quarter, as well as in all others, since the days of M. Haspel's pupilage. The practitioner should arrive in Algeria as unfettered as possible by scholastic theories and prepossessions; for one of his first means of acquiring the art of distinguishing and treating diseases there, will be to unlearn much of that which has been taught him with such mathematical precision at home.

Diseases of the Liver.—Keeping constantly in view the connexion which these have with intermittent fever, and especially with dysentery, M. Haspel proceeds to detail his experience in reference to diseases of the liver in particular. He claims the merit of having first drawn attention to their frequency in Algeria; and particularly to the preliminary stage of hyperæmia of the organ, which plays so important a part in the apparently sudden occurrence of suppuration.

The author enters a preliminary protest against the doctrine that *heat* is the cause of these diseases, inasmuch as they are by no means confined to hot climates, and have much diminished in the province of Oran since the marshes in the vicinity of towns have been drained, and agriculture more attended to. In 1846, the temperature became so high at Oran that the heat rapidly dried up all the marshes, and arrested the development of miasmata, and the consequent hepatic disease. However, all marshy districts do not give rise to liver disease, for in 60 autopsies performed by the author at Bona, during a year's residence there, he found abscess of the liver, which is so often met with at Oran, of only very rare occurrence; and Dr. Grebois, who has practised in the province of Constantine for seven years, states that the hepatic form of intoxication is rarely met with there. M. Haspel suggests, that when the waters are in so bad a condition as they are at Oran, the toxic miasmatic influence may be more especially determined to the liver. He regards *sudden changes of temperature*, which are frequent at Oran, situated as it is between the sea and the mountains, as a more powerful occasional cause than great heat.

1. *Hyperæmia of the Liver.*—By omitting the study of diseases of the liver in their earliest stages, authors have often attributed to them a greater rapidity of course than is exactly correct; and abscess of the organ, from its obviousness, has been too often described as a distinct and separate affection, instead of a mere stage or consequence of pre-existing, unobserved disease. It is only by recognising affections of the organ in their early stages, that therapeutical procedures can be brought to bear effectually upon them ; and for this reason M. Haspel frequently insists upon the necessity of watching for apparently trifling symptoms referrible to the state of the digestive organs, which, if duly attended to, often indicate the existence of an unsuspected, but rapidly advancing and dangerous malady. Hyperæmia may present itself in either an acute and active form, or in an hypostatic and passive form; and we subjoin M. Haspel's distinctive characters of these :

"1. The *acute or active form* is developed in the months of June, July, and August. 2. The dry and arid heat of marshy countries is its most prominent cause. 3. It especially attacks those of robust habits, bilious temperaments, and who have recently arrived from Europe. 4. It is frequently preceded or accompanied, especially at first, with a slight phlegmasia of the upper portion of the alimentary canal. 5. The crises are especially effected through supra-diaphragmatic organs (as by parotiditis, deafness, nasal hæmorrhage.) 6. It commonly terminates by resolution, sometimes by a hypertrophy of the liver, which is of common occurrence in Oran, and only rarely by suppuration. 7. The lesion may exist in any part of the organ. 8. General and local bleeding and purgatives are sometimes of service. Stimuli and tonics are contraindicated.

"1. The *hypostatic or passive form* is especially developed during September, October, and November. 2. It is most commonly produced by the damp air, loaded with miasmata, which in Africa so suddenly succeeds the heats of summer; by repeated attacks of intermittent fever; and by dysentery. 3. It attacks persons of lax habit, with large visceral development, and especially those of feeble temperaments, whose powers have become exhausted by their labours, excesses, chronic disease, diarrhœa, dysentery, obstinate intermittents, and a long residence in Africa. 4. It most frequently coincides with an irritable state of the lower portion of the digestive canal, so that it is often difficult to determine which was the part first attacked. 5. The sub-diaphragmatic organs especially suffer, in the form of diarrhœas, dysenteries, or hæmorrhoids. 6. Suppuration constitutes the most frequent termination. Ramollissement of different colours is often observed in both liver and spleen. Congestion is often present without hypertrophy. 7. The right lobe is affected by preference. 8. Depletion can rarely be used with advantage. Mild aperients are suitable in only the acute form, tonics and bitters being especially indicated in the chronic." (p. 121.)

2. *Hepatitis and Abscess of the Liver.*—The discussion of this topic occupies one-half of M. Haspel's volume, and he believes that he has supplied what, in spite of the number of excellent works on the diseases of the liver, did not exist—a complete and faithful history of hepatitis in its various shades of manifestation, which may serve to render its detection comparatively easy. In ordinary treatises, the chapter of causes is rich to abundance, as respects the number of these; but when we come to examine into their true morbific import, the disappointment is great. The symptomatology, again, is vague and obscure, so as to render the distinguishing the disease from diseases of various other organs difficult; and, as a consequence, affections of the heart, kidney, spleen, stomach, lungs, and pleura, are frequently mistaken in practice for those of the liver.

Moreover, from the rarity of the occurrence of *abscess in the liver* in France, few opportunities present themselves for submitting it to effectual observation. Having had ample opportunity of studying this affection in Oran (to which province it is almost confined, even in Algeria), M. Haspel has been enabled to collect many cases, twenty-nine of which he relates in the present work, commenting upon their salient points, and especially upon their dysenteric complications. For the purposes of description he arranges these cases of suppuration in two categories—one, in which the pus is infiltrated or disseminated in the substance of the liver; and one, by far the most frequently observed, in which this is accumulated in the form of abscess, properly so called. These latter are again subdivided, according to the mode in which the pus is disposed, whether towards the surface of the organ, or imbedded in its substance, or the manner in which it obtains exit, when discharged, through the abdominal parietes, by the respiratory passages, into the pleura, the peritoneal cavity, or some part of the alimentary canal.

Instructive as several of these cases are, we have no space to quote any of them, and shall content ourselves with an abstract of the author's general statement:

Of *idiopathic acute hepatitis*, M. Haspel, in ten years' residence in Algeria, has only met with two examples. In one of these death took place on the sixteenth day, and nine abscesses were found diffused through a friable parenchyma; and in the other, matter, which in most cases requires at least from three to four weeks to form, was found after death on the eleventh day. In both cases active depletion was resorted to, and in both a complication of hepatitis, gastro-duodenitis—often met with in Europe, but rarely in Algeria, existed. In the case of a soldier who suffered from violent external injury, the liver had become the seat of an enormous abscess by the twelfth day. Hepatitis occurring in persons in good health, and uncomplicated with dysentery, is rare.

The other cases belonged to the *chronic* form of hepatitis, and the following are some of M. Haspel's observations upon their *pathological anatomy*. The liver, though occasionally, in partial hepatitis, preserving its normal size, in general exceeded this by twice or thrice. The portions surrounding the inflamed parts were frequently gorged with dark blood, the spots themselves being firmer, but friable. The dark-red colour could not be removed from the organ by washing, and formed a true sanguineous infiltration. In the midst of the inflamed portions, incipient suppuration could be observed. In other cases, in place of this sanguineous injection, the liver was pale, and traversed by but few vessels; its reticulated parenchyma, separated into areolæ by numerous white lines, seeming composed of a yellowish consistent substance, deposited within these areolæ, and assuming a granular form. These anæmic granulations were soft and friable, as if their cohesion was diminished by the presence of fluid. In some cases the granulations became indurated; and either the condition of ramollissement or induration may co-exist with a red, blackish, or slate coloration. Abscesses are more frequently found deepseated than superficial, and although any part of the organ may become their seat, they most often occur in the posterior edge and right lobe—existing in this, as compared with the left, in the proportion of 30 to 1.

The deep-seated are slower and more insidious in their progress, although no absolute rule can be laid down. The superficial are, in general, much smaller, and consequently more curable, while the deep-seated are sometimes enormous, occupying the entire organ. The pus is usually inodorous, but in a few cases it is very fetid. In appearance it is white and homogeneous, like pus in other parts of the body; though occasionally it becomes reddened by the discharge of portions of the hepatic substance, which, in some cases, takes place to an enormous extent. This wine-lees colour, which has been stated to be characteristic of hepatic pus, is, on the contrary, an exception. As in several cases well-marked *cicatrices* have been observed, there can be no doubt of the occasional curability of these abscesses. The *bile* in these cases presents no constant characters, it being sometimes a mere colourless mucus, and at others dark, thick, and sticky. It has seemed to depart less from its normal characters in cases of abscess, than when the patient has died from acute disease, especially dysentery. The *stomach* and *small intestine* were usually found free from disease; while almost in every case the *large intestine* was diseased, as will be mentioned when treating of dysentery. The *spleen* undergoes great increase of size, softening and engorgement, but has never been found to contain abscess. Few organs undergo less alteration in persons who die from this disease than the *encephalon,* notwithstanding the intimate sympathy which prevails between this organ and the liver. The *lungs* are usually found engorged, and may undergo compression from the encroachment of the enlarged liver. *Pleurisy* is a common complication; but diseases of the *heart* are rarely met with, and then only in cases in which obstinate intermittents have prevailed. The *urinary organs* rarely offer any lesions —the co-existence of disease of the liver and albuminous nephritis so often met with in Europe not being observed here.

Symptoms.—Most authors speak of the disease as of easy detection; but at the bedside, many of their characteristic signs are found more or less defective. *Pain* may be entirely absent. It is not as a general rule an early symptom; and when present may exhibit every degree upwards, from a mere uneasiness which scarcely attracts the patient's notice. The pungent pain which accompanies inflammation of the upper edge of the liver —the *hepatitis pleuritica* of Sauvages—is referred by the patients to the right side of the chest, and that with the more confidence, as they suffer also from cough and dyspnœa. The dyspnœa is by no means always proportionate to the amount and severity of the inflammation. The pains which prevail in the loins and right shoulder, the health not seeming otherwise deranged, are often mistaken for rheumatism, until their fixity and intensity too late betray their hepatic character. Temporary cessation of pain has frequently been mistaken for an amendment that had no ground in reality. *Icterus* is not of frequent occurrence, and when present, usually is so only at the commencement, and in a feeble degree, affecting only the scleroticæ. When present, it seems to have arisen from very large abscesses having exerted compression on the biliary canals. In several cases, in which nearly all the substance of the liver had been destroyed by suppuration, it was absent; for bile had ceased to be secreted, and the gall-bladder contained only a whitish mucus. Although confirmed icterus is rare, a pale yellowish colour of the countenance is common; but

this is a usual appearance in congestion of the liver and spleen after intermittent fever. The *digestive organs* are those which undergo most disturbance; and frequently observation of some of their disorders, as a loaded tongue, bilious vomiting, &c., first directs attention to the important actions taking place in the liver. But, on the other hand, their presence is by no means essential; and there is no necessary relation between the condition of the tongue and that of the liver, M. Haspel's investigations not leading him to attach the same importance to the state of this organ as Annesley does. Nor has he met with the salivary flux, independently of the action of mercury, mentioned by several writers. Diarrhœa and dysentery are the commonest of complications, and so intimately have these diseases been found connected, that the affection of the large intestine and that of the liver seem to have alternately acted as cause and effect. It is an error, however, to state that these intestinal affections usually precede the development of the hepatitis. In relation to the *respiratory* organs, we have various degrees of *dyspnœa* produced by the encroachment of the liver upon the cavity of the chest, this being constant in some cases, occasional in others, suffocative in some, where great hepatic congestion is present, and in a few cases simulating asthma. In many cases a short dry *cough* prevails, the hepatic cough of some writers, and the *tussis ferina* of Hippocrates; and although auscultation proves the lungs themselves to be unaffected, too much security must not be indulged in from the examination, as sooner or later the pulmonary tissue may re-act, and a mere sympathetic bronchial action become converted into a true pneumonia. Affections of the lung or pleura may also be induced by the spreading of the inflammation by contiguity. Hæmoptysis is frequently induced. *Hiccough* was a rare symptom in the author's cases; it is met with as often in dysentery as in hepatitis. A full and rapid *pulse* may be met with in the rare examples of acute hepatitis; but of the more chronic forms it offers little indication. In the majority of cases it is rather small and slow than frequent, until the later period of the affection, when *hectic fever* becomes developed, which it does sometimes quite unexpectedly, amidst such mild symptoms, that these seem rather to denote the commencement of a case, than a period so near its termination. When *blood* has been drawn, it never presents the inflammatory crust seen in pneumonia. Although in acute cases there may be much *cerebral disturbance*, this is not observed in the chronic form. It is when hectic manifests itself that the *nutritive functions* suffer, until which period the strength and substance may have been well preserved; and the author relates the case of a man suddenly dying from the rupture of a large abscess into the respiratory passages, who presented every sign of flourishing health. This is the more remarkable, as among the *animals* which become seized with this affection, extreme emaciation is one of the earliest and most striking symptoms.

Diagnosis.—This, in the early stages, where only it is of much avail, is surrounded with difficulties; and in many cases can only be based upon local examination of the hepatic region. We would observe, that notwithstanding the author's severe denunciations of prior writers, his own instructions seem to us to be none of the fullest, constituting rather a series of hints upon the liability to deception, than much positive additional information as to

the means of detection. There is, doubtless, much difficulty inherent in the subject; but this should have taught more allowance for the shortcomings of others.

Prognosis.—This is very unfavourable, for the disease is rarely combatted while in the state of mere hyperæmia, and is commonly complicated with the other dangerous affections of warm climates. An alternation of favourable and unfavourable symptoms in the progress of the case is to be distrusted, giving rise, as it does, only to fallacious hopes. The only form of abscess which holds out a probable hope of cure, is that which exists at the surface or lower border of the organ, and which thus may present itself under the walls of the abdomen. Patients who have apparently recovered from hepatic disease, have yet died, months or years afterwards, exhibiting the symptoms of purulent resorption.

Treatment.—*Venesection* is only admissible in very exceptional cases; but the application of leeches to the part or to the anus is advisable in almost all. Still if these do not prove of utility, they must not be persisted in beyond the first few days; and their repetition is less decidedly indicated by the pulse, than by the state of the local symptoms and the existence of complications. In some cases the attendant hæmoptysis may call for depletion. If the existence of suppuration be already suspected, great care is required in the abstraction of blood, lest the necessary strength be impaired. Convalescence requires watching; and on the recurrence of pain, or disturbance of the digestive organs, mild antiphlogistics must be resorted to. M. Haspel believes that *emetics* exert a beneficial effect in aiding the disgorgements of the liver; but in enfeebled subjects they must be used with caution. They do not operate so easily or so effectually on the engorgement in the spring, as in the autumn, when the bilious condition prevails. Of all the *purgatives* the author gives preference to *calomel*, of which he entertains a high opinion, giving it in 15 or 30-grain doses, and preceding its use by a mild laxative, in order to prevent salivation, which he regards as injurious. He prefers friction with mercurial ointment, also, to that with iodine, from which he has derived no benefit. When the antiphlogistics are of little avail, *large blisters* are often of great service, and this even in cases which seemed evidently doomed to go on to suppuration. Tumefactions of the liver, also, occurring in subjects apparently in good health, are sometimes dispersed by their means. *Change of climate* is sometimes a very efficacious measure.

When the threatened *abscess* may be supposed to be of small size, and the subject is robust, its extension may be circumscribed by the cautious use of antiphlogistics; but in weakly subjects, some form of issue should be substituted. When, however, hectic becomes established, active revulsion of any kind only adds to the danger of purulent resorption; and every means of supporting the patient's strength must be resorted to. M. Haspel strongly insists upon the necessity of opening those abscesses which are accessible to the surgeon; and of the various means that have been recommended for accomplishing this, he prefers the use of the caustic potass as favouring the production of adhesions preventive of effusion into the cavity of the abdomen. The chief objection to this procedure is its slowness; and when, from the urgency of the case, a more rapid one is necessary, the plan of M. Begin, of carefully cutting down to the cyst of the abscess, and

opening this at the expiration of three days, when adhesions with the external wound have had time to form, should be substituted. However well the operation may be executed, it does not always save the patient's life, for the collection may prove larger than was anticipated, or the complications of the disease may be of a fatal character. M. Haspel considers that the operation should be resorted to as soon as the presence of pus is ascertained; and that cases have been lost in waiting for too evident fluctuation, and from the timidity of the surgeon. Even where the abscess is large and the powers exhausted, the operation should be performed, as giving the patient his only chance, especially since some cases have recovered under apparently hopeless circumstances. The author does not approve of injecting the cavity of the abscess with tepid water, or any other fluid—at least prior to its becoming encysted.

3. *Atrophy of the Liver.*—M. Haspel wishes to draw attention to a form of atrophy of the liver met with in Algeria, arising from the circulation in the organ becoming impeded by the retraction of its component parts. As a consequence of its repeated congestion in dysentery, and especially in obstinate intermittents, the parenchyma of the liver, and sometimes that of the spleen, may acquire the consistency and hardness of cartilage, undergoing very great diminution in size, and giving rise to symptoms of hepatic derangement. The disease is of frequent occurrence in Africa, and M. Haspel relates the particulars of three of the cases.

It usually commences obscurely, and has already made considerable progress before it has excited attention. A disturbed state of the digestive organs, and a feeling of debility, which is explained by no obvious cause, are the earliest symptoms. After one or more months, ascites appears, and this usually prior to œdema of the extremities. Ascites is indeed often the only symptom that excites attention; and when this appears without obvious cause or distinctive symptoms, this disease is always to be suspected. Attacked at an early period, the disease may, by preventing congestion, be arrested; but it is seldom recognised before all means of treatment are unavailing.

4. *Ramollissement of the Liver.*—Although authors have admitted an inflammatory and non-inflammatory ramollissement of the liver, they have not offered any distinctive description; and it is M. Haspel's desire in some measure to supply the deficiency. All forms of it are dependent originally upon partial or total congestion of the organ; and of these forms M. Haspel describes three:—ramollissement, with infiltration of red blood, which is not always a product of inflammation; ramollissement, with infiltration of pus; and ramollissement with serous infiltration. There is also another remarkable form of diminished cohesion of the texture of the organ, termed by Ferrus and Bérard *dry,* and described by Louis, in his work on typhoid fever, as *friable.*

The first-named group (of which several cases are given) of infiltration of blood into the liver, or *red ramollissement,* may be produced by various causes, such as hepatitis, a traumatic rupture of the capillaries, or a general hæmorrhagic disposition due to a change in the fluids, as in the scorbutic state. In the second form where *pus* is *infiltrated,* the *yellowish granular appearance* observed in some cases is due to the abnormal development of the fibro-cellular tissue, which constitutes the areolæ. It almost always

precedes the formation of abscess, and may be usually observed around these after their formation. It is in this cellular tissue, surrounding the elementary tissue of the organ, that the most important pathological changes take place. The most frequent one is a reddish or brownish infiltration, as in commencing phlegmasia, when the limits of the cells and the cellular tissue can be no longer distinguished, and the parenchyma of the organ presents a uniform appearance. If in place of blood being deposited in the areolæ of the cellular tissue, these contain a yellowish consistent matter, resulting from an abnormal secretion of the tissue, an appearance of either hard or soft granulations is produced; and the liver presents a yellow, anæmic, granular aspect.

The distinction between the inflammatory ramollissement and the *third* form, or the *serous non-inflammatory ramollissement,* is not always easy. In a paper formerly published, M. Haspel declared his conviction that ramollissement never resulted, except from phlegmasia or from the rupture of capillary vessels and consequent effusion of blood; but additional experience has modified his views, and he now presents examples of the cases which induced the change. They are chiefly found in instances of pernicious intermittent, in which watery exhalations in the form of diarrhœa, cerebral effusion, and copious sweats, are common—and constitute examples of the dropsy of the hepatic parenchyma of Glisson. In all the examples of pernicious fever in which this affection of the liver has been observed, there has been found great poverty of blood, with ramollissement of, or serous effusions into, other viscera. In both the first and the third forms of ramollissement, an afflux of blood had preceded; but in the one case an abundant, rich, plastic, highly-coloured blood was poured into the areolæ, while in the other it was only a pale, serous, watery blood. Thus we may have the same origin (local sanguineous congestion), the same danger, the same symptoms and general phenomenon (febrile paroxysm)—the anatomical characters alone establishing a difference, and these due to the remarkable change which the blood itself has undergone. This form of ramollissement is, in fact, only met with in pernicious or typhoid fever, and in scorbutus—all diseases in which evident alteration in the blood has taken place.

" According to the cases we have related, this ramollissement may occur in two modes: 1st. It may suddenly acquire its highest degree of intensity, and give rise to a train of symptoms that may rapidly lead to death; 2nd. In other cases it only reaches a certain amount of intensity, and that gradually. The loss of consistence is sometimes only manifested by a pasty softness of the tissue; and at others, as Andral observes, the parenchyma becomes truly liquefied, presenting the appearance of having been long submitted to maceration; or, again, the softened part takes on a greyish or dead-leaf colour, the gall-bladder containing only a viscous serosity. In all cases it is a general, diffused infiltration—the organ often presenting the appearance of a homogeneous *detritus.*" (p. 388.)

The notice we have bestowed on M. Haspel's work shows the estimate we have formed of its contents; and in our next number we shall give an analysis of his second volume, which has just made its appearance.

Hydropathy as Applied to Acute Diseases.   Illustrated by Cases.   By
    T. B. ARMITAGE, M.B. Lond., M.R.C.S. — *London,* 1852.   8vo, pp.
    178.

DR. ARMITAGE has chosen a dangerous title for his maiden publication,
unless he desire to appear in the character of a candidate for the manage-
ment of a water-cure boarding-house.   It seems to indicate that he is a fol-
lower of Priessnitz, and a postulant for fellowship with the motley crew who
practise the healing art under the banners of that successful charlatan.
Professedly, however, the object of the publication is far different, inasmuch
as it is declared to be the resuscitation of Currie's method of treating fevers
by cold affusion.   Dr. Armitage observes, that

"Dr. Currie, by the publication of his *numerous* cases fifty years ago, placed
the general advantages of the hydropathic treatment in fever beyond a doubt.   No
one can read his 'Medical Reports' without being satisfied that he had more suc-
cess in fever by cold water affusion than is obtainable in the present day by all
the aids which medical science, confessedly so greatly advanced since his time,
can supply.   My chief desire has been to remedy, however inadequately, a want
which I felt in reading those otherwise valuable reports; and if I have pursued a
closer investigation of the immediate effects of water treatment, and have given
more definite indications for its use, the object I had in view will have been
attained." (p. 23.)

Dr. Armitage ought to be aware that in applying the term " hydro-
pathic" to Currie's treatment, he is wronging the memory of that honour-
able physician.   It is a term which savours too strongly of quackery to be
pleasant to the ears of the upright practitioner.   It is a term strictly and
exclusively belonging to the quacks' vocabulary.   If it were not the
unmeaning and absurd word it is, and truly expressed the therapeutic use
of water, still it does not follow that it should be adopted; on the contrary,
the fact that that therapeutic use, so long known and so long esteemed by
the profession, has been so designated by a band of empirics, is a sufficient
reason for its rejection.

Dr. Armitage has, we think, committed another indiscretion in pub-
lishing the subjoined.

"I mean no disparagement to the great practical skill which Priessnitz un-
doubtedly possessed in the *treatment* of disease, by calling attention to the fact
that his want of any scientific knowledge rendered him utterly incapable of forming
a correct *diagnosis.*" (p. 16.)

How an incorrect diagnosis and such correct treatment as is charac-
terized by the phrase " great practical skill" can co-exist, is beyond our
apprehension.   It may be readily granted that the man knew pretty well
the cases which might be benefited by regimen and the internal and external
use of water; but as to those in which his method was useless, we suspect
he was quite at sea, and his " great practical skill" was only shown in
avoiding gross blunders in their management.   Dr. Armitage appears to
have a predilection for the system, as he informs us, that " at the present
moment I am subjecting myself to active hydropathic treatment, for the
alleviation of a complaint that I have but little hope of removing either by

medicine or regimen." Now to this no objection can be fairly urged; but we doubt very much the propriety of the following:—"To many invalids, also, I would recommend a residence at some well-regulated hydropathic establishment, in preference to any other mode of cure." We are very decidedly of opinion that the practitioner should prefer any other possible mode of carrying out a hydriatic system of treatment, and never sanction the establishments referred to. Dr. Armitage will probably think we are very hypercritical; but if he will pay a visit to the Ben Rhydding Hotel and Boarding-house, he will find a text-book for the inmates, which contains statements sufficiently decided to warrant us in looking upon books like his with suspicion. Dr. Macleod, the hydropathic super-intendent, has undergone the profitable infliction of martyrdom for his adherence to *homœopathy.* As a fellow of the Royal College of Physicians of Edinburgh, Dr. Macleod has committed breaches of professional etiquette of a character sufficiently serious to render certain resolutions of the college applicable to himself. Having identified himself with the aqueous and globulistic quacks, the fellows of the college pressingly invite him not to identify himself any longer with them. This calls forth a letter from Dr. Macleod, addressed to Dr. J. Y. Simpson, the president of the College, but evidently intended for the perusal of the customers of the Ben Rhyd-ding Board and Lodging house. If we were to accept the writer's account of himself as a true statement, he is a paragon of virtue rarely to be met with; so sound in judgment, so diligent in study, so zealous and courageous in the pursuit of truth, so *totus teres atque rotundus.* He began his medical career (he tells his readers) as "an ardent student," adding "and during nine months each year, and for years together, I was an occupant of the dissecting room, on an average, six hours a day. I laboured with zeal in the pathological department of the Royal Infirmary for upwards of three years. I performed nearly all the weights and measurements made there during that period. I devoted much of my time to microscopic studies. I went round the wards of the hospital when the physicians were not there, regularly for several years, watching and examining for myself every case of interest and practical importance; so that I might be able to comprehend, with as much completeness as was possible, the progress of diseases, their varied phases and tendencies," &c. There never was such a practical, such an unwearied, such a pains-taking student—if we may believe Dr. Macleod's account of his own conduct. And what was the conclusion this Sydenham *secundus* came to? That the practice pursued was "DANGEROUS, FREQUENTLY UNDERMINING THE CONSTITUTION FOR LIFE, AND SOMETIMES HASTENING DEATH." Dr. Macleod "being," as he says of himself, "a searcher after truth, and never caring much from whom it came, provided I could discern and appropriate it," proceeded to Vienna to study and be convinced of the "truth" of homœopathy and hydropathy. Returning home *re infectâ,* he, quite characteristically, "longed for the appointment of Physician to the Royal Infirmary," but quickly discovered that Edinburgh was no place for him. At this "critical juncture" the situ-ation at Ben Rhydding became vacant, and our adventurer, when he "dwelt on the rare opportunities it might afford for the development and promul-gation of Truth," was moved to offer for the situation, "with little regard to pecuniary remuneration," and so got the "inestimable boon." There is

a good deal of truth, probably, in one or two of the latter statements; for when stripped of the absurd sentimentality in which they are wrapped, they indicate a sharp eye for " the main chance."   But as to the rigmarole description of his virtuous truthfulness, we take leave to express our doubts of its correctness, for the simple reason, that in the same pages he has slandered his brethren, and wilfully misstated ourselves.   We subjoin the following note, having reference to an article in vol. vii. of this journal, which we find at the foot of page 12 of Dr. Macleod's " Letter":

"I point, for the assurance of our timid hydropaths, to an article in the last number of the 'British and Foreign Medico-Chirurgical Review,' on the use of baths—an article slight in itself, but sufficient to mark the direction of the wind. It is mainly an analysis of a volume by an excellent and instructed American physician and hydropath—Dr. John Bell, of Philadelphia—bearing the prudent title of 'A Treatise on Baths.'   Taking his cue from the title, the reviewer ventures to allege that the doctrines in question had long influenced medical practice—a delusion readily forgiven him, since he would help the said doctrines to be recognised as a part of authorized practice now.   He assents, in the course of his remarks, to almost every important advantage claimed by hydropathic practitioners for the use of water—among others, to its efficacy in scarlatina, typhus, &c.   He assigns the credit, not, of course, to unlucky hydropaths, but to certain spongers; forgetting, however, to ask, or at least to answer, two questions—1st, whether the said spongers would not have sponged more largely," &c. &c.

It is obvious, from even a mere glance at the article referred to, that this letter, addressed to the president of the College of Physicians of Edinburgh, was not really intended for his instruction, but for the perusal of the non-professional people who board at Ben Rhydding.   How otherwise could Dr. Macleod have ventured to pen the passage quoted—so utterly at variance as it is with truth ?   The " credit" was " assigned" to Floyer, Baynard, Cheyne, Wright, Jackson, Currie, Gerard, Kinglake, Good, Forbes—men who would have felt degraded by the designation " hydropath," and who advocated cold bathing before that class of aqueous charlatans existed.   The word " sponger," or " sponging," is never applied by us, directly or indirectly, to their methods of using water to the surface, and only occurs once, and that exceptionally, in the whole article of twenty-six pages.   That once is when we refer to *our* personal experience;— " whenever we feel an aversion (which may be termed instinctive) for the bath, we are content with *cold-sponging*"—are the words used.   Dr. Macleod must excuse us expressing a confident conviction that his self-laudations are as utterly unfounded as his assertions about the " spongers." If he can venture on statements so outrageously at variance with the truth, on occasions when he can be confronted, what will he not say when he cannot be confronted?   Undoubtedly he is safe from detection by those amongst whom his " letter" is intended to circulate, and his profession of love for truth may win *their* confidence in his bold assertions; but still it is equally certain that his sin will find him out, and public opinion adjudge his case rightly.

This conduct of Dr. Macleod's is very instructive to the profession, as to the best method of warring with quackery.   It is only through their pecuniary interests that any impression can be made on charlatans, or on those practitioners who desert the ranks of the legitimate body.   Abuse they welcome, but any measure which will effectually weaken their craft

meets their instinctive opposition. In penning the mendacious note we have quoted, Dr. Macleod knew well that the general adoption, by the profession, of the principles laid down in that article, would eventually extirpate the special establishments, like Ben Rhydding; because water as a therapeutic agent would then be placed within the reach of all classes. It would be equally applicable by the ordinary practitioner at Brighton as at Cheltenham, at Harrogate as at Scarborough, in the city or village as at Ben Rhydding. Nothing is more certain, we think, than that all special methods of treating disease are essentially charlatanic, and inevitably infect the profession with quackery. Hence we would have the profession generally and well informed as to all the methods of using water, which have been found the most beneficial and the most convenient, and as to the diseases which may be so treated with the most probable success. Dr. Macleod instinctively perceived, on reading our article, the dangerous results to the hydropathic craft which would follow from our views; and hence that eminent " Searcher after Truth" (as he characterizes *himself*), unguardedly manifested his feelings in a feeble explosion of calumnious misrepresentation.—Holding these opinions, we welcome all additions to our knowledge of the therapeutic uses of water, and gladly present to our readers a short analysis of Dr. Armitage's book.

The writer travelled on the Continent, towards the close of the year 1849, for the purpose of visiting all the great hospitals, and arrived in Berlin in January, 1850. Having attached himself to the Charité Hospital there, he obtained the opportunity he had much desired of testing Currie's method of treating typhus fever, Schönlein having given permission to his assistant, Dr. Traube, to carry on experiments with this view during the vacation. Previously to commencing these, Dr. Armitage paid a short visit to Gräfenberg, to be initiated into the mechanical processes used there. Currie's method had already been successfully practised at the hospital by Horn, when, in 1813, the retreating French, on their road from Russia, left typhus everywhere in their track. Dr. Armitage adds—

" Something of this method of treatment has been, ever since Horn's time, continued at the Charité, but the administration of the baths was left too much to the discretion of the bath servants and nurses, so that at the time when I arrived at Berlin, the method had so degenerated that it could scarcely be recognised. A very powerful douche had been substituted for the much more troublesome practice of cold affusion. I have frequently seen this allowed to fall for several minutes together on the head of the unfortunate patient. It is no wonder, then, if such a method should frequently be unsuccessful, and that the whole method should fall into disrepute. I have been told by the bath-master, that he has seen several cases of death while the patient was actually under the douche." (p. 7.)

Not only fever, but Asiatic cholera, and cases of delirium tremens and thoracic and abdominal inflammation, were treated by baths and affusions, or the wet-sheet. The whole number of cases of typhus treated, while Dr. Armitage was at the Charité, was sixteen. Six of these were treated on Schönlein's plan of heroic doses of calomel, of whom two died, having however, undergone a vigorous aquatic process before death: the remaining ten cases (two being very mild) were treated by cold-water applications, and recovered. Small quantities of wine and bark were sometimes administered during convalescence. Previously to mentioning particulars, we

will note the methods Dr. Armitage practised.   And first as to the indicants and contra-indicants.

" Cold baths must never be used in acute cases, unless the temperature of the body, as measured by the thermometer, is above the standard of health.   If the temperature is abnormally high, but any feeling of chilliness exist, the cold affusion is contra-indicated, and though some form of bath may sometimes be used in such cases, great caution is requisite in its employment.   If the powers of the system are so far reduced, that it is doubtful whether they will re-act sufficiently, no form of bath is to be used.   When the patient's body is bathed in a general perspiration, no form of bath must be used.   To this last rule I believe there may be exceptions, and partial perspiration is, as I have repeatedly convinced myself, no contra-indication to the use of the bath, provided the rest of the body be hot and dry, and there are no other reasons against the bath." (p. 54.)

The temperature of the adult human body, according to Dr. Armitage's observations, varies from 37·6 to 37·8 C. (99·6 to 100·04 F.)   If the temperature be permanently as high as 38·5 C. (101·3 F.), this indicates fever, and unless there be contra-indications, some form of cold bath may be applied with advantage.   How many individuals Dr. Armitage made his observations upon, and the circumstances of each, do not appear, so that his statement must be received with some reserve.   The particular forms of bath were, first, *affusion*, as practised by Currie.

" The patient is placed naked in an empty bath, and while so seated, several buckets of cold water are dashed from a height of one to three feet, or even more, on his head and chest.   The temperature of the water employed is generally about 40° Fahr.; but the exact temperature must be determined by the symptoms of each individual case.   The colder the water is, and the greater the height from which it is poured, the more stimulating is its effect.   In great stupor, therefore, the water should be very cold, and the height from which it is poured should be great.   The cold affusion so applied is indicated, where, along with considerable elevation of temperature, there is great stupor, and little irritability of the nervous system.   Its immediate effect is to diminish the temperature of the patient, frequently by 2° Fahr. or more.   The frequency of the pulse and respiration is also much diminished, and the greatest diminution does not follow the affusion immediately, but takes place at a period varying generally from half to three-quarters of an hour after it. The duration of this abatement of all the symptoms of fever varies with the case, but two or three hours may be assumed as the average in severe cases. During the affusion, the tongue, if before dry, almost invariably becomes moist and soft.   The stupor diminishes, and sometimes disappears altogether during the affusion, and seldom attains its former intensity for the next twenty-four hours. Sleep usually follows, as a consequence of diminished fever, and a critical perspiration sometimes breaks out after the patient has been replaced in bed." (p. 56.)

The immediate effects of the cold affusion on the temperature, pulse, and respiratory movements, were found to be as follows, in a case of fever so treated; in other instances the results were the same.   The date was September 21st, 1850.

" At 4.41 P.M., four buckets of water were poured over a man, named Hauston, aged 21, a cooper by trade, strong-built and muscular, and complaining since September 4th of the symptoms of fever.   Forty minutes previously to affusion his pulse was 90, respiratory movements 36, temperature in axilla 40·2 C. (104·3 F.)   He was affused, dried, and re-placed in bed, in two minutes—4.43. At 4.48 the thermometer was placed in the axilla: at 5.12 it was noted to be 102·5 F., and the same at 5.35.   The pulse and respiration were as follow:

Hour.			Pulse.		Respiration.
4h.	51m.	....	95	....	33
4	55	....	92	....	27
5	0	....	88	....	24
5	13	....	86	....	29
5	38	....	76	....	30

Cold affusion is contra-indicated in those cases in which there is great irritability of the nervous system; for whenever Dr. Armitage applied it in such cases, he found it was followed by increased irritability, restlessness, and loss of sleep.

" The *shallow bath* is a milder form of affusion, having similar results. It is a bath about six feet long, with a depth of water varying from five to twelve inches. The temperature of the water varies from 60° to 80° Fahrenheit. In this the patient is placed in a sitting position, with the lower extremities consequently covered by the water. They are constantly rubbed by an assistant, while water from the bath is poured gently over the head and trunk, from a pitcher. This operation is occasionally interrupted, and the trunk is well rubbed by an assistant, who wets his hands in the water of the bath. The patient is kept in the water a variable time until he is sufficiently cooled, which must be decided by the physician, according to the appearances during the bath. . . . . This bath is indicated in cases in which stupor is combined with a degree of nervous irritability which would counter-indicate the cold affusion. It will be found that women seldom bear the cold affusion, and that with them, therefore, this bath must be generally substituted for it." (pp. 58, 59.)

The *warm bath* (93° to 95° Fah.) is a cooling agent. A patient after being kept up to the chin in a bath at 95° for forty-two minutes, was found to have lost one degree Cent. of heat; or, in other words, the temperature in the axilla fell from 105·17 Fahr. to 103·37, or 1·8° Fahr. Dr. Armitage observes, as to its general effects:

" The patient during the bath feels himself very comfortable. The distressing sense of feverish heat diminishes. After the bath, at an interval which I have known to vary from five minutes to half an hour, *shivering* generally occurs, and lasts sometimes a considerable time; when this has ceased, the patient generally falls asleep, and all the feverish symptoms are found diminished."

The cold affusion and warm bath may be combined by pouring cold water on the head of the patient while immersed to the neck in warm water. He is held down, and a stream of cold water directed slowly over his head. The quantity of water affused must be determined by the nature of the case. Dr. Armitage has used a maximum of forty buckets, and a minimum of ten. The temperature of the water in which the patient sits must be kept up, by occasionally letting it off and adding fresh warm water. It is very useful in delirium tremens when the febrile phenomena contra-indicate the use of opiates. Its results are thus described.

" After the patient has been placed in the bath, he usually remains quiet until five or six buckets of water have been poured over his head. The expression of his countenance will then indicate very clearly that the cold to the head causes him severe pain, and he will make a struggle to get out of the bath. He must, however, be kept in, and the superfusion continue until he becomes quiet and more rational. This improvement of condition is generally noticed immediately after the bath, but it often requires five or six repetitions before sound sleep is produced. When this result has been obtained, the patient may be considered out of danger, for he awakes refreshed, [and] rational, and generally calls out for food. This form

of bath is equally useful in the delirium resembling delirium tremens, which comes on sometimes in the course of febrile diseases." (p. 65.)

Dr. Armitage cautions against an indiscriminate use of the cold affusion, and points out the necessity of using it with the most watchful and jealous attention. Great nervous irritability contra-indicates it.

"The cases in which alone the affusion should be employed are those in which, besides increased heat, there is also considerable stupor ; and patients in this state are far too lethargic, and too little alive to what is passing around them, to feel any alarm at the preparations. While the stream of cold water is descending on them, they no doubt do feel alarm the first time that the affusion is administered ; but having once experienced the pleasurable sensations produced by it, they will gene-rally, if sufficiently rational to express any wish for or against, of their own accord request a repetition. This was also remarked by Dr. Currie ; and Dr. Horn observes, that his patients very willingly submitted to it, after they had once felt the pleasurable sensations immediately following its use." (p. 70.)

Dr. Armitage thinks sponging of little use in those cases in which the use of water as a stimulant to the nervous system is indicated, because it can-not give a powerful shock. It is also much more likely to induce catarrh than baths ; still it is a useful cooling remedy.

Dr. Armitage states some practical precautions necessary to be taken in making thermometric observations on patients treated by affusion or bath-ing. He thought it requisite to have a thermometer constructed expressly, having a large bulb and a long slender stem, each degree (Cent.) extending over nearly three and a half lines. As to the relative value of the axillary or sub-lingual region for observation, we find the following :

"Repeated observations have proved to me, that if a thermometer be placed with its bulb under the tongue, at the same time that the measurement is being made in the axilla, the mercury will rise to exactly the same height in both cases, showing that the temperature of the blood circulating in the capillaries of the skin and in those of the mucous membrane of the mouth is the same at the same time. The mercury, however, attains its greatest height quicker in the mouth than in the axilla, which is accounted for by the greater delicacy and vascularity of the mucous membrane of the mouth, allowing the heat of the blood in its capillaries to pass through more readily than is the case in the skin, which, besides being less vas-cular, has a much thicker layer of non-vascular epidermis between the vessels and the external surface. With regard to the time which must elapse after the mer-cury has become stationary, in order to be certain that it will rise no higher, experience has proved to me that two minutes are sufficient in the mouth, and five minutes in the axilla. In no case did the mercury, after having been observed stationary for that time, rise more than one-tenth of a degree, even if the ther-mometer (as was sometimes done by way of experiment) was left in the axilla for half an hour. . . . . In different cases, the time required for the mercury to reach the true temperature is very different. . . . . I have never yet observed this time in the mouth to be longer than ten minutes, or in the axilla longer than thirty minutes. An observer, who, in such a case, would be satisfied with five minutes in the mouth, or ten minutes in the axilla, would therefore risk an error perhaps of a degree ; while an error of one-tenth of a degree is of great importance, when we consider that the temperature of the body is a direct index of the activity of the important chemical changes which are continually taking place in it. In making a thermometric observation, I therefore always carefully note down the progressive rising of the mercury, and do not assume it as stationary until it has been so for five minutes in the axilla, or two minutes in the case of measurement in the mouth." (pp. 84—86.)

The mouth, therefore, indicates the changes in temperature more quickly than the axilla, but it is more fatiguing to the patient. When the bulb is placed under the tongue, the lips should be pressed by an assistant closely to the stem, to prevent the patient breathing through the mouth. When the axilla is selected, the bulb should be well introduced into the cavity, and the arm brought across the chest.

The cases given in minute detail by Dr. Armitage are as follow:— Typhus 3; acute pleurisy 1; acute bronchitis complicated with pleurisy 1; typhus complicated with severe bronchitis, and violent delirium (in a drunkard) 1; and extensive organic disease of the heart, accompanied by intense congestion of the brain, 1. They are all (as intended to be by the writer) decided illustrations of the therapeutic value of cold affusion and cold baths, according to the method described. Dr. Armitage is evidently biassed in favour of the "hydropathic" method, and perhaps employed it more carefully in these cases than would be possible in the ordinary run of practice. There is amply sufficient, however, in their details, to interest the inquiring practitioner, and to confirm generally the soundness of the views of Currie and others, who have advocated the external and internal use of cold water in acute febrile diseases.

As we have thought it right to animadvert upon one or two little matters, which, at a first glance, identify Dr. Armitage with the hydropathic empirics, it is only just to the writer to add, that he considers their diagnosis often erroneous, and that generally their statements cannot be trusted.

Art. X.

Transactions of the New York Academy of Medicine. Vol. I. Part I. 1851. 8vo, pp. 165.

Although in point of bulk this fasciculus makes a poor show of academical labours carried over a space of four years, it contains papers of considerable interest on various points of practical medicine and surgery, contributed, for the most part, by writers whose names are well known in this country. Of the eleven communications, indeed, four have been made by Dr. Valentine Mott; and it does not speak much for the literary activity of his colleagues, to find that among from two to three hundred fellows of the Academy, only six besides himself have furnished contributions.

Passing over a paper by Dr. Pliny Earle upon the *History of the Institutions for the Insane in the United States,* as being chiefly of local interest, we come to an interesting report by Dr. Gardner, the chairman of a committee appointed by the Academy to examine into the *Comparative Value of Milk,* furnished by cows fed upon distillery slop and upon other more natural diet. New York, like all other great capitals, finds considerable difficulty in obtaining a supply of pure milk; and from the account with which this committee furnishes us, it would seem to be infinitely worse off in that respect than London, where, in spite of recent alarming reports, we believe the principal deterioration is produced by mere dilution. A portion of the milk employed at New York is obtained from the adjoining country;

but the great bulk of the supply is procured from cows that are kept in establishments attached to the great distilleries, being congregated together, in numbers amounting to some thousands, day and night, in close unventilated sheds, and fed upon the hot "slop" which flows directly from the distillery into their troughs.   In some of these places small portions of oil-cake, hay, or bran, are afterwards given, but in others nothing of the sort; and in none is a draught of pure water ever supplied.   The cow, usually refusing the food at first, especially on account of its heat, afterwards acquires a liking for it, and becomes excited as the time for its distribution arrives.   Prior to feeding, her naturally mild-looking eye becomes preternaturally brilliant, to be changed afterwards into a stupified, staring gaze. The animal, indeed, seems almost always in a state of stupor, and insensible to kicks or blows.   In some of the establishments the cows are kept very clean, and well curried, while in others the most horrid filth prevails.   Almost all suffer, to some degree, from a disease termed *sore-foot*, consisting of inflammatory action around the hoof; and *caries with absorption of the teeth* is another affection incident to the mode of life. The teeth that are specially exposed to the contact of the hot fluid, rot away, and become loose, and their alveoli are absorbed, while the grinders may continue sound.   When the slop gets cool, by transportation to some distance, the teeth suffer less. Another peculiarity is an *elongation of the hoof* (to eight or ten inches), owing to the absence of the wear and tear produced by the natural habits of the animal. Not unfrequently the cows die suddenly, especially when any epidemic prevails, and at the autopsy, little flesh and hardly any fat are to be found.   The omentum, which should, in a milch-cow, weigh from twenty to twenty-five pounds, weighs hardly one.   On one side of the thoracic cavity, effusions, amounting to several gallons, are found, more or less pneumonia being combined with the pleuritis which gives rise to these. Among other peculiarities observed by the committee, were, the unpleasant smell of the animal's breath (compared to that of an old beer-bottle), the absence of rumination, the panting respiration during summer, the small quantity of fæces, and the immense quantity of colourless urine.   This last circumstance would be expected, when it is stated that the daily allowance of slop is thirty-two gallons per cow, while ten quarts of milk is considered as a high daily yield.

There could be no hesitation in determining, *à priori*, that cows so kept could not yield healthy milk; but the committee also submitted the fluid to reiterated comparative examination.   Dr. Reid reports that the minutest examination could detect no vestige of spirit in the fluid; but that it yields a far less quantity of butter than grass-fed milk—this being only 15 or even 10 parts per 1000, instead of 35.   The butter, too, is whiter, and becomes associated with a larger proportion of curds and whey.   The effect of distillation is to abstract all the fecula and sugar from the grain, leaving the nitrogenized compounds and earthy matters almost undisturbed; and thus the quantity of caseine and ashes is above, while that of the sugar and butter is below, the standard.   An important point brought out by Dr. Reid is, that this milk, exposed to a temperature of 98°, required six hours for its coagulation, while dairy-milk coagulated in one hour.   Professor Alonzo Clark found, by the microscope, that the milk-globules, less

abundant and smaller than natural, possessed a strong disposition to firm aggregation.

"In two specimens there was an unusual number of epithelial cells, many of which were very markedly granular, and some highly coloured. In a few of these the milk-globules were still imprisoned, and of very small size, showing that these secreting structures had been discharged from the lactiferous ducts, of which they form the lining, before the complete elaboration of their contents."

The committee regard the statements of the slow coagulation of this milk, and the disposition of the globules to tenacious agglomeration, as very important, in explaining the disturbance it has been repeatedly known to produce in the systems of children fed upon it. By this delay of coagulation, its assimilation is prevented, and all the irritative consequences of repletion of the stomach result. They also believe the altered condition of the epithelial cells to be probably due to the injurious diet of the animal. Finally, they bring forward medical evidence to show the actual ill effects which have resulted from the use of this milk, and especially in the case of children.

The next paper is one of considerable practical interest, from the pen of Dr. Ashbel Smith, on the *Diagnosis of Yellow and Bilious Fevers*, founded upon his very familiar acquaintance with these affections, as met with on the Mexican Gulf Coast and in Texas. His object is to show that they are not different degrees of the same affection, but febrile diseases perfectly distinct from each other. He does not do this by drawing any elaborate portrait of the two maladies, but by roughly sketching their salient points of difference, deduced from observation of the general laws which govern their development, and a comparison of their symptoms and pathology. The chief points he dwells upon are as follow:

1. If yellow fever were merely an aggravated form of bilious or miasmatic fever, every case would exhibit great severity or malignity; but after the earlier period of a yellow-fever epidemic, a large proportion of the cases—often more than a half—are of a very mild character; these mild attacks, nevertheless, affording the same immunity from subsequent ones as do the more severe attacks.—2. Although in places where yellow fever prevails as an epidemic, sporadic cases do occasionally occur, yet, as a general rule, the disease is an epidemic. It appeared as an epidemic in Galveston, in 1839, 1844, and 1847; but Dr. Smith has never known a case occur there, except during the epidemic season. The miasmatic or bilious diseases are endemic, and no year is entirely exempted from their occurrence.—3. Bilious or miasmatic fevers pervade broad districts of country, while yellow fever is confined to the narrow limits of towns, which may be easily ascertained, and the disease avoided. In the level prairie country of Texas, intermittents and remittents are endemic, while yellow fever has never been known to originate anywhere in the interior out of the towns. Villages and towns are more exempt from miasmatic disease than the surrounding country; while, when yellow fever affects concentrated populations, the farmhouse and plantation are perfectly secure from its visitation. Yellow fever, indeed, usually commences in some particular quarter of a town, consisting of one or more streets, to which it is for a time confined, extending only more or less gradually over adjacent districts, and so

regularly, that its progress can be pretty accurately marked. No such rule prevails in regard to the miasmatic fevers.—4. Like other specific epidemics, yellow fever is much more malignant and fatal in the earlier cases; but no such character attaches to bilious fevers.—5. For the development of yellow fever, a high range of the thermometer is essential. In the epidemics observed by Dr. Smith, it stood at from 80° to 88° for several days prior to the outbreak. When once established, the disease may continue to rage when the temperature is a lower one than that necessary for its original appearance. Bilious fevers occur at temperatures too low for the origin of yellow fever, as well as during the extreme heats of summer and autumn.—6. Many persons believe yellow fever to be contagious and importable, while no suspicion of such property has ever attached to bilious fever.—7. Among the *distinctive symptoms* may be mentioned the fact, that bilious fever may be intermittent, remittent, or continued, although this last term is hardly applicable to a disease in which marked exacerbations occur daily, or every other day.

"Yellow fever is neither intermittent, remittent, nor continued; there are never two similar recurring paroxysms or exacerbations. It is essentially and uniformly a disease or fever of one single paroxysm. This paroxysm consists of states or stages, as follows:—1st, of *depression;* 2nd, of vascular *excitement,* which subsides of itself, if not rendered ataxic by injudicious medication, into a state of apyrexia, terminating at once in convalescence or in a third stage of *prostration* with new and peculiar symptoms; this last is the stage of hæmorrhage or black vomit. The state of vascular excitement is never renewed or repeated, any more than is the eruptive fever in the succeeding stages of small-pox. . . . . In many hundreds of cases of yellow fever which have fallen under my observation, I have never witnessed a second febrile paroxysm, in the same case, like the first. I have indeed seen yellow fever supervene on an intermittent, in persons affected with the latter coming into a district where the former prevailed; but in these cases, the yellow fever occupied at once the whole ground, if I may so speak, and marched on its course to its termination, regardless of the paroxysmal disposition of the disease which it had thrust out. In like manner I have seen the malignant miasmatic or congestive fever seize a person convalescent from yellow fever, who had previously been exposed to concentrated miasmata in the country districts; but the march of the two diseases was distinct, nor could they be confounded. Yellow fever, as has been stated, is sometimes rendered ataxic by injudicious treatment or neglect; but a second paroxysm similar to the first is not reproduced. To sum up: bilious fever consists of similar paroxysms or similar exacerbations; yellow fever never repeats itself, is not paroxysmal, has no character of periodicity." (p. 59.)

8. Relapses and second attacks are frequent in malarial fevers, and the liability of the system to such fevers is increased by prior attacks—convalescence frequently being only obtainable by removal from the malarial district. In yellow fever, *relapses*, properly so called, do not occur at all, *second attacks* are very rare, especially in the same locality, while the immunity may be regarded as complete if the person remain permanently and continuously in the same epidemic district. By imprudence, a patient imperfectly convalescent may fall into the state of prostration, a deceitful pause, of several days perhaps, preceding this. During all this pause, however, the hæmorrhagic state of the system has been present, and would, by the experienced practitioner, be distinguished from genuine convalescence. —9. Although one attack of yellow fever protects from a subsequent one, yellow and bilious fever afford no reciprocal immunity.—10. *Black vomit*

is the natural and regular termination of yellow fever that has run its course unmodified by treatment. It is a great error to confound with it the brownish saburral matter sometimes vomited in malarial fevers. In a large experience in malignant and congestive bilious fevers (to the extent of prescribing for 119 cases in one day), Dr. Smith has never met with a case of genuine black vomit. It is only one of the forms of hæmorrhage so common in yellow, and so rare in bilious fever. It is often revealed in autopsies where quite unsuspected, and in cases which had presented no immediately alarming symptoms. Hæmorrhage from the gums, also, is very common in yellow fever.—11. There is a peculiar *physiognomy* in yellow fever, which, though not easily described, is constant and unmistakable.—12. The *pulse*, in the last stages of fatal cases of yellow fever, is almost always preternaturally slow, and this often in despite of stimuli. In bilious fever, it is rare to find it below the healthy standard of frequency, except after the complete abatement of the disease.—13. In severe cases of yellow fever, the *urine* is usually of an intense saffron colour, and not unfrequently deposits blood, the bladder often remaining insensible to distension. In bilious fever, it is usually of a reddish hue, and scanty, or copious and limpid, while there is not insensibility to distension.—14. An occasional symptom met with in yellow fever, and not seen in any other febrile affection, is the occurrence of genuine *venereal desire*, in the stage of extreme prostration, and even of irremediable danger.—15. The attacks of yellow fever occur with greater comparative frequency at night, than do those of bilious fever.—16. Yellowness, commencing usually prior to death, and becoming more intense after it, is of such frequent occurrence as to have given the name to the disease. The yellowness occasionally met with in bilious fever is rarely observed to undergo marked increase after death.

17. The *pathological* circumstances adverted to by Dr. Smith, as distinguishing the two affections, are, the general hæmorrhagic disposition and black vomit already mentioned, an altered condition of the mucous membrane of the stomach, and the yellow or fawn colour of the liver. So frequently is the black matter met with in the stomach, notwithstanding it may have also been vomited, that Dr. Smith has only met with one case in which it was absent, while he has never seen it in the autopsy of a patient dying from paludal fever. In regard to the condition of the mucous membrane of the stomach he speaks as follows:

"I have always found the gastric mucous coat in a pathological condition in yellow fever. In the formation of black vomit, portions of the gastric mucous membrane are congested or gorged with blood, which is poured into the stomach, possibly by simple effusion, or, as I believe, by an imperfect or diseased vital process. The portions of the coat which furnished the black vomit are softened and generally thickened; but in the present state of pathological science and language, these effects cannot be distinguished from the effects of other diseases upon the same tissues. They will not, therefore, serve the purpose of practical diagnosis. I have, however, had opportunity, in several cases, to observe portions of the mucous coat in the state of sanguineous engorgement previous to the formation of black vomit—other portions of the tissue having furnished this fluid, and being in the condition above mentioned—and I do not hesitate to say I can distinguish the sanguineous engorgement in this disease, from the congestion in any other disease I have yet witnessed. Common gastritis can occupy one portion of the mucous coat, while another portion of the same furnishes black vomit." (p. 65.)

With the exception of two cases, in which hæmorrhage into the parenchyma rendered it intensely dark, Dr. Smith has found in all his dissections the fawn colour of the *liver* described by Louis. Indeed, prior to the appearance of Louis' work, he had published the accounts of autopsies in which this point was noted; but he attributes the merit to that great observer, of having directed special attention to this pathological appearance.

Dr. Valentine Mott contributes a short essay upon the *Value of the Seton as a Remedy in Ununited Fracture.* Having in preparation a critical examination of the various cases hitherto published, he furnishes us in the present paper with an account of the cases in which he has himself employed it; and this, as will be seen, with some encouraging results. Three of these cases (fracture of the humerus, tibia, and femur), in which it was employed with success, have been already published. The *fourth* case was an example of fracture of the tibia, occurring in a man æt. 35. After many months' trial of various means, the suture was resolved on. As it was found impossible to penetrate the ligamentous connexion of the fragments by means of a stilette, a common gimlet was used, and a skein of silk then passed through by means of an eye-probe. At the end of six weeks the seton was commenced to be removed thread by thread, and consolidation was found complete two months and a half after its passage. The *fifth* case was an ununited fracture of the middle of the os brachii, of eight months' standing, in a boy aged 12—the fragments being conical, and separated from each other by at least an inch. Two setons were passed at intervals, and respectively maintained for several months without any benefit resulting. The ends of the bone were now sawn off, and a silver wire passed through each, twisted, and brought out externally through a canula. The wire cut out from one of the bones in a few days, and the other was soon after removed. No great inflammation followed, and in a few weeks consolidation was complete. This case occurred in 1826, and is regarded by Dr. Mott as the first in which the ends of the bone have been brought into contact by the *wire suture*. Since then it has been repeatedly employed with success by the New York surgeons. It was thus employed several years prior to its adoption by Flaubert, of Rouen, to whom Malgaigne attributes its origin. The *sixth* case is precisely analogous to the last, an ununited fracture of the humerus with widely separated fragments, being excised, and joined by means of iron-wire, which came away in two or three weeks—union being complete in between two and three months.

The *seventh* case was an ununited fracture of the femur, at its upper third, in a man æt. 53. Owing to the lower fragment being so firmly lodged behind the upper, considerable difficulty was found in lodging the seton between the fragments, and a large spike-gimlet had to be employed to form a passage for the probe. Much inflammation, suppuration, and hectic fever, followed, exhausting the patient's powers so much, that, at times, Dr. Mott was on the point of withdrawing the seton altogether. He succeeded, however, in supporting the patient's strength, and complete consolidation was effected in three months. A year after the occurrence of the first accident, the patient refractured the thigh at the newly-united part; but at the end of six weeks the bone was found firmly reunited, the

ordinary amount of shortening only resulting. *Case eight:* A man of athletic form, æt. 44, presented an ununited oblique fracture of the femur at a little above its middle. The close approximation of the fragments required the gimlet to be used. Much inflammation and suppuration followed, but consolidation was complete eight weeks after the introduction of the seton. Dr. Mott regards the *ninth* case as unique, it being an example of *intra-uterine* non-union. The child, aged four months, was in excellent health, and well formed. It was brought on account of a supposed distortion of the foot; but upon examination this was found to be normal, while an ununited fracture of the tibia and fibula existed, about two inches above the ankle-joint. No pain was caused on motion, and the mother stated that the parts were just in the same state when the child was born. After in vain trying adjustment, movements of the ends of the bone, blistering, and galvano-puncture, a seton was passed with little pain, and gave rise to little inflammation or suppuration. In four weeks the union was firm, and some of the threads were removed. Now, however, without obvious cause, violent inflammation seized the leg, the seton had to be removed, and the bones regained all their mobility. The parent refused a re-application; but in a note to this communication, written in May, 1851, Dr. Mott states that three weeks since (the child then being eleven years of age), he had sawn off the ends of the bone, and connected them with a silver wire; and that thus far the case had done well.

Dr. Mott believes that the seton will usually be only found to succeed when the bones are in actual contact, or very nearly so; and that when they are not so, no operation promises better than that of resection of the fragments and their union by means of wire.

On Laceration of the Corpus Cavernosum Penis.—Dr. Mott relates two cases of this so-called "fracture" of the penis, which occurred under very similar circumstances—viz., the penis being struck while in a state of vigorous matinal erection. The great amount of tumefaction and distortion which ensued, gave rise to much alarm in the minds of the patients; but Dr. Mott found, that by quietude, and the application of discutient lotions or ice, absorption of the effused fluid was rapidly induced, the application of leeches to the part being avoided.

Cases of Tracheotomy. By Drs. Van Buren and Buck.—In the first of Dr. Van Buren's cases, tracheotomy was performed for the *removal of a foreign body* from the air-passages. The child, aged three years, was suddenly seized with paroxysmal cough, and occasional croupy respiration, after swallowing plums; but was treated for croup. When seen by Dr. Van Buren, fourteen days after the accident, he still suffered from severe paroxysmal cough, especially at night. During waking hours, a whistling sound was constantly heard in the throat; but not during sleep. The voice was unnatural; but deglutition was quite normal. Pressure on the thyroid cartilage caused pain and usually cough. The trachea was opened at the sixth or seventh ring on the fifteenth day, and air was expelled with great force by the cough. In less than two minutes, the shank of a plum-stem and the seed of a water-melon were forcibly expelled; and from this moment the cough ceased and the breathing became normal. In this

case *chloroform* was employed, the child being kept insensible until the dressings were completed—a period of fifty-five minutes. Its administration admirably facilitated the performance of what is usually, in a young child, a troublesome operation. In two other cases referred to, chloroform has been used in the United States with the same good effects; although its use is not admitted in this operation by European authorities. Tracheotomy was performed in Dr. Van Buren's second case, on account of impending suffocation in *syphilitic ulceration* of the *larynx*, coming on in the progress of syphilitic sore-throat. The relief was immediate and striking, although, in consequence of the progress of the disease, the patient eventually died. Six weeks after the operation, and when the patient's general condition seemed amending, the tube having been kept out for half an hour could not be reintroduced, in consequence of the great contraction of the aperture which then occurred. This had to be enlarged, suffocation in the mean time wellnigh taking place. The patient died, hectic, four months after the operation, all the diseased parts above the aperture exhibiting a disposition to reparation. The third case was one of *croup*, occurring in a child three years old. After the various other remedies had been resorted to in vain, tracheotomy was performed with apparent relief; but the child died in two hours after, while seemingly in a tranquil sleep.

Dr. Buck, however, relates a case of *croup*, in which tracheotomy was performed with success. S. B., a lad æt. 11, while gradually recovering from the effects of sloughing of the mouth, the consequence of the free administration of calomel in scarlatina, was seized with symptoms of croup, which were temporarily relieved. In a few days the disease re-appeared in an aggravated form, and as the boy's end seemed rapidly approaching, in spite of the varied treatment employed during forty-eight hours, a portion of the fourth ring of the trachea was excised, 8th July, 1849, with the effect of immediately relieving the embarrassed respiration. As the aryteno-epiglottidean folds were felt, by the finger passed into the fauces, to be swollen and pulpy, a nitrate-of-silver lotion (first ℈j, and then ʒj to an oz.) was commenced to be applied with a probang on the 11th. By great effort the patient was able, on the 14th, to breathe once or twice through the natural passage. The tube, on account of the accumulations of mucus, required changing twice a day for some time. By August 20, on removing the tube the while, and closing the aperture, a few words could be audibly uttered with considerable effort. In October, 1850, he was still obliged to wear the tube, which, however, did not prevent him freely engaging in robust sports. With the tube closed he could breathe eight or ten times uninterruptedly, though considerable effort was required. In using his voice, he closed the tube with the finger.

Case of Amputation at the Hip-joint. By Dr. Van Buren.—Three cases are on record, in which amputation of the hip-joint has been successfully performed, after prior amputation of the thigh (those of A. Cooper, H. Mayo, and Sands Cox), and Dr. Van Buren now adds a fourth. It occurred in the person of one Elijah Vanderhoof, forty-three years of age, and of good constitution. The amputation of the thigh was performed May, 1848, on account of a tumour weighing thirteen pounds, which proved to be " a magnificent specimen of true cartilaginous exostosis." At the end of six weeks

the patient was able to resume his occupation as a shoemaker. By July, 1849, an enlargement of the remaining portion of the femur became evident; and as this increased so much that it was by no means certain to what height it extended, amputation at the hip-joint was resolved upon. This was performed in March, 1850, twenty ligatures having been required, and not more than ten ounces of blood being lost. The patient was replaced in bed about forty-five minutes after having commenced the inhalation of chloroform. No shock ensued, the pulse being only seventy-five. Within six weeks the patient was in a condition to be presented to the Academy, and a residence of a few months in the country restored him so as to enable him again to support his family. Some difficulty was found in bringing him under the influence of chloroform, in consequence of the slowness of the respiration, and the irregular action of the heart which it seemed to induce. The fact of the patient of late having so freely resorted to the use of opium, to relieve his pain, may perhaps be adduced in explanation of this.

Dr. Van Buren observes, that the operation by lateral flaps was resorted to in the three other cases. After repeated trials on the subject, he adopted a modification of Liston's operation with antero-posterior flaps, in which the posterior flap is made by cutting from without inwards, towards the bone, the disarticulation of the femur being left to the last. The execution of this, with a little practice, requires less than half a minute.

Dr. Metcalfe has a very interesting paper on the *mode of employing chloroform*, based upon observations that he has made on near eight hundred cases, in which he has administered this agent. He observes, that the vast majority of the practitioners in the United States, as well as in Europe, prefer chloroform to ether, believing it to be possessed of the advantages claimed for it by Simpson, and, when properly employed, not more dangerous than that substance. In comparing the number of deaths caused by these agents respectively, we must bear in mind the infinitely greater number of cases in which chloroform has been administered. Although Dr. Metcalfe has found, like Soubeiran and Gregory, that headache, nausea, and bronchial irritation are caused by an impure quality of chloroform, he is not disposed to attribute all the deaths that have occurred to the use of such; nor does he agree with Dr. Snow in considering these explained by the neglect of the employment of an inhaling apparatus. In some of the cases due precautions have not been observed; while in some rare instances, pure chloroform itself seems to act as a poison on the system.

In considering the *precautions* that should be taken, Dr. Metcalfe regards as an essential one, that the superintendence of the inhalation, during surgical operations, should be confided to a person whose sole business it is to watch its effects upon the patient. Affections of the heart should contra-indicate its use; but pulmonary emphysema, or tubercle, except when very advanced, need not do so. Respiration in emphysema seems rather carried on with increased than diminished facility under its use. The *position* of the patient is of great importance. It should be unconstrained, and as nearly *horizontal* as possible. The tendency to syncope is thus obviated, and the respiratory movements are more freely

performed. It is to the fact of this position being then maintained, that Dr. Metcalfe attributes the exemption from all ill effects, that has hitherto attended the obstetrical employment of this substance. In several of the fatal cases, it has been administered in the sitting posture. Occasionally patients are met with who require instruction in the manner of inhaling, failing when left to themselves to take in a sufficient quantity into the lungs to produce the complete effect; and the unpleasant symptoms arise which attend an imperfect administration. In surgical operations, too, in order to avoid a shock from mental impression, the inhalation should be performed in another apartment, to which also the patient should be brought back after the completion of the operation, so that no unpleasant sight may meet his eye on recovering from the anæsthesia. In one case, alarming syncope was induced by the patient suddenly beholding his amputated limb. The stomach should be empty at the time of inhalation, as nausea is then much less likely to occur, and the operation less likely to be interrupted by vomiting. Dr. Metcalfe has, however, never known any serious symptom result from vomiting from chloroform.

In respect to the *mode of administration*, Dr. Metcalfe prefers administering it with a handkerchief, that is never allowed to come in contact with the face, so that a free admixture of air is secured at each inspiration. He considers the rapid induction of anæsthesia, as recommended by Simpson, for the purpose of avoiding the stage of muscular excitement, a dangerous practice in persons of great susceptibility. Bronchial irritation, cough, and congestion of the face, which follow the too rapid filling of the lungs with the unmixed vapour, all disappear, and the anæsthetic state is kindly induced when the above mode of administration is observed. During the inhalation, strict attention should be paid to the state of the respiratory function. When deep anæsthesia is induced, the voluntary muscles are completely paralyzed, so that respiration is exclusively carried on by the abdominal muscles, the diaphragm, and the elasticity of the lungs. When in this kind of respiration any considerable degree of dyspnœa exists, we should allow air alone to be inspired until that has become relieved. The occurrence of stertorous breathing is always a sign of deep narcotism, warning us to suspend the employment of the agent.

In treating cases in which the *anæsthesia has been carried too far*, Dr. Metcalfe regards the *prompt resort to artificial respiration* as the primary indication; and he criticises the treatment of some of the published cases, in which, in place of at once employing this, time has been lost in the attempt to force fluids down the throats of persons who had lost the power of swallowing. He relates an interesting case, which illustrates the advantage of the practice he recommends. Chloroform was administered to a man in the recumbent posture, æt. 50, of good and robust health, who was about to submit to the extirpation of an eye. He had been accustomed to drink freely, and, as often happens with such persons, he was brought under the influence of the chloroform with difficulty. There was a good deal of muscular action and incoherent talking; but in seventeen minutes there was slight stertorous breathing, complete muscular relaxation, and insensibility to hard pinching. The operation was completed in a minute, the patient groaning slightly, and making automatic muscular movements, not unfrequently seen when anæsthesia is not carried to its highest extent.

A smart gush of arterial blood followed the operation, but stopped in a few seconds, when it was also observed that the pulse and respiration were arrested, the face becoming rapidly blanched. Life to all appearance was extinct, when Dr. Metcalfe, recollecting a similar procedure on the part of Ricord, at once applied his lips to those of the patient, closing the nares with one hand and holding the mouth open with the other, and slowly inflated the lungs, imitating as far as possible a natural inspiration. An assistant, by compressing the thorax, and pressing up the diaphragm, performed artificial expiration, while another pressed the thyroid cartilage against the spine. After fifteen or twenty such respirations, which occupied about two minutes, the patient gave a feeble gasp, and a slight spurt of blood took place from the artery. Artificial respiration was continued about a minute longer, the hæmorrhage returned, the breathing and pulse recovered their natural condition, and in a few minutes the patient was able to speak concerning the operation. His convalescence was as rapid as is usual.

In one of the papers, Dr. Mott relates three cases, illustrating the value of this agent in extending the sphere of surgical interference, enabling many necessary operations to be undertaken which the fears of the patient or the hesitation of the surgeon would have caused to be declined. His cases were examples of very large glandular tumours situated in the necks of children, the removal of which entailed an amount of tedious dissection, and a risk of the production of shock to the nervous system, which he would not have ventured upon unaided by chloroform. As these cases occurred in 1848, we need not further advert to this point, which the experience of most surgeons has since then enabled them to appreciate. We may, however, state, that while two of the children reovered, the third died about seven hours after the operation; but Dr. Mott attributes this occurrence, the possibility of which had indeed been contemplated, to the great amount of hæmorrhage which took place from enormously enlarged veins. The child had completely recovered from the effects of the chloroform.

Case of Aneurism and Ligature of the Left Subclavian Artery. By Dr. Valentine Mott.—James Smith, a labourer, aged 35, received a violent blow from the edge of a cask, which struck him just below the left clavicle. In a few weeks a painful pulsating tumour became formed under the clavicle; and as compression outside the scalenus anticus arrested the pulsation, it was determined to tie the subclavian at this point, although the exhausted condition of the patient from suffering, and the highly œdematous and erysipelatous state of his arm, did not augur well for the success of the operation. The tumour was mostly below the clavicle in its entire length, extended considerably into the axilla, penetrated to near the outer edge of the scaleni, and raised the clavicle and shoulder. On the 15th of November, 1850, the patient being seated in a chair, the subclavian was exposed just where it passes from under the anterior scalenus. The vessel was distinctly felt and seen; but it was long found impossible to bring the hook of the needle around it, though for what reason Dr. Mott cannot understand. The artery seemed as if firmly bound down to the parts beneath. Reversing the position of the instrument, the hook was at last, after great difficulty

and perseverance, passed from above downwards under the vessel. In turning the point upwards so as to get at the eye, an alarming torrent of blood rushed from the sac. The hæmorrhage was controlled by passing the points of two fingers of the left hand into the wound; and in the midst of the blood, the needle was passed, and the hook and ligature brought out from below upwards. All present declared themselves satisfied that the ligature was under the artery; and although not quite sure upon this point himself, Dr. Mott tied it, and the pulsation in the aneurism instantly ceased. While, however, the wound was being dressed, the pulsation was reproduced. In the uncertainty of what had really been tied, attempts were again made, after exposing the artery under the outer edge of the scalenus anticus, to bring the hook under the artery, and after many trials, this was accomplished. The pulsation again ceased, and the tumour diminished to one-half its size. Very little local disturbance or constitutional irritation followed; and in a few days the tumour had nearly disappeared, the clavicle returning to its natural position. The first ligature came away with slight traction on the seventeenth day, having a larger noose than Dr. Mott had ever seen after ligature of any other artery. The second ligature was not detached until the thirtieth day, and presented almost as large a noose as the first.

"I am sure that the first ligature was not around a nerve, as the pain at the instant of its application would have been insupportable, and the numbness of some part of the arm or forearm and fingers would have been an inseparable accompaniment. While the ligature was beneath it, as is my custom, I pressed opposite it with the forefinger, and so did my assistants, and the pulsation of the aneurism ceased. And on tying it, the tumour ceased to beat. Now what did I tie in this ligature? I frankly own I DO NOT KNOW. I cannot feel willing to admit that I was mistaken in the artery, when it was plainly felt, and even seen, and when, too, the relative anatomy was all exposed before me, and which I feel I ought to be familiar with. But so it is—the facts I have stated—others may judge. If two subclavian arteries had ever been seen on one side, that would be my refuge. To me this case is peculiarly interesting and instructive. First, on account of the possibility of two subclavian arteries on the same side. Secondly, the laceration of the aneurismal sac by the hook. Thirdly, my being able to command the frightful torrent of arterial blood from the opening in the sac by two fingers of my left hand. Then making a further dissection, and tying the artery under the edge of the scalenus. And lastly, and best, and most certain of all, for the patient to recover." (p. 165.)

Dr. Mott has now operated in seven cases, six times successfully. In the fatal case, the ligature was placed within the thyroid axis of the right subclavian; in the others, without the scaleni.

Art. XI.

Physiological Researches. By Sir Benjamin C. Brodie, Bart., D.C.L., F.R.S., Corresponding Member of the Academy of Sciences of the Institute of France. Collected and Republished from the 'Philosophical Transactions.'—*London*, 1851. 8vo, pp. 146.

The name of John Hunter is held in reverence alike by every Physiologist and by every Surgeon; but it is not only in his separate excellence in each of these departments of labour, that his greatness must be

recognised.   He is the only man who ever, in anything like the same measure and degree, combined the theoretical and the practical, the science and the art, in all that relates to vital action in its normal and abnormal states.   Other great physiologists have thrown out hints for practical men to seize and apply; other great surgeons have employed a few occasional hours in prosecuting some favourite line of physiological inquiry: but the general fact has been, that each department has been separately pursued by those who make it their special vocation, and that practical men not unfrequently manifest an undisguised contempt for the mere theorists who maintain that a knowledge of the healthy action of the system is essential to a due comprehension of its abnormities, whilst physiologists are not slow to retort upon those, in whose practice they trace, not merely a profound ignorance of Nature's laws, but also, it may be, a systematic antagonism to her curative efforts.

Hence we heartily welcome any manifestation, on the part of the leaders in our profession, of a disposition to maintain the dignity and value of its scientific basis; which, in the earlier part of their career, they may have themselves contributed to extend and deepen; but from labouring at which they have been subsequently drawn-off, by the attractions of the more lucrative, though we cannot admit it to be the more dignified work, of building up the superstructure of surgical practice.   Among all the surgeons of our time, however, there is none in whose writings the advantage of having early laid such a foundation is more plainly discernible, than in those of Sir B. Brodie; and this not merely, perhaps not so much, in the actual amount of physiological information displayed, as in that philosophical habit of thought which leads to a right appreciation of pathological phenomena, and in that tendency to base the rules of practice upon general principles, which marks the distinction between the scientific and the empirical practitioner.   Entertaining, as we do, the highest opinion of Sir B. Brodie's qualifications for the study of physiology, of which his early contributions to that science are an evidence, it has always been a matter of regret to us that he should have found " the duties of an arduous profession" quite incompatible with " the pursuit of those physiological inquiries to which he was able to devote a considerable portion of his time during the early part of his professional life" (Preface); and we could wish that, like his great predecessor, he had continued to justify his early promise of greatness, in both these fields of honourable though not equally remunerated labour, and had given to the present generation an illustration of the possibility of the attainment of high distinction in both.

The two subjects towards which Sir B. Brodie's attention was especially directed, during what may be called the physiological period of his career, seem to have been the participation of the Nervous system in the production of Animal Heat, and the *modus operandi* of Poisons on the animal system.   We desire to direct the special attention of our readers to his contributions to the first of these inquiries; because we conceive that they have been too much overlooked by those who have considered the production of animal heat in a simply chemical point of view, and that they brought to light facts which cannot be fairly ignored in any discussion of the subject, and which, to say the least, require such an explanation from the upholders of the chemical theory as they have never deigned to bestow.

Although the parentage of the chemical theory of animal heat is not unfrequently assigned to Liebig, and although he has doubtless laboured to give it consistency and definiteness, as well as a surer foundation in facts, yet he would not himself, we are confident, claim to have originated it; since a mere tyro in the history either of Chemistry or of Physiology could immediately adduce full proof to the contrary.—The simple facts of the case we conceive to be these. The ancient opinion respecting the use of the lungs, and the introduction of air in respiration, was, that it served to *cool* the blood, which had been heated, according to the mechanicians, by the friction of its particles against the walls of the vessels, or, according to the chemists, by a fermentation taking place in the heart. The first writer who seems to have seriously questioned this doctrine, was Mayow; who, in his Tracts on various philosophical subjects (1674), explicitly advanced the two positions, that the effect of respiration is not to cool the blood, but to generate heat, and that it does this by an operation in every respect analogous to combustion. In the existing state of chemical knowledge, his hypothesis was, of course, very imperfect; but still his method of experimenting was the one, which, more fully carried out, has since been thought sufficient to establish it. He burned a candle under a bell-glass, and found the residual air so deteriorated as to be incapable of supporting combustion; he then caused a mouse to breathe a similar quantity of air, and the necessity of its renewal was soon apparent. After this, he put a mouse and a candle together under the same glass vessel, and found that the mouse lived only half as long as it had existed when alone under the glass; on the other hand, he endeavoured to fire combustible matter in air which had been spoiled by breathing, and found that no combustion took place. His conclusion, "that the nitro-aërial particles are absorbed both by the candle and the animal," is essentially correct in idea; for we have only to substitute a phrase expressive of the conversion of atmospheric oxygen into carbonic acid, as the change which is common to the two substances, to make the doctrine complete. That substitution *was* effected as soon as ever chemists had found out the true nature of oxygen and of carbonic acid; for when Black had shown that the expired air differs from the inspired by the addition of carbonic acid (or fixed air, as it was then termed), and Priestley had demonstrated that the expired air further differs from the inspired by the loss of a portion of its oxygen, the ground was prepared for Lavoisier to develope the conclusion, that the essential change which the air undergoes in respiration is the conversion of a portion of its oxygen into carbonic acid, by a process resembling combustion. That the evolution of animal heat was dependent upon this production of carbonic acid, just as heat is generated by the combustion of carbon and the production of "fixed air" elsewhere, seems to have been very distinctly laid down by Black; but the doctrine was subsequently advanced, no doubt independently, by Lavoisier, and derived a more secure support from his researches upon the nature of the combustive process generally, in which he had the advantage (not possessed by Black) of the discovery of oxygen that had been made in the interval. Thus the "combustive theory" of animal heat, which had been so clearly advanced by Mayow, was placed upon a firm chemical foundation; and although it is very probable that Lavoisier may have arrived at it from his own ad-

mirable researches on combustion, yet it must be borne in mind that the doc-
trine had not only been propounded in England, upon the basis of actual
experiment, a century before, but had been adopted and confirmed by an
eminent British chemist, who had furnished one of the most important
data whereon Lavoisier founded his own conclusion, the other of these
data having been supplied by another British chemist, whose hard-earned
fame has been but grudgingly admitted by the partizans of the distin-
guished Frenchman.   Thus, as it seems to us, it is no more correct to
assign the parentage of the Chemical theory of Animal Heat to Lavoisier,
than to confer it upon Liebig; although it may be freely conceded that
his researches upon combustion gave it that completeness and definiteness
n which it had been previously deficient.

But the doctrine of Black and Lavoisier, being founded on the assump-
tion that the combustive process takes place in the lungs, the oxygen of
the inspired air being there made to burn-off the carbon brought by the
blood, was open to the obvious objection that, if true, the lungs should be
hotter than the other parts of the body; and there seems reason to believe
that the force of this objection was so strongly felt by Dr. Black, that he
was led by it to relinquish his hypothesis.   The subject was taken up,
however, by Dr. Crawford, who applied to it Dr. Black's own theory of
" latent heat;" and by this theory, he raised upon his own experimental
investigations a superstructure of doctrine, which has been characterized by
a most competent judge as " one of the most interesting and beautiful
specimens of the application of physical and chemical reasoning to the
animal economy, that had ever been presented to the world."   The essen-
tial point of this doctrine was the (supposed) greater capacity of arterial
than of venous blood for caloric; whereby the heat generated in the lungs
by the combustive process is not set free, but becomes " latent" in the
arterialized blood, only becoming " sensible" when that blood is rendered
venous by its transit through the systemic capillaries; whereby its warmth
was uniformly distributed through the body.   It is worthy of remark, as
another example of the *appropriativeness* of Lavoisier, that in his ' Memoir
on the Source of Animal Heat,' published subsequently to Crawford's trea-
tise, he coolly adopts this doctrine as his own; notwithstanding that he
gives abundant evidence of possessing a full acquaintance with Crawford's
researches.   This doctrine, as we presume that all our readers are aware,
has been set aside by subsequent researches.   For the idea, originally
advanced by La Grange and Hassenfratz, that the blood brings to the
lungs, not free carbon, but carbonic acid, and that it goes from the lungs to
the tissues charged with free oxygen, has now been fully substantiated by
the experimental researches of Dr. W. F. Edwards, Professor Magnus, and
others; so that we now know that the combustive process takes place in the
systemic capillaries, at least as much as in the pulmonary.   And, moreover,
it appears from the inquiries of Dr. J. Davy, that if there be any difference
between the capacities of venous and of arterial blood for caloric, such
difference is in the contrary direction to that asserted by Dr. Crawford.

All the earlier theorizers upon this subject regarded the *carbonic acid*
of the expired air as the sole product of the combustive process; and
looked upon the watery vapour exhaled from the lungs as a mere product
of evaporation from the moist lining membrane of the air-passages and air-

cells.   This conclusion seemed to be borne out by the correspondence which various experimenters asserted to exist, between the amount of oxygen that disappears from the inspired air, and that which exists in the carbonic acid set free in expiration.   And it was maintained by Lavoisier and Laplace, as the result of comparative experiments, that the quantity of heat generated in a given period by the combustion of carbon in the living body, as measured by that which is evolved in the production of an equal amount of carbonic acid by the ordinary combustion of carbon, is equal to the maintenance of the animal temperature during the same period.   The more exact experiments of Dulong, however, showed that this was far from being the case; the quantity of heat generated in a given time by a warm-blooded animal, being considerably more than the combustion of the carbon contained in the products of exhalation would serve to account for.   But Dulong noticed, that besides the oxygen which is expended in the formation of the carbonic acid, a considerable quantity, sometimes amounting to one-third of the whole, was expended in some other mode; and he surmised that this must be consumed in other combustive processes, one product of which *might be* a portion of the water exhaled from the lungs. That such is really the case, has been assumed by most of the advocates of the chemical theory of animal heat, Professor Liebig amongst the rest; but we are not aware that any very exact data for the assumption have been supplied.   One of the strongest arguments in its favour has always appeared to us to be afforded by the calorific power, which ample experience has shown that those articles of aliment possess, which, in addition to their free carbon, contain a large quantity of hydrogen uncombined with its equivalent of oxygen.   Thus all voyagers and travellers in the Arctic regions rate the value of *oleaginous* substances far above that of *farinaceous;* the latter containing oxygen and hydrogen in equivalent proportions, and having a surplus of carbon alone; whilst in the former the quantity of oxygen bears but a small proportion to that of the hydrogen, which is thus ready to be burned-off in the body, and *must* be oxidized and got rid of under some form, since the quantity of fatty matter in the excretions bears no relation to that which is ingested.   Moreover, we know that, in cases of starvation, the fat of the body is gradually consumed in the respiratory process; and in this consumption, it may be inferred, water as well as carbonic acid must be given off.   All that Professor Liebig has written on the subject of animal heat, has been founded upon this basis. We are not aware that he has made any original researches on the subject; and his addition to our previous knowledge, or rather, his extension of our previous belief, seems to us to be limited to the two following points:— (1.) He has taken Dulong's own experimental results, and has calculated the amount of heat generated by the animals, on the one hand by the amount of carbonic acid exhaled, on the other by the surplus of oxygen which disappeared, this being considered as expended in the combustion of its equivalent of hydrogen; and adopting calorific equivalents for hydrogen and carbon, which he considers to be more correct than those of Lavoisier (which had been employed by Dulong as his basis), he shows that the quantity of heat thus generated comes sufficiently near to the quantity actually given off during the same time, to make it appear that the combustive process accounts for the whole of it.—(2). He has established the

distinction, which has now been generally admitted by physiologists, between those alimentary materials which constitute the pabulum of the tissues, and those which serve as the special pabulum for the respiratory process; this distinction, however, was based upon those researches of Mulder upon the protein-compounds, which first led to the conclusion, that as the azotized compounds generated by plants are identical with the materials of animal flesh, they can be applied to the purposes of nutrition without any transformation, whilst the digestive powers of animals are totally inoperative to effect any such transformation in the non-azotized compounds, which must consequently be applied to some other purpose in the economy.—In thus limiting Professor Liebig's contributions to the chemical doctrine of animal heat, to these two points of doctrine, we would not for a moment be supposed to under-estimate their value.   The first, as we shall presently show, is of fundamental importance, in whatever modification of the chemical theory we may finally settle ourselves down; whilst the second affords the only scientific foundation for a correct and economical system of dietetics.

The general evidence in favour of the Chemical theory of animal heat has received many important additions of late years.   The fact that a considerable amount of heat is generated by Plants during the processes of germination and flowering, when, like animals, they withdraw a large amount of atmospheric oxygen, and replace it by carbonic acid set free from their own tissues, has long been known; and very careful observations upon the *Colocasia odora,* a plant of the Arum family, which is remarkable for the elevation of temperature which its spadix undergoes at the height of the flowering-period, have demonstrated that the production of heat is constantly accompanied by the generation of carbonic acid, that it does not take place if the flowers be made to expand in an atmosphere of nitrogen, and that during both the attainment of the highest point and the subsequent decline, there is a close correspondence between the amount of oxygen consumed in successive hours, and the excess of heat shown by the spadix above that of the surrounding medium.   So, again, the researches of Mr. Newport upon insect temperature have shown that the same constant coincidence exists between that generation of heat which they occasionally exhibit, and the replacement of atmospheric oxygen by carbonic acid.   Moreover, since we may regard it as all but demonstrated, that the animal can under no circumstances convert the non-azotized saccharine or oleaginous compounds so abundantly supplied by plants into the albuminous constituents of its own tissues, the large amount of these substances consumed by animals, and the provision obviously made for their digestion and for their reception into the circulation, become utterly meaningless, if they are not introduced for the express purpose of being applied to the maintenance of the combustive process, and the consequent generation of heat.   Further, the direct effect of withholding food altogether, as the experiments of Chossât have shown, is to produce a complete absorption of the fat, which, with the products of the waste of the azotized tissues, is burned-off day by day for the maintenance of the animal temperature; the duration of life in this condition is strictly proportional to the amount of combustive material previously stored up in the form of adipose tissue; and when this is all exhausted, the heat of the body, which had previously suffered but little

decline, diminishes from hour to hour, until the animal dies of cold—the application of external warmth being still able to arouse it to activity, even when it has sunk into a state of torpor approaching to coma. All experiments upon the respiration, moreover, concur in showing that the liberation of carbonic acid is much more rapid at low temperatures than at high; showing that there exists within the system some means of self-adjustment, whereby the amount of fuel consumed is proportioned to the quantity of heat which it requires to produce, for the maintenance of its temperature at the regular standard.

Further, the recent researches of M. Bernard upon the functions of the liver, leave little room for doubt that one of the most important purposes of this organ is to prepare the respiratory pabulum; not so much, however, in the mode originally supposed by Liebig, by separating it from the blood as bile, this to be reabsorbed and subsequently burned-off in the lungs; but rather by transforming, without separation, certain materials previously contained in the blood, so that as the current passes through this gland, both the substances newly-absorbed from the alimentary canal, and those which have been taken up in the course of the general circulation, are subjected to its influence, and then pass on, through the right cavities of the heart, to the pulmonary vessels, in a state of peculiar fitness to undergo a further transformation by the influence of the oxygenated atmosphere which they there encounter. The combustive materials directly supplied by the food are chiefly, in herbivorous animals, those of the farinaceous and saccharine, and those of the oleaginous types. The farinaceous matters are converted by the digestive process into the saccharine form; so that grape-sugar is found in the mesenteric veins after the ingestion of such aliment, as is also cane-sugar if this have been taken in. But the blood is not very tolerant of the presence of grape-sugar, still less of that of cane-sugar; so that if these substances be injected into the *jugular vein* in any quantity, they are speedily detectible in the urine. During their passage through the liver, however, they are converted into a new form of sugar, which has been termed "liver-sugar," the characteristic properties of which seem to consist in its differing from grape-sugar, as grape-sugar differs from cane-sugar, that is, in being far more tolerated by the blood, and in being more readily converted into lactic acid. Hence when grape-sugar and cane-sugar are injected into the *vena portæ*, they do *not* show themselves in the urine; and the liver-sugar which is produced by their transformation is not traceable further than the pulmonary vessels, being speedily transformed, and in great part eliminated (probably passing through the intermediate stage of lactic acid) by the respiratory process. But it is one of the most curious results of M. Bernard's researches, that the liver not only thus transforms other saccharine compounds, but that it generates sugar *de novo*, under circumstances which seem to forbid our regarding it as anything else than a product of the retrograde metamorphosis of the albuminous compounds. For M. Bernard has found sugar in the liver and hepatic blood of carnivorous animals, and even in the liver of the mammalian fœtus and of the embryo chick; and so constant does the presence of this substance appear to be, that the production of a peculiar kind of sugar, specially fitted for undergoing the combustive process, must henceforth be regarded as one of the most important functions of the liver. It has been supposed that the

sugar generated by the liver may be derived from fatty matters brought to it by the vena portæ; but we cannot find in Bernard's experiments any evidence of this; and whilst we have no chemical reason for believing that sugar can be formed by the transformation of fat (which would be an inversion of the usual order of metamorphosis), we know that it may be generated as one of the products of the disintegration of muscular tissue, the juice of flesh having yielded *inosite,* a new form of sugar, to the analyses of Scherer.

It has been further shown by M. Bernard, that the liver produces a peculiar fat, from the constituents of the blood which passes through it ; and it seems that this liver-fat, like the liver-sugar, may be the product of the transforming process, exercised alike upon materials newly absorbed from the alimentary canal, and upon the products of the disintegration of the tissues.   That albuminous compounds in a state of retrograde meta-morphosis may generate fatty matter, can scarcely, we think, be any longer questioned ; since the phenomena of fatty degeneration concur with chemical evidence, to prove that this is one of the forms into which their elements pass, in preparation for their egress from the body.

Thus, then, it appears that the *respiratory pabulum* of carnivorous animals, which is derived from the decomposition of the albuminous consti-tuents of their food and of their own tissues, precisely corresponds with that which is furnished to the herbivorous races in the non-azotized articles of their diet; that the liver may be regarded as an organ specially des-tined to alter the characters of the blood, not merely by what it separates *from* it, but also by the changes it makes *in* it; and that these changes have reference to the constant sustentation of the combustive process, by the preparation of certain organic compounds peculiar to the living body, of a nature that may enable them readily to undergo oxidation when exposed to the air.   And it is a most interesting and suggestive fact, that the pro-ducts of the disintegration of the system should thus be applied—by passing through the very same course—to the same final uses, as are served in a large proportion of the animal kingdom by organic compounds that cannot be rendered subservient to the production of animal tissue.   We can scarcely fail to see in the whole of this arrangement, not merely a means of freeing the system from effete matters, but a scheme for the regular supply of the combustive apparatus with appropriate fuel ; and those, if such there be, who deny that the production of animal heat is essentially dependent upon the conversion of hydrocarbonaceous compounds into water and carbonic acid, have to explain how these compounds can be eliminated, as we know they are, by means of this oxidating process, *without* the production of heat; and they have also to explain why the herbivorous animal should be instinctively impelled to ingest a large amount of food, that can serve no histogenetic purpose in the economy, and why their digestive apparatus should be specially adapted to the reduction, solution, and conversion of non-azotized compounds, as well as to the introduction of azotized matter into the system.

The recent researches of M. Barral ' On the Statics of the Human Body,'* have afforded much additional information upon the subject of our present inquiry.   He has carefully determined the ultimate constituents

* Ann. de Chimie et de Phys., tom. xxv.

of the food, and of the various excretions, of four individuals, during five days, experimenting thus upon himself also during summer as well as in the winter; and presents us the results in an elaborate series of tables.   We shall cite only a few of the most important.   In the first place, the *carbon* of his food amounted to 5654·1 grains in winter, and 4090 grains in summer; and of this the proportion ejected in the form of *fæces* was 236·2 grains in winter, and 137·4 in summer; that eliminated by the *urine* was 234·6 grains in winter, and 211·5 grains in summer; whilst that which passed off by exhalation (pulmonary and cutaneous, the latter, however, being an insignificant fraction) was no less than 5183·3 grains in winter, and 3741·1 in summer.   Thus we see how large a proportion of the carbon of the food is thrown upon the respiratory process for elimination, and how considerable a diminution takes place in the amount exhaled during the heat of summer, involving a corresponding difference in the demand for aliment.   M. Barral's researches appear to afford the means of determining, approximatively at least, how much of the *hydrogen* of the food is oxidized in the respiratory process, and how much of the water exhaled is really generated combustively.   He found that of the whole amount of hydrogen taken-in, not more than from $\frac{1}{8}$th to $\frac{1}{10}$th passes off by the other excretions, the remaining $\frac{7}{8}$ths or $\frac{9}{10}$ths being exhaled in the condition of watery vapour by the lungs.   Now of the *oxygen* ingested, he found that 3841·4 grains in winter, and 2757·6 grains in the summer, passed off by the lungs in the condition of water; and this would, of course, carry off its equivalent of hydrogen, also derived from the food, without any production of heat.[*]   But the amount of hydrogen actually exhaled was 801·3 grains in winter, and 597·5 grains in summer; so that, after deducting one-eighth of the weight of the oxygen, there remain 321·1 grains in winter, and 252·8 grains in summer, as the amount of hydrogen which could not have been oxidized and eliminated as water, save at the expense of the oxygen of the air, and which must therefore have undergone a combustive process whereby heat could not but have been generated.

Thus it appears certain, that we are not to look only at the combustion of carbon, but to that of hydrogen, as a source of animal heat; but it is scarcely fair to reckon (as Professor Liebig has done) the whole surplus of the oxygen absorbed, above that contained in the carbonic acid exhaled, as being consumed in this mode.   For there are other oxidizing processes going on within the system, some of them of no mean importance; more especially the production of the sulphuric and phosphoric[†] acids from the

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[*] This will be obvious enough if we take the simple case of sugar, which is carbon + the components of water, already united in equivalent proportions; the carbon being eliminated by combustion, the water remains, to be carried off as such.   A fatty body, on the other hand, is carbon + hydrogen + the components of water; here a portion of the hydrogen, as well as the whole of the carbon, is destitute of its equivalent of oxygen; and water is actually generated in the combustive process, in addition to that whose components were already united in the substance, although under another form.

[†] Professor Liebig has lately asserted, with his usual dogmatism, that no phosphorus exists in the body, or in the alimentary materials, except in the state of phosphoric acid, and further, that the notion that such other compounds of phosphorus exist in the body, and that phosphorus occurs in the protein-compounds in a state analogous to that in which sulphur exists in them, " proceeds generally from superficial observations, without the slightest scientific foundation."   (See his ' Familiar Letters,' 3rd edition, pp. 451, 2.)   We apprehend that several chemists, of no inferior authority as analysts, maintain a different opinion; and that there is

sulphur and phosphorus contained in the protein-compounds of the food, giving rise to the production of the alkaline sulphates and phosphates which are excreted by the urine. In fact, it may probably be stated, that there is scarcely a single chemico-vital transformation in the body, either *progressive* (that is to say, tending to the formation of living tissue) or *retrograde* (that is, tending to the reduction of the more complex histogenetic substances to the comparatively simple compounds which present themselves in the excretions), in which oxygen is not concerned. Thus, then, although the production of carbonic acid and water may be immediately effected by the combustive metamorphosis of saccharine and fatty matters, and although these matters, when not directly supplied as such by the food, are prepared within the body for the respiratory operation, yet the process of such preparation is one that really involves a great number of separate stages of metamorphosis, beginning with the conversion of alimentary materials into living tissue, and including all the changes which take place during the life of that tissue, and in the progress of its final disintegration. Hence although the production of such part of the carbonic acid and water, as proceeds directly from the union of the carbon and hydrogen of the alimentary materials with the atmospheric oxygen, *may* be looked on as a simple combustive operation, yet as to the whole of the remainder, which proceeds from the disintegration of the tissues themselves, it must be admitted that the appearance of a given quantity of carbonic acid in the expired air, can only be looked on as the exponent or manifestation of a long series of changes of composition taking place in the interior of the body, of which it is one of the ultimate products. Some of the advocates of the Chemical theory of Animal Heat, have spoken of the body as if it were a mere furnace, into which a certain quantity of fuel is put, whose combustion affords a given equivalent of heat; whilst from its chimney there proceed so much carbonic acid, so much water, and so much imperfectly-consumed smoke. The whole series of intermediate operations, whose original materials and final products alone are thus recognised, is completely ignored; and the confident assertions of the chemists, based, as they appeared to be, upon a very broad foundation in fact, have passed muster with the general, and even with the medical public, notwithstanding that there is a considerable body of facts, which are not only not explained by chemical doctrine, but which seem, at first view, to be really in antagonism with it. These facts cannot be passed over, either by the scientific physiologist or by the observant practitioner; for they force themselves upon the attention of the latter; whilst they must be recognised by the former as having no less a title to his consideration than have the ordinary phenomena of calorification;—we refer to the local development of heat, or to local deficiency, altogether irrespective of the activity of the circulation; and to the con-

abundant physiological evidence, derived from the occasional luminosity of the urine, breath, and sweat, as well as of the solids of the body when undergoing decomposition after death, or even before life is extinct, which proves the existence of phosphorus either in a basic or in an imperfectly oxidized condition, as one of the normal constituents of the body; this phosphorus being usually completely oxidized within the system, and carried off as phosphoric acid in combination with alkaline bases, but passing into the excretions in its lower state, when, either from an absolute deficiency of respiration, or from the greater demand for oxygen set up by other matters contained in the blood (alcohol, for example), it does not undergo complete metamorphosis.

nexion of such irregularities with peculiar conditions of the nervous system.   And it is because we desire to draw the attention of our readers to this subject, and think we can help them to an elucidation of it, that we take a more special notice of Sir B. Brodie's republication, than it would have otherwise demanded at our hands.

Sir B. Brodie's first series of experiments, whose results were embodied in the Croonian Lecture for 1810, and published in the 'Philosophical Transactions' for 1811, seems to have been performed with the original purpose of ascertaining how far the action of the heart would continue, after the removal of the influence of the brain by division of the spinal cord high up in the neck.   Having found that, under these circumstances, the heart continued to contract with as much strength and frequency as in the living animal, provided artificial respiration was maintained, it occurred to him to ascertain whether the animal heat was maintained under these circumstances at its natural standard.   In the first observation, upon a middle-sized dog, he found that the thermometer introduced into the rectum fell in the course of an hour from 100° to 94°; an hour afterwards, a thermometer introduced into the thorax in contact with the heart fell to 86°; and half an hour afterwards, the thermometer in the same situation was no more than 78°; the temperature of the room during the experiments being 63°.   In the second observation, which was made upon a rabbit, the temperature of the abdomen fell, within an hour, from 100° to 89°, and in forty minutes more it had fallen in the same situation to 85°; and when the bulb was placed in the thorax, in contact with the lungs, the mercury fell to 82°.   Upon this he remarks:

"It has been the generally received opinion that the heat of warm-blooded animals is dependent on the chemical changes produced on the blood by the air in respiration.   In the two last experiments the animals cooled very rapidly, notwithstanding that the blood appeared to undergo the usual changes in the lungs, and I was therefore induced to doubt whether the above-mentioned opinion respecting the source of animal heat is altogether correct.   No positive conclusions, however, could be deduced from these experiments.   If animal heat depends on the changes produced on the blood by the air in respiration, its being kept up to the natural standard must depend on there being a certain quantity of air inspired, and a certain quantity of blood propelled through the lungs in a given space of time: in other words, it must bear a relation to the fulness and frequency of the pulse, and the completeness of the respiration.   It, therefore, became necessary to pay particular attention to these circumstances." (pp. 6, 7.)

In his two next experiments, accordingly, the rate of the heart's action was noted, and particular care was taken to make the artificial respiration resemble the natural, so far as possible, both in fulness and frequency.   In the first of these cases, the heat of the rectum fell from 99° to $92\frac{1}{2}$° within forty minutes, without any lowering in the rate of the pulse; and in the second, the fall during an hour was from $101\frac{1}{2}$° to 92°, the pulse still continuing at its original rate of 140°.   During this experiment, moreover, "the blood in the femoral artery was seen to be of a bright florid colour, and that in the femoral vein of a dark colour, as usual."   In subsequent experiments, the rate of cooling was compared in rabbits killed in the same manner, the respiration being artificially maintained in one, whilst the other was left to itself.   The following table shows the result, in a case in which the animals were selected as nearly as possible of the same size, colour, and general vigour:

TIME.	Rabbit with artificial Respiration.		Dead Rabbit.	
	Thermometer in the Rectum.	Thermometer in the Pericardium.	Thermometer in the Rectum.	Thermometer in the Pericardium.
Before the experiment	$100\frac{1}{2}$	...	$100\frac{1}{2}$	
30 minutes . .	97	...	99	
45 ,, . .	$95\frac{1}{2}$	...	98	
60 ,, . .	94	...	$96\frac{1}{2}$	
75 ,, . .	92	...	95	
90 ,, . .	91	...	94	
100 ,, . .	$90\frac{1}{2}$	$87\frac{1}{2}$	93	$90\frac{1}{2}$

From this it appears, that not only did the rabbit in which artificial respiration was maintained, cool *more* rapidly than the other, but the difference was more perceptible in the thorax than in the abdomen, indicating that the inflation of the lungs with cold air had assisted in producing the depression. In order to ascertain, as nearly as might be, the cooling influence of this inflation, the comparative experiment was repeated, a ligature being first placed around the base of the heart of the rabbit whose lungs were to be inflated, so as entirely to check the circulation. The following table exhibits the result:

TIME.	Dead Rabbit whose Lungs were inflated.		Dead Rabbit whose Lungs were not inflated.	
	Thermometer in the Rectum.	Thermometer in the Thorax.	Thermometer in the Rectum.	Thermometer in the Thorax.
Before the experiment	100	...	100	
30 minutes . .	97	...	98	
45 ,, . .	$95\frac{1}{2}$	...	96	
60 ,, . .	94	...	$94\frac{1}{2}$	
75 ,, . .	$92\frac{1}{2}$	...	93	
90 ,, . .	91	86	$91\frac{1}{2}$	$88\frac{1}{2}$

By comparing this table with the last, it will be seen, that whilst the difference in the temperature of the two rabbits in the first comparative experiment was $2\frac{1}{2}°$ in the rectum, and $3°$ in the thorax, it was in the second only $\frac{1}{2}°$ in the rectum and $2\frac{1}{2}°$ in the thorax; showing that, of the two rabbits whose lungs were inflated, the one whose circulation was maintained cooled more rapidly on the whole than the one whose circulation was checked—apparently because in the former case the continuance of the circulation propagated the cooling influence from the lungs to the remoter parts of the body.

In a second series of 'Experiments and Observations on the Influence of the Brain on the Generation of Animal Heat,' the results of which were published by Sir B. Brodie, in the 'Philosophical Transactions for 1812,' means were taken to measure the quantity of carbonic acid generated by

the animals on which artificial respiration was practised; and in most of these cases, it was found preferable to suspend the functions of the brain by inoculating the animals with the woorara poison, or the essential oil of almonds, rather than to diminish the extent of the circulation, and incur the risk of hæmorrhage, which the operation of pithing almost necessarily involved. Without entering into the details of these experiments, we may state as their general result, that the quantity of carbonic acid thrown off by the poisoned animals under these circumstances, in the half-hour which was the usual duration of the experiment, was *fully equal* to that which was generated in the same time by uninjured animals of the same size, and imprisoned in the same limited quantity of air; yet the temperature of the poisoned animals fell from 6° to 7° within this short time, whilst that of the others underwent no diminution. From the precautions with which these experiments were made, Sir B. Brodie having had the assistance of Professor Brande in the chemical part of the investigation, there seems no room for questioning the accuracy of the results; though at first sight these appear to make the production of carbonic acid of no account in the maintenance of heat, and to show that the process of calorification is so essentially dependent upon the functional activity of the brain, that heat must be regarded as one of the products of its operation. Any such inference, however, would be opposed, as we have seen, by such a vast array of facts, that some fallacy must be strongly suspected ; and this Sir B. Brodie seems himself to have felt; for he remarks :

"It would be absurd to argue from this fact, that the chemical changes of the blood in the lungs are in no way necessary to the production of heat ; since we know, not only that there is no instance in which it continues to take place after respiration has ceased, but that respiration is necessary to all the vital functions." (p. 32.)

In an appendix to this paper, now first published, Sir B. Brodie examines the results subsequently obtained by some other experimentalists, and compares them with his own. We could wish, however, that the comparison had been more complete ; as it is almost entirely limited to the experiments of MM. Legallois and Chossat, taking very little account of those of Dr. Wilson Philip, and making no mention whatever of those of Drs. Hastings and C. J. B. Williams. The first source of fallacy in the result of Sir B. Brodie's experiments, arises out of the difference between artificial and natural respiration. It is obvious that the mere inhalation and exhalation of air is in itself a cooling process; and that in proportion to the excess of air thus made to pass through the lungs, above that which is properly subservient to the true respiratory change, will there be a tendency to a preponderance of the depressing over the elevating power. In the normal condition of the animal, the respiratory movements are instinctively regulated in accordance with the wants of the system, so that no more air is caused to pass through the lungs than is demanded for the due performance of the respiratory process; but when artificial insufflation is practised, the result is certain to be very different from the normal one, and the probability is, that the work will be *over*-done. Such seems to have been the case in Sir B. Brodie's experiments ; for Dr. Wilson Philip found that when the artificial respiratory movements were *less frequently* performed, being repeated only twelve times in a minute, instead of thirty, as

in Sir B. Brodie's experiments, the cooling process was decidedly retarded, instead of being accelerated. Legallois ascertained, moreover, that by inflation of the lungs even of perfect and healthy animals, he could so far lower their temperature as to cause them to die of cold; and he showed that, even though animals in which artificial respiration has been maintained subsequently to pithing, may cool more rapidly than those which are left without this treatment, yet they part with a larger quantity of heat in doing so, showing that a certain amount of heat *is* generated in their bodies. Moreover, in the experiments of Dr. C. J. B. Williams, as in those of Drs. Wilson Philip and Hastings, an absolute augmentation of temperature was observed in some of the cases in which artificial respiration was performed after the cooling process had already commenced; and the results obtained by all these observers favoured the belief, that whatever may be the immediate source of the production of animal heat, it is essentially dependent upon that due performance of the respiratory process which no artificial means can effectually replace. Still, as Sir B. Brodie justly remarks, one of Chossat's experiments shows that this difference between artificial and natural respiration can only account for a part of the result ; for when the section was made in front of the pons varolii of a dog, so as to intercept the communication of the nervous influence from the brain to the spinal cord and nerves, but without interfering with the natural respiratory movements, which continued for many hours (as did also the pulse) as frequent as under ordinary circumstances. the animal at the end of four hours had lost 17·5° of heat, whilst a dog a very little larger, which had been killed by the division of the spinal cord in the upper part of the neck, lost nearly 16° in the same period of time. It is unfortunate that the amount of carbonic acid set free by the first of these animals was not ascertained, and compared with the amount liberated by an animal of the same size and species under ordinary circumstances ; for the results of Sir B. Brodie's experiments upon this point are vitiated, as we shall presently see, by the brevity of their duration,—as are also those of Legallois, which, as Sir B. Brodie successfully shows, are not really discordant with his own, although they seem at first to indicate that the amount of carbonic acid set free in the animals that are undergoing artificial respiration is considerably less than the natural standard.

We may, then, regard it as the general result of all these experiments, that, as Sir B. Brodie expresses it, " under certain circumstances there is no relation between the oxygen consumed in respiration, and the heat generated, while there is a very close relation between the calorific function and the integrity of the functions of the nervous system;" and the question now arises, what the nature of this latter relation may be,— whether the nervous system is in any way directly concerned in the production of heat, or whether it only exercises a calorific power through the medium of the organic processes over which it has a certain degree of influence, these last being the immediate source of that power. To the former of these opinions, it is evident that Sir B. Brodie has always been inclined; and he finds for them a new basis, in the doctrines propounded by Professor Grove and by Dr. Carpenter in regard to the "correlation of forces," physical and vital. " Supposing these views to be correct," he says, " may it not be that the union of carbon with oxygen gas, which, under ordinary

circumstances, is immediately followed by the evolution of heat, is, in the living body, productive of a different result (such as the maintenance of the nervous power, or the irritability of the muscles), and that it may thus be only indirectly concerned in the calorific function?"—whilst, on the other hand, taking into account the many points of strong resemblance between nerve-force and electricity, " there seems to be no reason, *à priori,* why the resemblance should not extend still further, nor why the evolution of heat should not be one of the results of the operation of nervous power as it is of electricity." To us it seems that no objection can be taken to the possibility of such a view, by any who admit the doctrines referred to; and it has the great advantage over all the previous notions which vaguely connected the production of animal heat with nervous activity, of recognising chemical change as its original source, neither the one force nor the other being capable of development without a consumption of alimentary material and of atmospheric oxygen; but still the question arises, whether it is alone, or chiefly, through the intermediation of nerve-force, that chemical change is subservient to animal heat. The phenomena of calorification in plants are of course adverse to such a doctrine; but, on the other hand, the close relation which we have seen to exist between the production of heat in the higher animals, and their nervous activity, would seem to show the essential importance of the latter. To us, however, it appears, that before we abandon the doctrine that chemical change in the system generally, in which a process of oxygenation takes place, is the immediate source of animal heat, we ought to be in a condition to prove that such change goes on without interruption after those severe lesions of the nervous system, which Sir B. Brodie and all other experimenters have found to be ordinarily attended with depression of temperature, even when the artificial respiration has been most successfully practised.

Now, as we have already remarked, the absorption of oxygen and the extrication of carbonic acid do not of themselves constitute the changes on which calorification depends; and it is quite possible that the former may continue *for a time,* and yet the latter may have been almost entirely at a stand. It is the great mistake of what we may call the ultra-chemical doctrine, to overlook this fact, for a fact it has been shown to be, by the researches of Edwards, Magnus, &c. The latter has proved that both oxygen and carbonic acid are present alike in arterial and in venous blood; and that the change which is effected by the respiratory process in the blood, mainly consists in the substitution of a portion of oxygen for a portion of its carbonic acid. The experiments of the former have shown, that this liberation of carbonic acid may go on for many hours, in cold-blooded animals, at the normal rate, in an atmosphere of hydrogen or nitrogen; and here it is obvious that the want of oxygen must have put an almost complete check upon those chemical changes, taking place in the *penetralia* of the system, by which alone carbonic acid could be newly generated, and that the continuance of the exhalation must have almost entirely proceeded from that charge of the gas which the blood previously contained. Now, although the more complete and rapid respiration of warm-blooded animals altogether precludes the idea that such could be the case with them for the same length of time, yet there is to us no unlikelihood, but, on the other

hand, a very strong probability in the idea, that during the half-hour, which was usually the limit of Sir B. Brodie's observations upon the quantity of carbonic acid exhaled, the proportion of that carbonic acid which was newly generated in the system was extremely small, and that the exhalation which was noted proceeded chiefly from the accumulation of the gas which the blood previously contained. At any rate, the extrication of a given measure of carbonic acid within a certain time, must not be held as a proof that a combustive process equivalent to its production had been taking place during that time; and any inferences founded upon such an assumption are altogether invalid. Now it is quite certain that the sudden destruction of cerebral influence has usually a marked effect in retarding the various vital transformations which are taking place within the body; and this was evidenced, in Sir B. Brodie's own experiments, by the cessation of the secretion of urine, which he almost constantly observed. Consequently, in the general depression of vital activity which the lesion of the nervous system may be considered to have produced, we think that we have an adequate *vera causa* for the depression of temperature; the continued exhalation of carbonic acid, at the usual rate, during a short period after the suspension of cerebral power, being, in our apprehension, a fact of quite second-rate importance, since it is (so to speak) the expression of previous changes, not of those taking place during the period in question. And this view is confirmed by the striking fact, that the generation of animal heat may take place without any corresponding exhalation of carbonic acid, and under circumstances which seem to forbid the idea that nervous force can have any concern in its production. Thus it is well known, that after death from Asiatic cholera, an elevation has frequently occurred in the temperature of the body, which had been considerably depressed before death; and this phenomenon is even more strikingly exhibited after death from yellow fever, the subjects of which present many other phenomena indicative of a remarkable persistence of molecular life after the cessation of the general circulation. For, according to the observations of Dr. Bennet Dowler, of New Orleans,* the thermometer, whose highest elevation during life was in the axilla 104°, had risen to 109° in the same situation within ten minutes after death; fifteen minutes afterwards it was 113° in an incision in the thigh; in twenty minutes, the liver gave 112°; in one hour and forty minutes, the heart gave 109°, and the thigh in the former incision 109°; and in three hours after the removal of all the viscera, a new incision in the thigh gave 110°. It is curious that, in every one of the five cases noted, the temperature of the *brain* remained at the ordinary standard, whilst that of every other part of the body which was examined, rose from five to eleven degrees.

From this general review of the subject, then, we are led back to the conclusion that the " chemical theory" of animal heat is essentially correct; and that its apparent inconsistency with the experimental results obtained by Sir B. Brodie and others, is to be in great part accounted for by the entire want of agreement which has been proved to exist, between *the amount of carbonic acid exhaled in any given time,* and *the amount of chemical change taking place in the system during the same period,*—a source of

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* Western Journal of Medicine and Surgery, June and Oct. 1844 ; cited in Philadelphia Medical Examiner, June, 1845, and in Prof. Dunglison's Human Physiology, 7th edition, vol. ii. p. 718.

fallacy which seems to have been completely overlooked on both sides. We believe, therefore, that the rapid lowering of the temperature of animals, whose brains have been cut off from connexion with the spinal cord and its nerves, or have been paralyzed by the influence of a narcotic poison, is mainly due to the retardation of all those chemico-vital changes which in the normal state are concerned in the production of heat; this retardation being what we might anticipate from the withdrawal of nervous power, which, if not the cause of those changes, has a most important influence on their rate of activity.

That the production of heat *is* affected by conditions of the nervous system, we conceive that there can be no doubt whatever; among those at least who attentively weigh the foregoing and many other facts which have been witnessed, both as results of experiments, and as spontaneously presenting themselves in the course of diseases of various kinds. The withdrawal of cerebro-spinal influence from a part seems usually to tend to produce a depression in its temperature; and this especially in the extremities. Thus, in the well-known paper of Mr. H. Earle,* several cases are recorded in which the temperature of paralyzed limbs was slightly lower than the temperature of the corresponding sound limbs. And a case of hemiplegia, of five months' standing, is mentioned by Professor Dunglison,† in which the temperature of the axilla was $96\frac{1}{2}°$ on the sound side, and $96°$ on the paralyzed, whilst that of the hand was $87°$ on the sound side, and only $79°$ on the paralyzed; and in another case, of only a fortnight's duration, the temperature of the axilla, which was $100°$ on the sound side, was only $98\frac{1}{4}°$ on the paralyzed, whilst that of the hand was $94°$ on the sound side, and $90°$ on the paralyzed. But it is a remarkable fact, that instead of a depression, an elevation of temperature is sometimes observable in paralysis. Thus, it has been observed by many experimenters, that one of the first effects of division of the spinal cord in the lower part of the back in warm-blooded animals, is to raise the temperature of the posterior extremities—this elevation continuing for some hours. And Professor Dunglison states, that in hemiplegia it is not uncommon for the thermometer to rise higher on the paralyzed than on the sound side. The following case, observed by Sir B. Brodie, is perhaps the most remarkable example of this kind yet recorded.

" A man met with an accident which occasioned a forcible separation of the bodies of the fifth and sixth vertebræ of the neck, attended with an effusion of blood within the *theca vertebralis,* and a laceration of the lower part of the cervical portion of the spinal cord. It is well known to surgeons, that, under such circumstances, inspiration is always very imperfectly performed by means of the diaphragm only, without the assistance of the ribs; while, in consequence of the paralyzed state of the abdominal muscles, expiration depends altogether on the pressure of the abdominal viscera acting on the relaxed diaphragm. The patient, in this instance, died at the end of twenty-two hours from the period of the injury, and for a considerable time previously to his death breathed at very long intervals, the pulse being weak, and the countenance livid. At last there were not more than five or six inspirations in a minute. Nevertheless, when the ball of a small thermometer was placed on the inside of the groin, the quicksilver rose to $111°$ of Fahrenheit. Immediately after death the temperature was examined in the same situation, and found to be still the same. The case occurred under my care in

* Medico-Chirurgical Transactions, vol. vii.
† Human Physiology, 7th edition, vol. ii. p. 238.

St. George's Hospital, in the year 1821, at which time Mr. Cæsar Hawkins was house-surgeon; and the facts were observed and noted by Mr. Hawkins and myself, and witnessed by several of the students." (p. 121.)

Sir B. Brodie also cites a case from John Hunter's Lectures, of a gentleman seized with an apoplectic fit, whose body became extremely cold in every part, continuing so for some time, and then becoming extremely hot, this alternation being repeated several times, without any sensible alteration in the pulse or breathing.

Among the most remarkable and apparently the most constant phenomena, indicating a close relation between conditions of the nervous system and the development of heat, are those which have been recently observed by M. Cl. Bernard, and communicated by him to the Biological Society of Paris. Of these observations, which show that the section of the trunk uniting the sympathetic ganglia on one side of the neck, still more, the removal of the superior cervical ganglion, produces a persistent elevation of temperature of that side of the neck and face, to the extent of from 7° to 11°, we have already given an account;* and we have now, therefore, only to add, that we understand M. Bernard to have further shown, that when the animals so treated were kept in a *heated* atmosphere, until the temperature of their own bodies underwent a considerable elevation, this elevation did *not* affect the side on which the sympathetic had been divided, which maintained the standard it had previously attained; and the same result was also noted, when the animals were kept in an atmosphere so *cold* that the temperature of the rest of the body was considerably lowered.

In those local alterations of temperature, then, which are dependent upon conditions of the nervous system, we have a class of phenomena which the chemical theory, *as commonly received*, is entirely unable to explain. But, as we have endeavoured to show, this theory, when properly understood, leaves a wide range of operation for the nervous system, in the *regulation* of those metamorphic changes in the components of the body at large, to which it refers the production of heat. And if, as we think it far from improbable,—the doctrine indeed having been originally suggested in our own pages,†—the nervous system may generate heat, as it generates electricity, as a 'correlated' form of nerve-force, the chemical theory is not in the least invalidated; since any such production of heat must, like the generation of nerve-force itself, involve a retrograde metamorphosis of nerve-tissue; and thus its continued production through the instrumentality of the nervous system is not less dependent upon a supply of the appropriate material of nerve-tissue, than if it was immediately generated by the combustion of alimentary substances burned-off as fast as they are introduced.

It scarcely appears to us, that the general proposition—that the generation of Heat in the living body is dependent, like the generation of other forces, upon changes in the state of combination of its component elements, which may be comprehensively expressed by the term combustion—*can* be invalidated by any accumulation of facts of the order we have last noticed. It is most desirable, however, that such facts should be collected and compared in as great a number as possible; since they

* Vol. ix. p. 543.   † See vol. i. p. 233.

tend to fill up the great hiatus in our knowledge of the changes with which the production of heat is immediately and essentially connected. And the experiments of M. Cl. Bernard appear to open-up an entirely new field of inquiry into this subject, which will, we trust, be diligently and successfully cultivated.

In bringing this discussion to a conclusion, we would tender our thanks to Sir B. Brodie for having, by the republication of his valuable memoirs, and by the notes which he has appended to them, led us to direct the special attention of our readers to the influence of the Nervous system on Calorification, which the indifference of most of the advocates of the Chemical theory to every fact that does not readily harmonize with their views, has led them altogether to ignore; whilst, of those who did take cognizance of them, many have been induced, by the vast array of facts that can be brought in support of the chemical doctrine, and by the tone of dogmatic certainty with which it has been advanced, to conclude that every observation that presents an appearance of inconsistency with it must, from that very circumstance, be fallacious. It has been our aim to show that the apparent inconsistency is not real; and that the chemical doctrine cannot be made really complete, save by the assistance of nume-rous facts of the very class which has been regarded as antagonistic to it.

The other two Memoirs, from the 'Philosophical Transactions,' for 1811 and 1812, contain the account of numerous valuable experiments made to determine the manner in which various poisons, vegetable and mineral, act upon the animal body, and produce death. The whole of this subject, at the period in question, was involved in great obscurity; and Sir B. Brodie's experiments were among the first which threw the light of physiological science upon its numerous and complicated problems. We need scarcely inform our readers of the rapid progress which has been made since that period, in our knowledge of the *modus operandi* of poisons, or enter into the satis-factory evidence we now possess, that, in by far the larger proportion of cases, the effects of poisons upon the system at large, or upon remote organs, and even upon the parts to which they may have been themselves applied, are due to their reception into the current of the circulation. Of this general fact, some of the most valuable data were furnished by Sir B. Brodie's experiments; especially by those which proved that the local action of arsenic on the stomach is equally developed, when it is introduced into the circulation through some remote part of the body. He now seems ready to admit that this principle is capable of being extended also to many poisons, which have a powerful action on the nervous centres, and which he formerly believed to " produce their effects through the medium of the nerves, independently of their being absorbed into the circulation;" but still, he remarks that " other facts may be adduced, which render it doubtful whether the whole of the phenomena admit of this explanation, at the same time that the analogy of what happens under other circumstances justifies us in regarding the agency of the nerves in transmitting the influence of certain poisons to the vital organs as no improbable hypo-thesis." In this view we fully concur; and considering that the question cannot be better stated than it has been by Sir B. Brodie, in his Additional Notes, we extract the following summary of the grounds on which he rests it:

" 1. The rapidity with which the poison operated in some of my own experiments, in those of Dr. Christison made with the active principle of hemlock, and of M. Magendie and Mr. Taylor with the hydrocyanic acid, is even greater than can be well accounted for otherwise, however rapid the circulation, and however easy the transmission of the poison may be through the substance of the mucous membranes and the tunics of the bloodvessels.

" 2. In the first of my experiments on alcohol, the introduction of two drams of proof spirit into the stomach of a cat immediately affected the nervous system to such an extent as to cause total insensibility with laboured and stertorous respiration ; nevertheless after the lapse of eight minutes these symptoms began to subside, so that presently the animal was able to stand and walk.   In another experiment, in which as much as an ounce and half of proof spirit had been injected into the stomach of a full-grown rabbit, the insensibility which it occasioned began to subside at the end of 40 minutes.   It is easy to understand that the effects of the impression made by the poison on the sentient extremities of the nerves, like those of a concussion of the brain, should thus subside, but it does not seem very probable that so large a quantity of spirit should have been absorbed into the circulation and then ejected from it in so short a space of time ; nor does this at all correspond with what happens where intoxication is gradually induced in the human subject, and where there can be no doubt as to the alcohol having entered the circulation.

" 3. That poisons may have a local action on the nerves, so as to affect distant organs, independently of their admission into the blood, is proved by the well-known fact to which I have adverted elsewhere, of the pupil of one eye becoming dilated in consequence of the application of the extract of belladonna to the conjunctiva of the same eye, or the neighbouring part of the integuments, while the pupil of the other is wholly unaffected by it.

" 4. There are numerous examples of mechanical impressions on the sentient extremities of the nerves, the influence of which can be propagated only through the medium of the nerves themselves, affecting the brain so as to occasion a temporary suspension of its functions.   Every practical surgeon will recal to his mind numerous instances of a common, simple, and bloodless operation being followed by syncope, and even by stupor, with dilatation of the pupils, and stertorous respiration, and continuing for several minutes ; and there is, *à priori,* no evident reason why the impression made on the nerves by so powerful an agent as the hydrocyanic acid, or the essential oil of bitter almonds, should not do in one case what a slight mechanical injury does in another.

" 5. In another publication I have referred to another class of cases, the phenomena of which cannot be well explained except by attributing them to an influence transmitted through the medium of the nerves.   Thus, in one instance, acid in the stomach caused a severe pain in the foot, which was immediately relieved by a dose of alkali neutralizing the acid.   In another instance a violent pain in the ankle, with inability to move, subsided immediately on the rejection of some indigestible food from the stomach.*

" 6. I have elsewhere adverted to the analogy which exists between the operation of the nervous power and that of electricity.   The influence of volition is transmitted instantaneously from the brain to the muscles, and impressions on the sentient extremities of the nerves are communicated to the brain, the nerves themselves answering the same purpose as the conductors of an electric apparatus.   The agent in both instances is invisible, intangible, and known only by the effect which it produces ; and, these things being considered, it seems to be not contrary to what analogy would lead us to expect, that in like manner as the electric force, generated by chemical decomposition at one end of a metallic wire, directs the needle of a telegraph, or causes the explosion of gunpowder, at the other extremity, so a substance, such as the hydrocyanic acid, which powerfully affects the vital

* Lectures illustrative of certain Local Nervous Affections, p. 11.

properties of the part to which it is applied, should be capable, through the medium of the nerves, of disturbing, and even of arresting, the functions of the brain." (pp. 129—132.)

To this we may add, that it has always appeared to us that the effects of *shock*, as propagated not only from the nervous centres to the system in general, but also from remote parts of the system to the nervous centres, and thence to the heart, are quite conclusive as to the transmissibility of an influence, which we may call "anti-vital," along the nerve trunks.   Of such an influence we see the results in the extreme prostration which imme-diately follows the rupture of internal organs, extensive burns of the surface (especially in children), the action of corrosive poisons on the stomach, &c.; and there is to us a strong *à priori* probability, therefore, that the peculiar influence of certain poisons, whose special action is exerted on the nervous system, should be received and transmitted through it, as that of other poisons is through the circulation.

We believe that to Sir B. Brodie is due the merit of having first sug-gested artificial respiration as a means of reanimation after apparent death under the influence of narcotic poisons; and with an extract from his Additional Notes on this subject, we shall close our present review of his physiological labours; again thanking him for the opportunity which he has given us, by the republication of these memoirs, of reverting to topics of such interest, and of profiting by his more matured thoughts upon the subjects of his early investigations.

"The success of the treatment depends, 1st, in cases of poisoning, on the dose of the poison, there being a limit to the period during which life can be maintained by means of artificial respiration ; 2ndly, on the inflation of the lungs being carefully made ;* 3rdly, on the animal being kept in a temperature of not less than 85 or 90 degrees of heat of Fahrenheit's thermometer.   This last precaution is of course a matter of greater importance where the animal is of a small size (as in the case of a cat or rabbit), than where it is larger ; still it is not to be neglected even in the case of the human subject ; otherwise the animal heat gradually diminishes until it reaches that point at which the action of the heart can no longer be maintained, when we have the singular result of an animal perishing from cold in the ordinary temperature of the atmosphere.   I have not myself known the circulation to con-tinue where the temperature of the interior of the thorax has been below 78° of Fahrenheit ; but an experiment is related by Dr. Chossat, in which it had fallen still lower.

"It is needless to multiply examples of the kind.   I am, however, induced to record the following experiment, as it derives a peculiar interest from the circum-stance of the use of ether and other anæsthetic agents having been lately introduced into the practice of surgery.

"February 5, 1821.

"A guinea-pig was placed under the bell-glass employed in my experiments on animal heat.   The bell-glass was left open above, while a small retort containing sulphuric ether was adapted to the tube communicating with the lower part of the apparatus.   A lamp was placed below the retort, so as to make the ether boil.   The vapour of the ether thus became mixed with the air in the bell-glass, a portion of it becoming condensed on the inner surface of the latter, and on that of the wooden stand on which it was placed.

"In two minutes after the experiment was begun, the animal moved about briskly, as if affected by the first symptoms of intoxication.   In two minutes more

* On this subject I have offered some observations elsewhere.   See Lectures illustrative of various Subjects in Pathology and Surgery, p. 7

he lay on one side in a state of insensibility, but still breathing. He continued in this state, breathing at longer and longer intervals, for six minutes, when respiration had entirely ceased. After two minutes more he was removed from underneath the bell-glass. Though he was apparently dead, the heart could be felt beating feebly through the ribs. An opening having been made in the trachea, the lungs were now artificially inflated. Only a few seconds had elapsed before there was a spontaneous effort to breathe, and the pulsations of the heart were more distinct. When the artificial respiration had been kept up for some minutes longer, it was discontinued. The animal now breathed naturally, and gave some slight indications of sensibility when touched. This was followed by a tremulous motion of the limbs, and soon afterwards by complete recovery." (pp. 142—145.)

ART. XII.

1. *Die epidemische Ruhr in allgemeinen Krankenhause zur Prag.* Von Dr. FINGER. ('Prag. Vierteljahrschrift,' Band iv., und CANSTATT'S 'Jahresbericht,' 1849.)

The Epidemic Dysentery at Prague. By Dr. FINGER.

2. *On the Dysentery of India.* By JOHN TAIT, Esq., Assistant-Surgeon Madras Retired List, H.E.I.C.S. ('London Journal of Medicine,' August 1851.)

3. *On Bengal Dysentery and its Statistics, with a Notice of the use of large Enemata in that disease.* By JOHN MACPHERSON, M.D., First Assistant, Presidency General Hospital, Calcutta. 1850. Pp. 63.

4. *On the Treatment of Tropical Dysentery by means of Enemata of tepid water.* By J. E. HARE, M.D. ('Edinburgh Medical and Surgical Journal,' July, 1849.)

5. *Cases of Dysentery treated by Enemata of warm water.* By JAMES IRVINE, M.D. ('Edinburgh Medical and Surgical Journal,' January, 1849.)

6. *Zur Therapie der Ruhr.* Von Dr. EIMER. (HENLE'S 'Zeitschrift,' Vol. x. p. 238.)

The Treatment of Dysentery. By Dr. EIMER.

7. *Zur Chemie der Ruhr.* Von Dr. ŒSTERLEN. ('Zeitschrift für Dat. Med.,' Band vii.)

The Chemistry of Dysentery. By Dr. ŒSTERLEN.

8. *Observations on the late Epidemic of Dysentery in Dublin.* By R. MAYNE, M.D., Physician to the South Dublin Union Workhouse, and Lecturer on Anatomy at the Richmond Hospital School. ('Dublin Medical Journal,' May, 1849.)

THE epidemic dysentery at Prague may be said to have commenced in February, 1846, and to have lasted till September, 1848. During this time, no less than 231 dissections were made in the Prague Hospital; and the paper of Dr. Finger, in which the general result of these is contained, gives us a most accurate account of the morbid anatomy of dysentery.

Dr. Finger is led by the morbid appearances to distinguish two forms, which were, however, with the exception of one case, always combined, and are considered to be but different degrees of the same process. In both forms

the seat of the disease was chiefly the large intestines, yet in some cases it extended into the small intestines.

1. The first or elementary form is termed intestinal catarrh, with especial implication of the follicles. In this stage, the large intestine viewed from without appeared more or less reddened, and was moderately distended with air; if the disease had been of short duration, its coats did not appear thickened. When the intestine was opened, a yellow, brownish, greyish red, or sometimes purulent-looking fluid, more or less intermixed with fæcal matter, flowed out. The mucous membrane was reddened in patches of variable size, and was usually covered with a greyish-white or greyish-red pulpy substance; underneath this the membrane itself was softened, and was easily removed in the form of a greyish-red jelly-like pulp. The solitary glands were swollen to the size of millet or hemp seeds (*zu Hirse bis Haufkorngröse*), by distension with transparent jelly-like mucus, which could be forced out by pressure; and were often surrounded by vascular zones. Afterwards the mucus became opaque, thick, and purulent, and then arose little (hemp-seed or lentil-sized) ulcerations of the glands; these were at first solitary, then ran together by the destruction of the intervening mucous membrane, and formed ulcers varying in size from the original ulcer to that of a sixpence and larger; the borders of the ulcers were loose, ragged, swollen, and usually undermined. By the farther extension of these ulcers, very frequently large ulcerations arose, which spread over great part of the colon, and were connected by bridge-like portions of mucous membrane. Here and there little pits were formed by suppurated follicles, which sometimes passed through and even perforated the peritoneum. The submucous tissue formed, however, the most frequent floor of the ulcers.

When the process extended into the small intestines, it was found in an earlier stage than in the large, showing that the affection of the ileum was subsequent to that of the colon. The follicles were here also affected. In two cases of the 231, the jejunum and ilium were diseased, while the large intestines were healthy. Of the large intestines, the transverse colon and the sigmoid flexure were most affected. In some cases there was extensive catarrhal redness (*catarrhalischer Röthung*) of the small and large intestines, with numerous small, isolated, usually round aphthous erosions.

The tendency to gangrene of the intestines was not so great in the cases of pure follicular disease, as when the next form was combined with it; and when it did occur, it attacked chiefly the undermined mucous membrane. By longer duration of the follicular disease, which sometimes lasted even for many months, without attendant croupous exudation on the mucous membrane (which was chiefly the case in children and feeble anæmic adults), the intestinal coats were thickened and anæmic, and the mucous and submucous coats were softened, infiltrated with serum, and thereby thickened, either pale as if washed, or of dark, smutty, or slate-grey colour from pigment, or they presented yellow or grey projections (*Bückeln*), between which ran enlarged and varicose vessels. The follicles were often collapsed, with sometimes a brown or black circle of pigment. The process could, however, be seen in various stages; in some parts the follicles contained fresh infiltration-matter, or had suppurated; in other parts they had

healed. By successive attacks of portions of the mucous membrane, the disease thus lasted sometimes for months.

The cicatrization occurred by the borders of the ulcers becoming covered with a white more or less thick callus, which joined on to a more or less shining smooth tissue, which spread over the base of the ulcer. The cicatrices sometimes formed hard fibroid intersecting cords and elevations, by the side of which were often still swollen and suppurated follicles. The submucous tissue was, like the mucous, changed into a firm, thick, fibroid formation.

The mesenteric glands were found unchanged in cases of short duration, they subsequently became swollen, injected, succulent, and softened, but without evident infiltration.

2. The foregoing changes were present in every case but one; but in addition there existed in many cases what Finger calls a " croupous exudation," which was either spread uniformly over great part of the mucous membrane, or was collected near the folds of the colonic mucous membrane. In colour it was whitish, greyish-white, or reddish; it had a granular (*körniges*) or scaly appearance, and was usually easily detached. Under the microscope, besides epithelium, and mucous corpuscles, there was an amorphous molecular mass, often mixed with blood-discs, or at a later date with pus-cells. According to the varying condition of this exudation, the intestinal contents presented various characters, and sometimes, by the suppuration of the exudation, were chiefly made up of pus mixed with blood. Underneath the exudation the mucous membrane was often softened, sometimes to a great extent, was at first reddened, and afterwards in chronic cases became anæmic, presented pigment-discoloration, and was thickened from serosity. The follicular lesion was combined with this exudation in 230 cases, and was absent in one, and was generally far advanced before the croupous exudation commenced on the other portions of the mucous membrane. Sometimes the follicular ulcers were themselves covered with croupous exudations, which contained no pus-cells. Many stages were generally seen in the same intestine; in some places the mucous membrane, deprived of epithelium and covered with exudation, presented many ecchymosed vessels running through it, and was easily detached in the form of a reddish pulp; at other places, the membrane was bluish or slate coloured, elevated in knobs, and with its submucous tissue converted into a thick callus-tissue, while the follicles of the neighbouring membrane were filled with purulent infiltration, or were destroyed by ulceration.

The tendency to gangrene was considerable when there was croupous exudation, but was much less marked in the follicular disease. It occurred in about one-third of the whole number of cases. It was confined to small follicular ulcers in only three or four cases; in other instances it spread over a considerable extent of surface. The gangrene often occurred in a very short time, especially in persons previously healthy; and it was observed with surprise, that it was less marked in persons who, before the attack of dysentery, had laboured under a blood-disease, such as puerperal fever, typhus, gangrenous tuberculosis, or cancer. The coincident or subsequent conditions of the intestines were, peritonitis without perforation, peritonitis with perforation and effusion, and narrowing of the intestine from healing of the ulcers.

Before proceeding with the analysis of this able paper, we may observe that the observations of Finger appear to throw great light on the much-disputed question of the resemblances and differences between tropical and European dysentery. In spite of the interesting observations of Dr. Baly, which were reviewed in the pages of one of our predecessors,* we could not, without some violence, reconcile the various descriptions of the morbid anatomy of dysentery. We found that the most recent writer on the Dysentery of India, Dr. Parkes,† described as an acute disease what appeared in the pages of Rokitansky as a chronic and rather rare affection; while the dysenteric process of Rokitansky was evidently considered by Parkes either as an independent and rare affection, or as an epipheno-menon to the true dysenteric condition. But there cannot now exist a moment's doubt that Finger and Parkes have given the same description of the follicular disease; for so perfectly do their respective accounts tally, that no important differences, either in statements made or omitted, can be traced between them. The "croupous exudation" of Finger appears also to correspond in great measure to the "effusion of lymph" described by Parkes as following the ulceration of the follicles, and as occurring upon the unulcerated membrane, and between the coats. The term "lymph" must be considered as an ambiguous expression, which might be applied to an exudation having the microscopic characters described by Finger. The greater tendency to gangrene, when this exudation occurs, is alluded to by both writers; we must, however, believe that Finger's estimate of the infrequency of gangrene of the follicular ulcers is pitched too low; for the valuable observations of Dr. Baly are to us conclusive evidence of the frequent occurrence of gangrene in this condition, although the extensive gangrene and softening of the coats, which is sometimes found, is probably more often connected with the failure of nutrition, and perhaps obliteration of vessels, which accompanies the extensive and rapid exudation. The obliteration seems to be obvious, from the subsequent general anæmia, and enlargement of other and smaller vessels. The observations of Finger have certainly placed this exudation in a more important light; it occurs very frequently, and is perhaps more immediately dangerous than the follicular disease, and it may, in very rare cases (1 in 231, or ·43 per cent.‡), be the only dysenteric process. It will also be observed that Finger points out, that the ulcers may not only be formed by extension from follicular ulcers, but also by separation of the sloughs, at the parts where the "croupous exudation" has caused gangrene. We have no doubt that this is correct, and have, in fact, ourselves described the same fact on a former occasion, in the following words:

"We have witnessed several times in dysentery, and in other cases in which there was dark inflammatory congestion of the mucous membrane tending towards diffuse sphacelus, roughness of the mucous membrane, with effusion of granular lymph upon it. This lymph has been in many cases evidently effused over the course of the larger vessels, and it has generally occupied the summits of the plica. Although it has often been effused upon the mucous coat, in some instances it has seemed to press on and destroy this coat, and then by its disintegration and detach-ment to form, apparently, sloughing ulcers."§

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* British and Foreign Medical Review, vol. xxiv. p. 333.

† Remarks on the Dysentery and Hepatitis of India, 1846.

‡ It is not improbable that the uncomplicated exudation may be more infrequent even than this in some epidemics.

§ British and Foreign Medical Review, vol. xxiv. p. 344.

Admitting this origin of ulceration, although we suspect it is comparatively rare, dysenteric ulcers appear from the observations hitherto made to originate in six ways, which we may arrange in what is probably the order of frequency.   Ulceration occurs—

1. From infiltration into and subsequent ulceration of the solitary glands.
2. From infiltration into and sloughing of the solitary glands.
3. From detachment of sloughs, following the " croupous exudation."
4. From the effusion of a fluid beneath the mucous membrane, raising and finally detaching it.
5. From ulceration of the mucous membrane at the base of inflamed glands.
6. From aphthous erosions.

It still remains obscure how Rokitansky should have omitted all mention of the affection of the solitary glands in the acute disease.   This omission on the part of so accurate an observer, either implies that the extensive superficial inflammation with croupous exudation and consequent rapid gangrene, without follicular disease, may, in some epidemics, be the sole anatomical sign of dysentery, and that Rokitansky's acute cases had been all of this description; or that there is another disease, in which the solitary glands are never primarily and specially affected.

But to return to the Prague epidemic.   The dysentery was frequently complicated with other diseases.   Thus, in the 231 cases, there was tuberculosis in 48; cancer in 24; typhus in 11 (which either preceded or followed the dysentery); puerperal fever in 10; " croupous" pneumonia in 11; lobular pneumonia in 16; secondary syphilis in 9; Bright's disease in 8; chronic bronchitis in 5, and recent bronchitis in several; heart-disease in 3; diabetes in 1.   The astonishing comparative infrequency of tuberculosis and frequency of cancer is shown by the fact, that in 320 bodies with tuberculosis, in 48 there were traces of dysentery; while out of 50 bodies with cancer, in no less than 24 dysentery was present.   We may observe, incidentally, that Œsterlen, in the late epidemic at Dorpat, observed that in 46 dissections of dysenteric patients not one was tuberculous, although tuberculosis is very common at Dorpat.   As to the liver, it was generally pale and anæmiated; in one case there was inflammation of the vena portæ; *in not a single case was there abscess!*

In respect of symptoms, the Prague epidemic presented nothing unusual. It was noted that healthy motions were occasionally observed alternating with dysenteric stools, so that two or three fæculent motions by themselves were not considered as indicating improvement; but, as Eisenmann remarks, in his analysis of this paper,* in other epidemics this has not been the case, as fæculent stools always denote improvement.   In six cases there were vomiting and choleraic symptoms, with cold skin, whispering voice, and cramps. In four of these cases after death, in addition to the dysenteric appearances, there was " catarrh" of the mucous membrane of the stomach, which was injected, covered with thick mucus-like substance, and eroded with aphthous-like ulcerations, or even covered with diphtheritic exudation.   In two such cases the blood was dark and tarry, and without fibrine.   In other cases there were catarrhal inflammations of the bronchial and the urinary

* Canstatt's Jahresbericht, 1850, band iii. p. 178.

(especially the vesical) mucous membrane. General fever was very frequently absent. Nervous symptoms, such as delirium, cramps, and contractions, were often observed ; but after death no corresponding disease was found in the nervous centres.*

This epidemic attacked more women than men (216 to 106); it was most common during the summer, and occurred in so many patients in the hospital, as to excite a suspicion of contagion. In badly-ventilated rooms the disease was worst.

The treatment was very various, and in bad cases very unsuccessful ; general and local bloodletting were not used, and opium with ipecacuanha, tannin, and acetate of lead, were chiefly employed. Calomel was given, sometimes to salivation. Nitrate of silver and nux vomica were found to be useless.

The next paper on our list, by Dr. Tait, is one of those useful and practical papers which we are always pleased to meet with. The morbid anatomy of the disease is described in three phases—namely, of simple ulceration, of spreading ulceration, and of sloughing. The description of the first stage will be best given in the author's own words.

"The acute dysentery of India is found associated with three grades of ulceration of the mucous membrane of the colon. In the first, which may be styled simple ulceration, in contradistinction to the others, the ulcers in the earliest stage of their development are seen as minute points affecting the solitary glands, and from this to the size of half-a-crown they present every intermediate variety in point of extent. They are of circular shape, having a ground of coagulable lymph superimposed upon cellular membrane, which frequently presents a variegated appearance from the presence of minute red points in the subjacent muscular fibres. The circumference is guarded all round by an effusion of lymph under the mucous membrane, which directly limits the spread of the disease. The mucous tissue for the space of an inch, or an inch and a half, in the vicinity of the ulcers, is in an inflamed state. With reference to the ulceration originating in the glands, I may state that shortly after Dr. Parkes promulgated his views upon the pathology of the disease, I had an opportunity of verifying the truth of his observations by examining the intestines of a patient who died from an attack of cholera, after being three days in the hospital for acute dysentery. By the aid of the microscope, I discovered the small tumefied glands with an ulcerated depression in the centre, and with the eye unaided by optical instruments, I was enabled to trace the ulcerated specks in different stages of advancement to the size of a split pea. The appearances now described have been carefully looked for by me in the colon of different subjects, but except in those who had suffered from dysentery, I have not been able to trace them." (p. 207.)

In the second stage the ulceration undermines the border, and the ring of protective lymph breaks down; the ulceration then spreads to a greater or less extent, and the ulcers run into each. On the ulcers, layers of lymph are often deposited "in such quantity that the sore is prevented from contracting by a redundancy of lymph." (p. 705.) In the third stage or variety the "local change sets in with great intensity, and runs its course with fearful rapidity to the sloughing stage." "Pseudo-membraneous incrustations" cover the mucous membrane; and after death all the coats are found excessively softened, so as to break down under the hand.

The similarity, or we may say, the identity, of this description with that given by Finger, is manifest, and affords us still further ground for

* This observation recals to mind Dr. Baly's interesting cases at Milbank.

believing that there is more constancy in the morbid appearances of dysentery than has been supposed.

The remainder of Dr. Tait's paper is chiefly occupied with details of treatment as adapted to particular stages or particular varieties. He recommends general bleeding whenever the mucus which is voided in the earlier periods of the disease is either viscid and adherent to the vessel, or is voided in firmly coherent masses. This constitutes his grand indication both for depletion and mercury, but he expressly advises that mercury should not be used without depletion. Opium is also strongly recommended as an adjunct. In the second stage of spreading ulceration, the evacuations lose their tenacious character; they become watery, and resemble the wastings of raw meat; this symptom both indicates the extension of ulceration, and points out that mercury is to be discontinued. Dr. Tait now employs ipecacuanha and opium in full doses, with local bleeding and counter-irritation. Nitric acid and opium are also recommended. In the sloughing stage or variety, the stools are grumous and offensive, with shreds, débris, and pseudo-membrane. Dr. Tait states that he knows no remedy of use in this stage, and that everything he has tried has invariably failed. He makes one recommendation which is worthy of trial—viz., the employment of *yeast.* In convalescence, Dr. Tait speaks highly of nitric acid. There are many other interesting observations in this paper, which prove its author to be not only a sound practitioner, but an ardent cultivator of his profession.

The third paper we have to notice is also a very excellent one, and contains much information. Dr. Macpherson has been able to use the records of the General Hospital at Calcutta, and has thus had the advantage of studying facts collected by numerous individuals.

The *mortality* of both acute and chronic dysentery among Europeans, as calculated from the returns of the General Hospital for 20 years (1830 to 1849 inclusive), is 457 deaths in 2044 cases, or 22·3 per cent. This is greater than the mortality in the Bombay General Hospital, which on an average of 5 years was 18·3 per cent., and lower than the Madras statistics, which give, on an average of 10 years, 30 per cent. mortality among civilians, but only 5·3 per cent among the military. (!) (p. 5.) Among natives the mortality is 16·9 per cent. Dr. Macpherson objects to the severance of acute and chronic dysentery in the returns; but we cannot coincide with him in this view, for we believe that if chronic dysentery, which kills in a variety of ways, be not separated from acute, no certain results can be arrived at. The above calculations are indeed not comparable with other statistical returns, on account of this mixing up of acute and chronic cases.

The *prevalence* of dysentery varies according to season, and at Calcutta is nearly represented by the following figures. In the four cold months, 74 cases; in the three hot ones, 68 cases; in the five rainy months, 88 cases. The corresponding mortality (of both acute and chronic cases), is, for the cold season, 18·4 per cent.; for the hot season, 26·2 per cent.; for the rainy season, 31·4 per cent. The inconvenience of combining acute and chronic cases becomes again evident here; for a chronic dysenteric case may swell the mortality of any period, although, properly, the disease may have virtually run its course and determined the fatal result in a former period.

The "*pathological facts*" of dysentery are preceded by a brief abstract of 160 *post-mortem* examinations of acute dysentery, and 55 of chronic, taken from the records of the General Hospital.   Dr. Macpherson says:

"On going over the records of these cases, it is impossible not to be struck with the fact, that no two observers saw with the same eyes.   Thus, one gentleman has invariably found the mesenteric glands enlarged, while another describes the liver as dry in one half of the cases in which he mentions the condition." (p. 12.)

The following summary is given of the 160 cases of acute dysentery:

"The liver was found to be altered in   84
    „   to contain abscess in   21
    „   was enlarged in   40
    „   gorged or turgid in   4
    „   small in   7
    „   pale in   26
    „   granular or nutmeg in   22
    „   soft in   12
    „   hard in   5
    „   contains cicatrices* in   3
The gall-bladder appears to have been almost always full, and to have contained healthy or somewhat inspissated bile.
The ileum is noted as over-vascular or congested in   21
Slight ulceration and abrasion were seen in   3
It was in a state of sphacelus in   1
The stomach had its mucous coat over-vascular, or somewhat softened, in   4
    „   and ulcerated in   1
The large intestine was ulcerated in all, and chiefly at the cæcum, sigmoid flexure, and rectum.
The cæcum, transverse and descending colon were free from ulceration in   3
There was perforation, generally in the cæcum, in   8
There was ulcerative destruction of ileo-colic valve in   3
Suppuration of appendix vermiformis in   1
Thickening and stricture in   4
Dilatation in   1
Mesenteric glands enlarged and inflamed in   17
Spleen enlarged in   6
Kidneys diseased in   2
                                                                (p. 34.)

These numbers require, however, some qualification.   Thus Dr. Macpherson says, that in his table he has left a blank space when a particular organ is not mentioned.   Under the head of mesenteric glands the space is blank, or does not refer to the glands in 140 cases; and these are stated to be healthy only in 3, while, as stated above, they are said to be enlarged in only 17.   But no one who has dissected cases of dysentery can doubt that their condition must have been generally overlooked.   So with the spleen.   Again, in the case of the stomach and small intestines, we find many cases in which these organs are not noted, so that when it is stated that the ileum is ulcerated in 3, it should not be taken to be 3 in 160, 157 being healthy, but 3 in some number not stated, possibly a small one. So with the liver: no note at all is made of it in 35 cases; in 38 it is said to be healthy; abscess is, as above said, recorded in 21 cases.   Now, are we to

* " These cicatrices do not appear to be the sequelæ of abscesses."

deduct the 35 cases in which no note was made of the liver, or not? If we do not, the per centage of abscess in fatal acute dysentery is 13·1; if we do, it rises to 16·8, that is to say, 1 case of abscess to every 6 cases of fatal acute dysentery. The liver is said to be "altered" in 84 cases; are we to say 84 in 160, or 84 in 125, the 35 unrecorded cases being deducted. We beg to call the attention of Dr. Macpherson to these difficulties; the present exact system of statistics demands perfect accuracy on the part of observers, or their facts become useless for such calculations.

In addition to the summary given by Dr. Macpherson, we observe that sloughing of a greater or less extent of the large intestine is recorded in 40 of the 160 cases; or if we add 2 more in which the terms employed lead us to believe that gangrene existed, there were 42 cases of mingled sloughing and ulceration, or at the rate of 26·2 per cent., or rather under Finger's average of one third. In all the rest, Dr. Macpherson says, the large intestine was ulcerated, yet in Case 3 it is stated that there was no "perceptible alteration."*

An abstract of 55 cases of chronic dysentery is given, of which the following is the summary:

" The liver was altered in 31
    Abscess in 6
    Hydatids in 1
    Cirrhosis in 1
    Enlargement in 5
    Diminution of bulk in 8
    Nutmeg alteration in 6
    Pale in 11
    Hard in 4
    Soft in 1
    Contained cicatrices (?) in 1
    The gall-bladder and its contents seem generally to have been healthy; the
        bile occasionally rather pale coloured, but at other times the reverse.
    The large intestines were ulcerated in 50
    ,,   colon was contracted in 3
    ,,   cæcum nearly closed in 1
    ,,   colon perforated in 1
    ,,   stomach is noted as unhealthy in 6
    There was chronic inflammation and softening in 2
        ,,   increased vascularity in 2
        ,,   abrasion of pylorus in 3
        ,,   cancer of pylorus in 1
    The small intestines are noted as unhealthy in 12
    Ulceration or abrasion of ileum in 3
    Mesenteric glands enlarged in 16
    The spleen was enlarged in 4
                                                        (p. 44.)

It would appear from this table, that hepatic abscess is less common in chronic than in acute dysentery.

With regard to hepatic abscess in fatal *acute* dysentery, the various accounts given by Indian writers stand as follows:

* Case 3 was not surely a case of dysentery; there was no perceptible lesion of the large intestines, but an " immense abscess" in the liver. Mucous membrane was passed during life, which was found to have come from the ileum.

" Abscess occurs in Calcutta (General Hospital; Macpherson) 16·8 per cent.*

,,	,,	,,	(Medical College Hospital†)	. . 25·9	,,
,,	,,	Bombay	(General Hospital; Morehead)	. 40	,,
,,	,,	Madras Presidency (Annesley)	. . . .	. 50·97	,,
,,	,,	,,	,,	(Moulmein & Madras; Parkes‡) . . . . 21·73	,,
,,	,,	,,	,,	(Secunderabad; Innes) 17·9	,,
,,	,,	,,	,,	(Shanks) . . . . . 37·4	,,

The contrast between these results and those of dysentery in cold climates is remarkable. Among the 231 dissections made by Finger in Prague, there was not one case of abscess; in the autopsies made by Mayne in Dublin, there was no case of abscess. We have on a former occasion alluded to Dr. Baly's experience on this point:—" Among many hundreds of cases of dysentery observed at Milbank, not one has been complicated with hepatic abscess."§ It is evident that this subject is not sufficiently explained by the supposition that hepatic abscess succeeds ulceration of the large intestines, by means of phlebitis or of purulent absorption—if so, how is its rarity in cold countries to be explained? The connexion of dysentery and liver-disease is also not confined to hepatic abscess; the liver undergoes other alterations, enlargement, granulation, &c., which appear as intimately connected with dysentery as is hepatic abscess. The whole subject requires indeed a thorough examination.

After a brief notice of Chusan dysentery,‖ Dr. Macpherson devotes a page to a " true description of the changes produced by Bengal dysentery." He says, " The process is very generally one of mortification and sloughing, not of simple ulceration—i. e., the ulceration is often secondary, and occurs only when the sloughs are thrown off." (p. 47.) The descriptions of Baly and Rokitansky are the best, "although the latter has not met with the amount of ulceration which is common here." After stating that Parkes considers it to be a " process of ulceration universally commencing in the solitary glands of the large intestines," which, on referring to Dr. Parkes's work, we find to be not quite correct, he states that such " inflammation and ulceration of the solitary glands is very unusual, or has been very carelessly observed; and I believe it may be stated generally, that in Bengal dysentery they are not peculiarly or primarily diseased." (p. 48.)

In the short notices which Dr. Macpherson has given of the cases he has collected, very little has been said about the intestines, except that they were ulcerated or sloughing, and the early process cannot be ascertained from them, but if it be really the case that in Bengal dysentery the solitary glands are not affected, this is a most important point which we should like to have confirmed, but we have some doubts about this. At page 53, Dr. Macpherson gives one dissection at greater length than those in his table; and as it appears to us to be a most accurate description of the usual form of dysentery, we extract it.

* We have adopted our own calculation. Dr. Macpherson elsewhere (p. 49) gives his as 27 in 215 (acute and chronic).

† Macpherson, p. 49.

‡ There is an error in Dr. Parkes's calculation which we have corrected. He has estimated his own cases, and those of Dr. Innes, as 13 in 61, but the numbers given make 12 in 62, which, taking the two observations together, gives 19·35 per cent.

§ Gulstonian Lectures, 1847.

‖ In Chusan dysentery it is stated, that the large intestines are little affected, but that there is ulceration of Peyer's patches! An accurate description of this singular disease is a great desideratum.

"As the number of autopsies in cases of dysentery occurring in children at the General Hospital, is extremely small, and as no such case is included in the preceding tables, I add the *post-mortem* appearances in a case lately treated by my colleague, Dr. Cantor, and obligingly communicated by him to me. It will be seen that the case was one of simple ulceration without any sloughing; also that the ulcers were uniformly diffused along the mucous membrane, and that the solitary glands of the cæcum were not specially implicated; the process of ulceration was still going on, and that of reparation had not commenced at any point.

"Child, æt. 4, ill for three weeks, death preceded by convulsions.

"Large intestine studded throughout with equally diffused ulcers. The earliest stage of the ulcer was a raised white point with greyish contents (enlarged mucous follicle?) which enlarged and ulcerated, destroying the mucous membrane. The ulcers varied in size from a pin's head to a sixpence, and had raised margins of a white finely fringed appearance. The cæcum and rectum were somewhat thickened; appendix vermiformis studded with numerous minute grey points. There were nine lumbrici in the large intestine. The small intestines were healthy; slightly injected here and there; liver somewhat small; spleen healthy. There were clusters of enlarged mesenteric glands, and some single ones enlarged, while others were natural." (p. 53.)

This description appears to us to mark the chief points with great precision; the ulceration of the large intestines not arising from sloughing, but apparently, although Dr. Macpherson says otherwise, from inflammation of the solitary glands, or "mucous follicles," as Dr. Cantor terms them, and the enlargement of the mesenteric glands come out very clearly.

In regard to treatment, Dr. Macpherson is very brief; he states that "practitioners seem to have been gradually losing faith in the mercurial treatment." Ipecacuanha is "not so much trusted to now as formerly." Opium is more freely used. The acetate of lead is much praised. Of 227 cases treated in five years with bleeding and calomel at the Seaman's Hospital, 48 died, or at the rate of 21 per cent.; while in 80 cases, treated in ten years, with bleeding, opium, and astringents, only 10 died, or at the rate of 12 per cent. Dr. Macpherson alludes, also, to the practice of large enemata in dysentery, and regards them as of minor value, and as adjuvants to more active treatment.

The two next papers on our list, by Drs. Hare and Irvine, are chiefly occupied with details of the treatment of dysentery by injections. In order to get fluids into the colon effectually, these two writers have found it necessary to use the long tube, and to pass it up beyond the sigmoid flexure. From four to six pints of warm water are then injected, and occasionally other remedies, as nitrate of silver, acetate of lead, opium, &c., are used. The best results are said to follow this mode of treatment, although, as remarked above, Dr. Macpherson does not share this opinion. We have ourselves no power of judging on this point, and the evidence for and against the plan must be more extensive before we decide on its relative value. We understand that Dr. Hare is engaged in actively testing its efficiency in one of the hospitals at Calcutta, so that we shall doubtless soon be in a better position to discuss this practice.

The short paper by Dr. Eimer is written in praise of local applications in dysentery, such as nitrate of silver, and especially iodine, in the form of enemata. The latter is used in quantities of from five to ten grains, with an equal quantity of iodide of potassium, and two or three ounces of water. The injection is to be repeated generally two, or sometimes three or four

times a day; and when the rectum is very sensitive, a little opium is to be added. Acetate of lead and opium by the mouth are also recommended. The iodine injections are not spoken of as being always successful, but as very generally affording great and speedy relief.

The observations of Œsterlen, on the 'Chemistry of the Stools of Dysentery,' are of great interest, although we have some doubts how far they will generally be found to hold good. In our ninth volume (January, 1850, p. 245) will be found a sufficient account of these researches; we need now only recall the fact that Œsterlen found that there was an excessive elimination of water and albumen, and have directed attention to the paper in this place, because we deem it most desirable that the subject should be prosecuted in India. The stools should be collected free from urine, which was not done in any of Œsterlen's four cases; and the albumen dissolved in the fluid should be estimated separately from any insoluble sediment, and from epithelium. If these observations be correct, if in very severe cases two and a half ounces of albumen may be passed in twenty-four hours, or as much in eight days as the blood contained at the moment when the disease commenced, it is impossible to overlook the importance of the fact. But though in some cases there may be this immense discharge, yet there are others in which, for days together, comparatively small quantities are passed by stool, and the amount of soluble albumen appears small, while what is usually considered as fibrine, but which may be insoluble albumen, is in larger quantity.

The epidemic dysentery in Dublin, described by Mayne, requires but a short notice, as the original paper is in the hands of most of our readers. The details are drawn from the observation of the male patients admitted into one of the union workhouses. Of 1222 patients admitted, 393 died, of whom 179 were persons over 60 years of age, and 74 were children under 10. This great mortality in children arose chiefly from the simultaneous occurrence of measles, which was very generally complicated with dysentery.

The *post-mortem* appearances are given in general terms, and it is not stated how many cases were examined.

"Dissection," says Dr. Mayne, "showed that the disease consisted essentially in a severe inflammation of the large intestines, tending rapidly to ulceration, and sometimes even producing the death of the structures implicated." (p. 101.)

The large intestine in its entire extent was generally diseased, and most considerably so towards the rectum; the small intestine was usually found healthy, but in a "few instances, the mucous membrane of those intestines was excessively congested, although never actually ulcerated."

"The walls of the diseased intestine were always enormously thickened and indurated; grasped between the fingers, the part felt semi-cartilaginous, and to the scissors it offered an undue degree of resistance, cutting like brawn. The cavity of the intestine was found almost invariably contracted. In no instance were scybala present, and rarely were any traces of true fæces discovered, the contents of the lower bowels being similar to the matters last evacuated during life."

The mucous coat was variously coloured, from the most intense vermilion to the dark green and purple of gangrene. When the disease was very rapidly fatal, its entire surface was covered with a "branny exudation," which could be easily scraped off, and disclosed the subjacent membrane

red and superficially ulcerated. This probably corresponded with Finger's croupous exudation, although the co-existing disease of the follicles and ulceration were apparently less marked. This exudation gave the interior of the intestine an appearance like "ichthyosis or shagreen." In other cases there were "various forms of ulceration;" in many cases, where the ulcers were large, irregular, and jagged, they penetrated to the serous coat, but it is rather singular that in so large a number of cases there was no perforation. The state of the solitary glands, and the mode in which the ulcers were supposed to have commenced, are not noticed.

As already noticed, no case of abscess of the liver is mentioned; the liver was sometimes excessively congested, but "this was certainly not essential, for it was frequently absent."

Symptomatic fever always accompanied the dysentery. The other symptoms mentioned were of the ordinary kind.

The treatment which was most successful, was general or more frequently local bleeding, and mercury. Dr. Mayne speaks warmly of the good effects produced by mercury; he gave two grains of calomel and three of Dover's powder every fourth hour. Mercurial inunction was less useful than calomel by the mouth. Next to, and especially when used with, mercury, alkaline medicines, such as the liquor potassæ, with small doses of mercury, were found to be very soothing and beneficial. Astringents were always hurtful. Opium in large doses "most certainly aggravated the disease;" in small doses with mercury it was frequently used with benefit.

Art. XIII.

The Principles and Practice of Surgery. By William Pirrie, F.R.S.E., Regius Professor of Surgery in the Marischal College of Aberdeen; Surgeon to the Royal Infirmary, &c. &c.—*London*, 1852. 8vo, pp. 952.

Mr. Pirrie informs us, in a brief and modest preface, that the present work is not intended to compete with any of the existing treatises on the same subject, but is rather designed to supply the students of Surgery, at the University of Aberdeen, with a compendium of the author's lectures on surgery. This statement, perhaps, ought to silence criticism; for if we considered Mr. Pirrie's work as a mere text-book, supplying heads for oral commentary, it is exceedingly well adapted to answer the purpose in question. But the work goes far beyond that limited range; and, whatever Mr. Pirrie may have originally intended, it has received so much expansion in the process of preparation, that it cannot fail to enter into competition with the systematic treatises on surgery, with which the author, too modestly, disclaims all rivalry.

We have great pleasure in giving it as our deliberate opinion, that by far the greater part of the work is most carefully, judiciously, and ably written; and Mr. Pirrie has, for the most part, fully succeeded in his "endeavour to combine simplicity of arrangement, and conciseness and clearness of description, with the elucidation of sound principles and practice." (p. vii.) Indeed, were there an equality of performance throughout, we could indulge in the pleasure of speaking of the work in terms of

unqualified praise, and of recommending it as, perhaps, the best student's compendium of surgery in the English language. But the book has its faults—(and what book has not?)—which we the more willingly point out, as they are of such a kind that Mr. Pirrie can find no difficulty in remedying them in a future edition. We may be wrong in our conjecture, but we are inclined to suspect that the book was put together in a hurry. The author, it is evident, very carefully collected, and maturely considered and digested his materials; but the final operation of moulding them into shape for print seems to have been rather hastily performed. At least we can scarcely otherwise account for the want of due symmetry and proportion of parts that pervades the treatise; and still less for the very defective treatment, and even total omission, of a good many subjects, which should find their place in a work on the 'Principles and Practice of Surgery.' We shall endeavour to justify the opinion just expressed, by passing some of the principal chapters of the work rapidly in review.

To illustrate the want of proportion that exists between the several parts of Mr. Pirrie's book, we may contrast the length of the first chapter with that of the fourth. The latter chapter, On Burns (and an excellent one it is), occupies thirty pages; while the former, On Inflammation and its Results, including suppuration, acute and chronic abscess, ulceration, granulation, ulcers, grangrene, and sphacelus, is compressed into thirty-nine pages. It is not that a word too much is said respecting burns; but that scarcely enough is said respecting inflammation, while there is too little, by a great deal, respecting the results of inflammation. It is surprising, it is true, how much information Mr. Pirrie has contrived to convey within the narrow limits of this chapter; but from mere want of space much is of necessity omitted,—as an example of which, it may be quite sufficient to state, that absolutely not one word is said about the *treatment* of gangrene and sphacelus. We may here also observe, as a further example of the description of omission of which we complain, that nowhere in the work have we been enabled to find furunculus, anthrax, or paronychia, so much as mentioned.

It would be a tedious and unprofitable occupation of our space to carry our readers through a detailed examination of each chapter of Mr. Pirrie's work, with the view of specifying the more important omissions that struck us while perusing it. We shall dismiss this part of the subject by simply stating that, not to mention other surgical affections and maladies of more or less importance, we could find no allusion in the book to tetanus, wounds of the thorax, paracentesis thoracis, disease of the bursæ mucosæ, fracture of the lower jaw, ununited fracture, dislocation of the vertebræ, dislocation of the astragalus, the question of reducing old dislocations, aneurism by anastomosis, arterial varix, secondary hæmorrhage, varicocele, spina bifida, wry neck, false anchylosis, tenotomy of the ham-strings, ranula, salivary fistula, œsophagotomy, foreign bodies in the trachea and œsophagus, cancer of the tongue, perineal rupture of the urethra, puncturing the bladder. We might readily enlarge this list of omissions; but we shall only add, that not so much as a hint is given that amputation by the circular method has ever been proposed or performed, or that a limb can be removed by any other than the flap operation. And yet rhinoplasty and staphyloraphy are rather minutely described,—with which, of course, we find no fault; but, in our judgment,

these refined proceedings might have been much better omitted, than the subjects above mentioned, many or rather most of which are, comparatively speaking, matters of every-day occurrence, and of far greater interest and importance to the immense majority of practitioners.

So much for subjects that have been entirely overlooked; but it also occasionally happens, that Mr. Pirrie dismisses the topics he does discuss with a brevity that has little advantage over total omission. Thus, the following passage contains *all* that is said respecting retention of urine from stricture of the urethra:

" Should a case of retention present itself, caused by a hard and gristly stricture situated in front of the scrotum, and should it be found impossible to penetrate the stricture by the usual gentle manipulation, in which the instrument is held lightly and pressed against the stricture without any force, the surgeon should take hold of the hard part between the forefinger and thumb of the left hand, and pass down the catheter to the stricture, and gently and cautiously effect penetration, and lodge it in the bladder. Should the stricture which causes retention be situated behind the scrotum, and should it be found impracticable to pass the catheter by the most cautious, gentle, and dexterous manipulation, the proper practice is to perform the old operation of sending back a catheter to the stricture, cutting into the dilated portion of the urethra behind the stricture, when the urine will escape, and dividing the stricture by cutting forward upon the point of the catheter, and sending it back so as to lodge it in the bladder. Such are the most advisable proceedings when retention is caused by stricture anterior to the prostate gland." (p. 715.)

Retention of urine from stricture of the urethra is thus disposed of in eighteen lines; but about double as many *pages* are devoted to the operations of lithotomy and lithotripsy. Neither is there a syllable respecting the varieties, symptoms, and consequences of disease of the prostate gland, beyond the naked mention that retention of urine may result from the enlargement of the organ; and yet *three* methods of amputating the finger, each illustrated by a woodcut, are given at pages 738—741.

The faults of the work, however, are almost entirely those of omission, and not of commission. What Mr. Pirrie has done, is almost uniformly done well; and after having so freely noticed the defects of his treatise, it would be a manifest injustice were we not to submit to our readers some specimens of the portions of the book in which he has done himself more justice. It is a most difficult thing to select extracts for favourable notice from an elementary work, in which novelty is not, and ought not to be, pretended to; but we shall make our first quotations from that part of the chapter on *wounds*, which relates to the treatment.

" *The treatment for adhesion*—with reference, chiefly, to incised wounds, comprehends *four* important indications—namely, to arrest hæmorrhage—to remove foreign matter—to effect and maintain coaptation—and to guard against excess of vascular action.

" The *first* indication is fulfilled, by aspersion of cold water, if mere oozing exists; or, by the ligature, when a distinct artery is seen pouring forth its contents. The ligatures, one end of each having been cut off near the noose, are brought out between the lips of the wound, by the shortest route; and if numerous, are arranged without entanglement into one or more bundles. These should, when otherwise convenient, leave the wound at its most dependent part; so that the slight purulent secretion, which is pretty certain to occur in their track, may find the most direct and easy exit, and, by at once escaping, not interfere mechanically with the process of adhesion in other parts of the wound. In amputations of the extremities, they are usually brought out at the angles of com-

missure between the flaps. The method of cutting off both ends of the ligature, and leaving only the knot, is now restricted, by almost all surgical authorities, to those cases in which the wound has no chance of uniting by the first intention.

" The *second* indication, which is to remove all foreign matter, including coagulated blood, should be attended to as soon as active bleeding has been suppressed. Were its fulfilment neglected, adhesion would in consequence be prevented.

" All oozing having been completely arrested, foreign matter removed, and the surface of the wound having taken on a glazed appearance ;—the *third* indication, —namely, to effect and maintain coaptation, should next be proceeded with. Such are the conditions which render coaptation advisable; and with regard to the means employed for effecting it, they are, position, plaster, sutures, when necessary, and, in some particular circumstances, carefully adapted pressure.

" The position should be such as will best relieve tension of the muscles and integuments, and obviate venous congestion. A greater amount of relaxation is necessary in some wounds than in others. As muscles are the principal agents in causing retraction, and in preventing easy coaptation, the general rule is to put the limb or part into the position that would be given to it by the natural contraction of the wounded muscle. When muscular fibres are cut transversely, there is much greater retraction, and consequently more necessity for the observance of a position that will secure relaxation, than when the wound runs parallel to, or between them, in which case relaxation might be carried too far, by making the sides of the wound bulge loosely, and thus preventing accurate coaptation. In such cases, the parts should be laid so as sufficiently to relieve tension, without permitting undue laxity. In amputation wounds of the limbs, where little relaxation is necessary, more than is already present, elevation, to such a degree as to prevent congestion, is the chief point of consequence with regard to position." (pp. 60, 61.)

After describing, in the succeeding paragraphs, the varieties of plasters and sutures in use among surgeons, and the principles which should govern their employment, Mr. Pirrie continues :

" In many, indeed in most wounds, no other retentive apparatus than suture and plaster need be employed; but in certain cases, when the wound is very deep, and its sides exceedingly loose,—conditions occasionally co-existent in persons of flabby fibre, and which favour the accumulation of secretions between the parted sides,—it may then be advisable to surround the wounded part with a turn or two of a bandage; under which, but not over the mouth of the wound, a soft compress may sometimes be placed with advantage. The roller, at this early stage, must, however, be applied very lightly; so that it shall merely assist in giving support, and in preserving apposition of every part, deep as well as superficial; and operate more as a precautionary measure to prevent displacement during any irregular muscular twitching, than as an immediate means of retention. Gentle support— not actual and injurious pressure—is wanted; and the better to avoid this evil, it is well to damp the bandage previous to application; for the dry fibre soon imbibes moisture from the integument or the wound, and, in so doing, grows thicker and shorter, so as ultimately to become much tighter than when applied, or than was intended.

" The retentive apparatus having been thus applied, the wounded part is to be laid in a suitable position, combining relaxation and elevation. The latter is the point chiefly to be attended to after amputation; and, for this purpose, the stump or other part is laid on a soft pillow, or any convenient rest, over which, for the sake of cleanliness, is spread a piece of oilcloth, or of thin-sheet gutta percha, in order to prevent the parts beneath being soaked with any discharge. Along the margins of the wound, when large, a single strip of soft linen is placed, and kept moist with cold water; but in smaller and more sheltered injuries, this may be omitted. When the wounded part lies beneath the bedclothes, their pressure and heating effect must be prevented by a suitable cradle.

" All that now for some time requires to be done, is merely to keep the parts clean, wiping away any fluid secretion from the neighbourhood of the wound, but never actually touching its raw and tender margins. These matters being attended to, the part is to be kept otherwise, as far as possible, at perfect rest.

" Supposing all to go on well, the stitches, if such have been employed, are removed at the proper time, as before explained; but the plasters may possibly, in a large wound, require occasional renewal, owing to the fluid secretion trickling down and loosening their dependent extremities, or from their becoming unduly loose as the process of adhesion goes on and the edges spontaneously approximate more perfectly. When, from any of these causes, it becomes necessary to change the plasters, they should be seized by both their extremities, and raised from each end towards the centre, which overlies the line of wound, and from this, lastly, they are with gentleness to be lifted. If, on the contrary, the strip were seized at one end, and pulled off along its whole course towards the other, it is obvious that after passing the central part, it would, if at all adherent, be apt to tear away the edge of the wound covered by its last half, from that margin to which its first raised portion had been applied. In renewing plasters, no more than one or two of the old strips, however loosely adherent, should be removed at once, before supplying their place with new pieces; but as each slip is taken away, the vacancy is to be filled up before detaching another. This precaution is necessary, because if all the strips were removed at once, the wound, being unsupported, might fall open, and the tender adhesions—the work of several days—be in an instant destroyed. Any necessary moving of the wounded part, whether for correcting malposition, cleaning the support on which it rests, or applying fresh plasters, must be conducted with great care and gentleness.

" When ligatures have been employed, some of them will probably be loose by the end of ten days. Accordingly, about the expiry of that period, each ligature, except that on the main artery, which should be left undisturbed for at least a week longer, may be carefully isolated from the others, and gently pulled by the fingers or forceps. If loose, it will come away immediately; but if the slightest resistance be felt, no force must, on any account, be used to withdraw it; a few days longer being allowed to elapse before it be again tried. The utmost gentleness is to be observed in this proceeding, lest the ligature should be drawn away before perfect occlusion of the vessel has taken place; but with this precaution, it is better to try the ligatures, and remove them when loose, as if left to themselves they might remain in the wound long after they were detached, and thus retard its complete healing.

" Perfect healing, after adhesion has progressed favourably for about a week, is often retarded by an œdematous swelling, the result of undue vascular relaxation. In these circumstances, a bandage is to be applied, so as to give support, and exert a moderate degree of pressure. This, however, must neither be severe nor unequally disposed, because in either case it would excite irritation, and the swelling of œdema would soon give place to that of inflammation.

" It must always be remembered, that at any stage, however late, excess of vascular action may set in, and prevent the further progress of adhesion, or even destroy the union already effected. It is frequently induced by cumbersome dressings, officious sponging and rubbing of the wound, and by an over-stimulating diet.

The *fourth* indication, which is to repress inflammatory action, is fulfilled, partly by the simple local treatment just detailed, and partly by treatment directed to the system in general. The strict antiphlogistic regimen should be enforced, all stimuli removed, and perfect rest, general as well as local, enjoined. The food must be small in quantity, unstimulating in character, and given pretty cold. In feeble persons, and in individuals at an advanced period of life, the antiphlogistic regimen must, however, be instituted with great caution, and its effects closely watched; but, regarding these and many other points, the surgeon must be guided by the peculiar circumstances of each particular case." (pp. 65—68.)

The passage which describes the *treatment for granulation* is so good a specimen of Mr. Pirrie's clear and concise manner of treating a practical subject, that we shall give it entire.

" Wounds may require to be treated for granulation, either when inflammation has proceeded too far in a case which it was first attempted to heal by adhesion, or when, from the beginning, it was evident that granulation was the most suitable mode of healing, whether owing to loss of substance preventing coaptation, or to extensive contusion, or to the presence of foreign matter which could not be removed; all of which conditions are incompatible with the attainment of adhesion, on account of the active inflammation to which they give rise.

" In the former case, that of a wound treated hitherto for adhesion, its edges become swollen, red, and painful. Swelling more deeply-seated causes the margins to separate, and purulent matter is soon poured forth. The indication here is to repress inflammation; in fulfilment of which, all sources of local irritation and general stimulation must be withdrawn. Sutures, if present, should be removed, and only a few strips of plaster left, to prevent any unnecessary gaping of the wound, and in many cases they also must be dispensed with. To the parts thus relieved from every kind of local irritation, warm water-dressings are applied, or a light, soft, moist, and warm poultice, if that application be still employed.

" In the second case, where granulation is from the first considered to be the most available mode of cure, the treatment is essentially the same as that mentioned above. The part is elevated, and kept at perfect rest; no sutures are employed, and only a few strips of plaster are used to connect the more loose portions of the wound. Cold water-dressing is applied till oozing of blood ceases; it is then gradually changed to the tepid, and next to the warm dressing, as the vascular action rises, so as to soothe and relax the tumefying wound. When the inflammation proves so active as to threaten gangrene, it must be repressed by local bleeding, and if absolutely necessary, by general depletion, in addition to the antiphlogistic regimen, which during this stage of acute inflammation is to be adopted.

" In both cases the same point is now reached. Warm dressings are continued so long as inflammation remains active; but as it subsides, the heat of the dressing is gradually lowered until it be again merely tepid, or even cool.

" Under this treatment, the surfaces, if matters go on favourably, become clean in a few days; granulations spring up, and healing advances. The discharge, which, during the height of the inflammation, had been very profuse and far from laudable, now diminishes in quantity and improves in quality. In these circumstances the wound, when superficial and broad, rather than narrow and deep, requires merely the treatment proper for an ordinary ulcer; comprising the water-dressing, medicated, when necessary, with metallic salts, to stimulate indolent granulations, and the employment of carefully adapted pressure by a bandage, when necessary to repress œdematous swelling.

" But if the wound be deep, without much loss of substance—in fact, such a case as would have healed by adhesion had not inflammation prevented—then, at this stage, when the surfaces are granulating well, and secreting little pus, they will, if placed in mutual contact, speedily cohere, affording a most satisfactory and rapid cure by secondary adhesion. Plasters are employed to retain the parts in apposition, and a bandage, lightly and uniformly applied, is in general necessary to give support. As absolute local rest is necessary for healing; any neighbouring joint which interferes with this essential condition in the wound, must be prevented from exercising its natural functions, by a splint fastened with a few turns of a roller, or with a buckle bandage applied at two or more points, lightly, so that no œdema may ensue on the distal aspect, and arranged so that neither splint nor bandage shall compress the injured parts, or come in the way of the requisite dressings. Cleanliness is throughout attended to; the actual edges of the wound are not touched, but from all around them the discharge is frequently wiped away

with a small dossil of clean lint, tow, or rag, or indeed of anything clean, soft, and absorbent. A good sponge answers well when there is only one wound to dress, because it can then be frequently washed; but in hospital practice it would come into contact with all kinds of sores, and would never be sufficiently well or often cleansed after each time of its employment. On the other hand, the morsel of tow, being of small value, may be destroyed, and a fresh piece employed on each occasion.

" The constitutional treatment—which, during the height of the inflammation comprised at least the antiphlogistic regimen, and sometimes also local or general bleeding, according to circumstances—consists now, while matters are going on favourably, in attention to the secretions, and avoiding, equally, undue stimulation or hurtful abstinence. In individuals of debilitated constitution, and even in persons previously of good health, when the wound is large, suppuration may continue profuse, cicatrization proceed very slowly, and the secondary adhesion fail. Under these circumstances, the diet must be full and nutritious, with a due allowance of stimuli. In severer cases this generous regimen must be farther assisted by the exhibition of medicinal tonics. Though the part itself must be kept at rest so that it may heal, yet it may be much benefited indirectly by appropriate general exercise, with the view of strengthening the system." (pp. 68—70.)

It must be obvious to our readers, that such an admirable sketch—for of course it pretends to be nothing more—of the indications of treatment, and of the mode of fulfilling them, could only have been written by a surgeon, not merely of experience, but of skill in discriminating and generalizing its results. And there is scarcely a topic handled by our author, which does not bear that stamp of personal familiarity with it, by which the production of the genuine practical writer is at once to be distinguished from that of the mere compiler and digester of the opinions of others.

Our next extract, which, though lengthy, is too good to be abridged, will be taken from the chapter on *Burns.*

" *Consequences of Burns.*—If at all severe or extensive, this kind of injury is liable to be followed by many serious consequences, which, though generally more or less combined in practice, may, for better description, be divided into two orders—namely, Local and General; or, into Inconveniences and Dangers; the former being hostile to the preservation of the comfort or limbs of the patient— the latter being dangerous to life itself. By a due knowledge and consideration of these, the treatment and prognosis must in every case be regulated. The first order, which consists of those *local* effects not directly dangerous to life, consists, with a single exception, of various conditions attending cicatrization, and productive of functional lesions, partial or complete. They have been enumerated under the heads of adhesions, deformities, and mutilations; to which may be added disfigurements, and affections of the cicatrix. The *disfigurements* consist of those unseemly cicatrices, especially on the face, neck, and other exposed parts, which merely affect the appearance rather than entail any serious discomfort. They are produced principally by burns of the third degree, by slight cases of the fourth, and sometimes also by severe instances of the second when the epidermis forming the vesicles has been torn off, exposing the cutis to the stimulus of the atmosphere, to irritation and subsequent suppuration. The *adhesions* imply those conditions in which, during cicatrization, contiguous tissues or surfaces, which in their natural state move freely on each other, have become mutually adherent, thereby abridging voluntary motion, as when a cicatrix adheres firmly to a muscle, tendon, or aponeurosis beneath; or these latter to one another. *Deformities* are constituted by any considerable alteration in the shape of an organ, or in the relation which one part naturally bears to another. They may be produced in two ways; either by contraction of the cicatrix, or by destruction of muscular antagonism. The cicatrix following a burn is said to have a greater tendency to

contract, than after any other species of injury. Like all new and lowly-organized structures, it is very liable to absorption, which makes the contraction and puckering of the tissues around go on long after the sore has healed. Wherever a portion of skin has been destroyed in this manner, as in a burn of the fourth degree, its place is eventually supplied, not altogether by a new and permanent structure, but to a very considerable extent by the uninjured integument in the neighbourhood, which, by the steady drag exercised on it by the gradual contraction of the cicatrizing ulcer, or the cicatrix, is drawn together towards a central part, which is at last occupied by the cicatrix, now much diminished in size, shrivelled, and sometimes almost of a horny texture. The surrounding integument stretches to a certain extent, more especially in those parts where it is loosely connected with the tissues below: but if the loss has been very extensive, the requisition on the integument around will be proportionately large, and this demand may prove more than its extensile qualities can supply. Accordingly, if a burn be so situated, that flexion or other posture of a neighbouring articulation will relax the skin around the seat of injury, the steady drag on the integuments, added to the natural tendency of the limbs to preserve a slightly flexed position, will produce, if not guarded against, a permanent flexure of the joint. The same remarks apply with still more force, when the deeper-seated parts have, as well as the skin, been destroyed. Thus the fore-arm has been immovably bent on the arm, the latter bound to the side, the lower jaw dragged down to the sternum, and the head drawn back between the shoulders. When the injury is situated on the extensor aspect of an extremity, the tendency, above mentioned, of the limbs to sustain a slightly bent position, is in general sufficient to counteract the extending force of the contracting cicatrix. This is not, however, always the case, for the fingers have frequently been bent backwards upon the metacarpus, and the foot has been so twisted and deformed that all trace of its original conformation has been destroyed. Deformities from this cause, and to this extent, are now, however, much less frequently met with than formerly; though, in injuries of such a nature, the motions of the joint almost always remain more stiff and constrained than natural, and are farther restricted by the abnormal adhesions formed between the cicatrix and the subjacent parts. Again, in those cases where the tissues beneath the integument are destroyed, as in a burn of the fifth degree, in which the continuity of muscles, tendons, or aponeuroses has been interrupted, the contractions of the cicatrix, together with the unnatural adhesions, frequently cause deformity, fixation, and even dislocation of a neighbouring joint. As in other injuries, when the solution of continuity affects a nerve, loss of voluntary motion or of sensation must ensue in the parts supplied by it on the peripheral side of the injury, by which occurrence the antagonism of two sets of muscles may be destroyed, and deformity produced. *Mutilations* consist in the partial or complete loss of an organ. They are immediate in all burns of the sixth degree, and in those of the fifth, in which the possibility of saving an useful limb is at once rendered hopeless. They are consecutive, when caused subsequently either by the violence of inflammatory re-action, inducing extensive gangrene, or where the limb, as it remains, is so utterly useless as to necessitate amputation; an operation which is also sometimes necessary to save life when it is endangered by hectic from the exhausting effects of profuse and prolonged suppuration. The *affections of the cicatrix* are chiefly excessive contraction, fissure, ulceration, and irritability. As it is less highly organized than the original integument, it is peculiarly liable to the first three of these conditions, in conformity to the general law, that newly-formed and lowly-organized structures are much more prone to absorption, to inflammation, and other diseases, than older and more highly constituted tissues. Irritability of the cicatrix may be the result of a nervous filament or trunk being implicated in it, as occasionally happens after an ordinary amputation; or it may occur without any such apparent cause.

" The second order of consequences, comprising the *general* or constitutional effects, are those which more immediately endanger life. They may be arranged,

chiefly according to the periods at which they occur, into six groups. First,—when a large extent of surface is burnt, though but superficially, and more particularly when to the third degree, a shock is communicated to the nervous system, either by the intense pain excited in the wide expanse of integumentary nervous web which is injured, or by the sudden destruction of the functions of the integument. It is believed that this shock occasionally causes instantaneous death by asthenia, or the asthenic form of syncope—the heart ceasing to act, from its irritability or contractile power being annihilated. The same result may take place when the burn, though of less extent, has penetrated more deeply and injured some vital or important organ; but, with this exception, it is a well-ascertained fact that burns are more dangerous from their extent than from their depth. Second,—more frequently it happens that death is *not* immediate. There is great depression and collapse of the vital powers, which gradually sink in a few hours. The immediate cause of the fatal issue in this instance may be, as in the former, asthenic syncope, with this difference, that here the functions of the heart are slowly and gradually suspended, instead of being instantly arrested. Females, children, and persons of a nervous and irritable temperament, are most liable to sink in this manner. In other cases, death is preceded by typhoid symptoms, low muttering delirium, and coma. When the functions of a large portion of integument are suddenly suspended, the healthy balance between them and those of the lungs is destroyed; the latter become, with the other internal organs, greatly congested, and soon cease to effect proper aëration of the blood. This leads to more retarded circulation in the pulmonary system; the brain is supplied with imperfectly purified blood; coma ensues, which still further retards the flow of blood through the lungs; and death results at last, from apnœa, accelerated by coma. Third,—In other cases, the collapse goes off, and is succeeded by an imperfect and feeble re-action, attended with great irritability and excitement of the nervous system, under which the patient may sink exhausted. Constitutional debility and irritability predispose to this termination. Occasionally, death has occurred about this period from tetanus, or from convulsions. Fourth,—On the disappearance of collapse, vigorous re-action may ensue. When this is confined within proper limits, it is the first step towards recovery; but when excessive, and accompanied by very high symptomatic or inflammatory fever, it is equally perilous to life, as would be its deficiency. Sometimes, accordingly, the patient dies during the stage of excessive re-action. At this period, also, congestion and inflammation are very apt to occur in the mucous membranes, and several internal organs, more especially the lungs, the intestines, and the brain. These serious complications render the prognosis much more unfavourable, and frequently prove the chief causes of death. The upper portion of the duodenum is the part of the intestinal canal which is most frequently affected. Sometimes the inflammation here leads to ulceration, especially in young persons; and, occasionally, during the ulceration, a small artery is opened, hæmorrhage ensues, and the patient generally dies, either from a single profuse loss of blood, or from a more sparing discharge, frequently repeated. Death, in this instance, takes place by that form of syncope in which the heart primarily ceases to act, from the want of a sufficient volume of blood to excite its contractions; the nervous system being consecutively affected by the deficiency of the nutrient fluid. This mode of death is accordingly called, by Dr. Watson, anæmia. Of hæmorrhage from the above cause, about a dozen cases are on record. Gangrene, from excessive inflammatory re-action, may prove fatal by a combination of asthenic syncope and coma. Any of the serous membranes, or the organs which they invest, may, in like manner, be attacked by inflammation. As a general rule, those internal parts are most apt to suffer, which are nearest to the external lesion. Apoplexy occasionally occurs from the fifth to the seventh day. Dupuytren considered this to be owing to idiosyncrasy, but it is more simply explained by referring it to vascular excitement in a person, the arteries of whose brain are already in a state of disease. Confirmed drunkards have been attacked, about this period, with delirium tremens; and in pregnant females, the

premature expulsion of the fœtus is said to have occurred. Fifth,—During, and after the detachment of sloughs, new dangers arise. In bad constitutions, or where the powers of life are much enfeebled, the separation of the eschar by ulcerative absorption may not have been preceded by a sufficient effusion of plastic lymph on the layer of living tissue next to the dead mass; accordingly, if any considerable artery, or even vein, has been involved in the slough, dangerous or fatal hæmorrhage may take place from its open mouth, which has not been sealed up, as, under a more favourable state of the system, it would have been. The same result may ensue from an artery being denuded at this period, and afterwards ulcerating. The possibility of such an occurrence suggests the propriety of using *no* force in removing the sloughs, lest the bloodvessels be not yet prepared for the separation. When the eschar has been very extensive, persons have occasionally died soon after its separation without any very obvious cause, unless it has been owing to the sudden exposure of a large ulcerating surface to the irritation of the atmosphere, inflicting a second shock on the system, which, though it was able to withstand the primary effect of the injury, succumbs to this second attack in its now enfeebled state. If this be the true explanation, then the raw surface, when of large extent, should be exposed only partially as seldom as possible, and for as short a time as practicable, at each dressing. During suppuration, phlebitis and pyæmia have sometimes occurred, and destroyed life with the most urgent typhoid symptoms. After all the preceding dangers are past, if the process of cicatrization, over a large surface, be tedious, and suppuration very profuse, the exhausting effects of this drain on the system, combined with long confinement, tend to induce hectic fever, under which the patient may sink. The fatal issue is sometimes much accelerated by the development and rapid progress of phthisis pulmonalis. A more common adjunct of the hectic, is colliquative diarrhœa, from irritation and ulceration of the intestinal mucous membrane, particularly in the vicinity of Peyer's glands on the lower part of the ileum. Sixth,—Even the period of cicatrization, according to Dupuytren, is not exempt from danger; for he mentions that when this process has been nearly or entirely completed, persons have sometimes died suddenly and in a manner unaccounted for, even on dissection. This singular occurrence may be supposed to be connected with the suppression of the purulent discharge, which, though not natural, yet from its long continuance before cicatrization was effected, had become a habit—and, in fact, necessary, in some degree, to the constitution." (pp. 96—101.)

Dr. Pirrie's chapter on Hernia is altogether extremely good; and as his experience has been by no means trifling, and as his attention has been specially directed to the question of opening the sac, we shall quote some of the passages which bear upon this.

" It is a question of great importance, in reference to the operation for strangulated hernia, which of the two following modes of proceeding in regard to the hernial sac is the more advisable; namely, that of opening the sac, and dividing the stricture from within; or that of dividing the stricture, and replacing the parts without opening the sac. Of these two, technically called the *intra-peritoneal* and *extra-peritoneal* modes of division, the former is that, which, except in a limited number of cases, has received the sanction and adoption of most surgical authorities in these islands. It appears certain that, in the great majority of cases, it is by that mode alone, that it is possible to accomplish the two grand indications which it is desirable to fulfil by the operation; namely, the removal of the pressure by division of the stricture, and the return of the hernia. The fulfilment of the former, namely, the removal of the pressure by division of the stricture, is essential to the safety of the patient; and that of the latter, the return of the hernia, exceedingly desirable when practicable and proper.

" With regard to the *first* indication, when the stricture is external to the sac, as is not unusual, it is possible to divide it by adopting either mode; but if formed by the sac, or within it, it is clear, that by intra-peritoneal division alone can the

more important indication be fulfilled, or any good effected. Cases belonging to the latter class are by no means of unfrequent occurrence. That the neck of the hernial sac occasionally constitutes the stricture, is a point regarding which surgeons are agreed, instances having been recorded by the great surgical authorities of this and other countries, and examples occurring frequently in the practice of many surgeons. The sac, necessarily narrower at its neck than in other parts, is liable to be still further diminished by effusion and organization of lymph, either on its outer or inner surface, as well as by a thickened and indurated state of its own substance,—conditions which, separately, or in various degrees of combination, diminish the canal of the sac. For eighteen years I have availed myself of every opportunity of examining the condition of hernial sacs, and from my dissections I am led to conclude, that in herniæ of considerable standing, thickening of the neck is of frequent occurrence. Although constriction, when sufficient to render a hernia irreducible, is usually at the neck of the sac, yet it is not invariably so. This fact is of little practical moment if a hernia be merely irreducible; but it becomes of the greatest importance if it be strangulated, and require an operation, as the paramount object of the operation is to divide the constriction, in order to relieve the symptoms of strangulation.

" The stricture is occasionally found within the sac. In a very few instances it has been found to be occasioned by a loop of intestine; in some by a band of omentum; and in others by a band of lymph effused from the serous coat of the intestine, and surrounding and constricting it as by a ligature.

" This last-mentioned condition has been described and delineated by Sir Astley Cooper. It has also been met with by other surgeons; and not fewer than four cases of it have come under my own observation." (pp. 587—589.)

Passing over the details of these cases, we find Mr. Pirrie thus continuing.

" From what is stated above, it appears very clear that the extra-peritoneal mode of herniotomy is quite unsuitable when the stricture is formed by the sac or within it, whatever be the nature of the stricture itself; and the above-mentioned examples of strictures formed by membranous bands, suggest very strongly the propriety of great caution in arriving at the determination of adopting the extra-peritoneal division; as well as, in those cases in which the sac is opened, of examining very carefully before the hernia be returned, whether membranous bands do or do not exist.

" The *second* indication which it is desirable to fulfil by an operation, is the return of the hernia.

" With a view to facilitate the inquiry, as to which of the two modes of procedure is the more suitable, cases may be arranged into the three following classes:

" First,—Those in which the stricture is external to the sac, in which it is neither impracticable nor improper to return the hernia—and in which no obstacle exists to that return after the stricture has been divided. Secondly,—Those in which an obstacle does exist after division of the stricture: and, Thirdly,—Those in which the return of the included intestine would be practicable, but improper.

" First,—In cases belonging to the first class, either mode is applicable; but extra-peritoneal division being attended with much less danger, is decidedly preferable.

" Secondly,—In regard to cases in which, independent of the stricture, an obstacle to reduction exists, it will be proper to consider what are the principal obstacles most frequently met with. These are adhesions of the protruded parts to the hernial sac, the natural means of connexion, in some rare cases; adhesions of the protruded parts to each other; and the large size of the hernia.

" For a description of these obstacles to reduction, the reader is referred to the section on irreducible hernia.

" If any of these conditions exist, and if the sac be not opened, reduction is in general impracticable. If the sac be opened, two of them may easily be over-

come; namely, the soft recent adhesions formed by coagulable lymph, and the fila-mentous,—the former can be broken down with the finger, the latter divided by the knife. Two of them present an insuperable impediment to reduction; namely, the natural means of connexion, and the close organized adhesions, if these be to a great extent, and the hernia large. With regard to the two remaining conditions, the possibility of overcoming them, and the propriety of attempting to do so, must depend entirely on the particular circumstances of the case; but frequently, it is more judicious not to interfere with them, unless they exist only to a limited extent, and in herniæ of moderate size. Most of these conditions, however, are principally met with in cases of large and old hernia; and, on account of the risk of injuring the intestine in attempts at reduction, as well as that of inducing dan-gerous inflammation by much handling of the intestine, and the difficulty of main-taining the parts reduced, even should reduction be possible, the majority of surgeons seem now disposed to follow the advice of Sir Astley Cooper regarding such cases. His practice was, to divide the stricture, which fortunately in such cases is, for the most part, external to the sac, and to leave the latter unopened, and the hernia unreduced. The stricture being divided, the principal cause of danger is removed. The coverings of the hernia should be replaced, and proper means taken for promoting the healing of the wound.

" Thirdly,—There are certain states in which it would be extremely improper to attempt reduction; namely, when the hernia is gangrenous, or when the intestine has given way from inflammation having gone on to gangrene, or when it has been torn, or accidentally wounded in the operation. The two last-mentioned conditions can only result from unskilfulness in the mode of procedure; but, should they exist, the hernia ought not to be returned. When the intestine presents such an appearance as to render it doubtful whether its return may be followed by fæcal extravasation, the surgeon should content himself with carefully dividing the stricture. In all cases in which the intestine is gangrenous, or not entire from whatever cause, it ought to be allowed to remain, so that the fæces passing off by the wound may form an abnormal anus, and extravasation into the abdomen be thereby prevented. When omentum forms the hernia, and it is gangrenous, the gangrenous portion may be removed, and the remaining part returned to the abdominal aspect of the mouth of the hernial sac. The practice of removing a portion of omentum, when from growth it renders a hernia irreducible after divi-sion of the stricture, is a proceeding which, in some cases, may be adopted with advantage. For cases belonging to this class, extra-peritoneal division is of course quite unsuitable. These remarks, it is to be hoped, will be sufficient to point out the proper mode of procedure when the hernia is sound, and reducible after divi-sion of the stricture; when it is irreducible after such division—and when it is in any of the various conditions in which reduction would be dangerous and im-proper; and also to show, that to follow one method indiscriminately in all cases would be unwise; that intra- or extra-peritoneal division should be adopted accord-ing to the particular circumstances of the case; that in the majority of cases intra-peritoneal division is not only the more suitable mode, but the only one which is safe, or by which any good can be effected; and that the cases in which extra-peritoneal division is suitable are those of very short standing, where there is no reason to apprehend the existence of adhesions, or of an unsound condition of the hernia; and those also of large and old hernia, where the more judicious proceeding is to divide the stricture, and not to attempt reduction." (pp. 591—594.)

Our last extract will convey Mr. Pirrie's judicious cautions on the use of the taxis, which correspond with those given by some of our ablest metro-politan surgeons, and are justified by the success of his own practice.

" There can be no doubt that intestinal inflammation is the most frequent cause of death after the operation for strangulated hernia. Some of the advocates of Petit's method have assigned as the causes of that inflammation, when the ordi-nary proceeding is adopted, the exposure of the intestine to light and air, change

of temperature and handling. I agree with Mr. Lawrence in ascribing it, not to these agents, but chiefly to the long-continued pressure of the stricture, owing to the operation being *too long delayed,* and to an injudicious and *too frequent use of the taxis* previous to the operation. I remember being very much struck with an observation of Desault's; I have not his works beside me at present, but it is to this effect :—' Think well of that hernia which has been little handled and soon operated on.' The operation is justifiable and necessary, when the patient has been brought fully under the influence of chloroform, and the taxis has been fairly, fully, and skilfully tried without producing the desired effect. The conviction being thus produced, that by no other means than an operation is there hope of saving the life of the patient, it ought to be resorted to as quickly as possible. Much handling must not only give unnecessary pain, but also increase the risk of hurrying on the inflammation to results which, even though the operation should be performed, would render it unsafe to return the hernia. When therefore the taxis has been fairly and skilfully tried, on a patient fully under the influence of chloroform, no advantage can, but considerable injury may, result from the repetition of treatment already found to be unavailing. Many considerations show that the operation should be performed as soon as possible, after its inevitable necessity has been found to exist. Delay, like undue handling, increases the risk of inducing such a state of the hernia, in consequence of inflammation, as would render its return unsafe. From the circumstance that a hernia may speedily prove fatal, and from the depressed state which comes on in consequence of delay, rendering the patient less able to stand the shock of an operation, will be seen the importance of being as prompt as possible ; but there is another and a very urgent reason—namely, that if the operation be delayed until intestinal inflammation has been induced within the abdomen, it is far from certain that this inflammation will subside on the removal of the hernia which caused it   I have performed the operation for strangulated hernia, according to the usual mode, a considerable number of times, I believe twenty-three in all, and (except in one case, where death occurred in consequence of an attack of phlegmonous erysipelas which commenced after the patient was considered out of all danger) in every instance with success. This success I attribute to two things—namely, avoiding all undue and useless handling, and performing the operation early. My decided impression is, that the reason why the operation is so frequently followed by death, instead of being one of the most successful of the great operations of surgery, is, too great delay in resorting to an operation, and the undue and injurious use of the taxis, even after its adoption has proved unavailing." (pp. 595, 596.)

We fear that our review may seem to have scarcely done Mr. Pirrie full justice; for though it is exceedingly easy to detect blemishes and shortcomings, it is extremely difficult to convey an adequate idea of the merits of an elementary work, where those merits consist in method, condensation, and perspicuity. By far the greater part of Mr. Pirrie's book possesses these in a very high degree ; and the author will find it easy, with a small additional expenditure of diligent care, to render the next edition of his work, which must, we feel sure, be required at no distant date, the best compendium of surgery in our language, if not the best existing treatise of its kind.

Art. XIV.

Researches and Observations on Scrofulous Disease of the external Lymphatic Glands. With cases, showing its Connexion with Pulmonary Consumption and other Diseases. By Thomas Balman, M.D., M.R.C.S., &c., one of the Medical Officers of St. Ann's Dispensary, Liverpool — *London,* 1852. 8vo, pp. 189.

" Very nearly three years ago," Dr. Balman observes, " I suggested to one of my colleagues, that each of the honorary surgeons to the Dispensary should be at liberty to select some particular group or class of diseases as his speciality, and that the cases belonging to such group occurring in the practice of the other surgeons should, as far as might be agreeable, be transferred to the care of the surgeon who had adopted this particular speciality. In this request all my colleagues readily acquiesced : hence the origin of this inquiry." Dr. Balman was thus enabled to note the particulars of 141 cases of scrofulous enlargement of the external cervical glands, which he did according to a fixed form; and the results are presented numerically and generally in the volume before us.

The first chapter is headed " Histology of Scrofulous Affections of the External Glands," and inquires into the temperament most commonly prevailing in the scrofulous constitution ; the ages of the patients ; the regions of the body most frequently affected; the probable causes ; the connexion between scrofula and phthisis ; and " the diseases occurring in the parents and relations, some of which have been supposed to be sometimes associated with the strumous habit." This chapter contains, therefore, the etiology and pathological relations of the disease ; consequently, we are puzzled as to what can have induced Dr. Balman to head it with the title " histology," unless he has thought that term and " history" synonymous. That he does not know the meaning of the word is obvious, however, or he would never have used it so mis-appropriately.—We will give a brief summary of the facts arrived at. There were 90 males and 51 females ; 34 per cent. of these had dark hair and complexion ; 73·76 per cent. were aged from 2 to 15 years, and only 2·84 per cent. above 30. But about one-tenth of the whole were obtained from different charitable institutions or schools ; and all those (comprising a greater number of adults than of children) who could not give the necessary information as regards their family history were excluded. This appears to us to have been a mistake on the part of Dr. Balman. The mere fact of occurrence at a certain age has no necessary connexion with the family history or with hereditary predisposition ;—we therefore think that he has restricted his numerical inquiries on the *general* points of age, sex, temperament, &c., very unnecessarily, by taking in the additional element of family predisposition. It is obvious, at a glance, how this circumstance wholly vitiates the general conclusions which might have been otherwise deducible from Dr. Balman's tables. As to the region affected in the 141, by far the greater proportion (83·69 per cent.) had disease of the *cervical* glands alone. The axillary glands were diseased (as well as the cervical) in 6·30 per cent., the inguinal in 4·20, and the glands above the elbow in 4·97 per cent. As to the causes ; in 55·45 per cent. they were not evident ; infantile fevers were

assigned in 23·40 per cent.; exposure to cold and damp in 14·18 per cent.
Dr. Balman " purposely omits many of the external causes which are well
known to act most injuriously in persons predisposed or otherwise to
scrofula—such as bad air, deficient or unwholesome articles of food, defec-
tive ventilation, or deprivation of exercise, because " they have already
been so ably discussed by Mr. Phillips, as to preclude the possibility of my
adding anything to what he has already recorded"—just as if the play of
' Hamlet' were to be played with the Prince left out, because the character
had been already so admirably represented by Kemble.

The hereditary connexion between scrofula and phthisis pulmonalis, is,
in fact, the chief object of Dr. Balman's inquiries.   This is shown in Table 5,
from which it appears that in 9 cases (of 141) the father had died of
phthisis, in 11 the mother; in 11 the paternal grandfather, in 17 the
paternal grandmother; in 9 the maternal grandfather, in 20 the maternal
grandmother; and in 99, uncles and aunts (on both sides) had died of
phthisis.   This table is interesting as far as it goes, but it is doubtful
whether it shows the full extent of hereditary predisposition derived from
the parents and collateral branches.   The large proportion of *young* cases
points to the probable circumstance, that the parents of *some* had still to
become the victims of phthisis.   Dr. Balman would have increased the
value of his table very considerably, if he had had columns showing the num-
bers of *cousins* affected with struma or dying of phthisis.   From inquiries
as to the liability to phthisis (or " consumption") of persons born of
phthisical parents, Dr. Balman found that in 20 instances in which the
grandfather died of phthisis, it was transmitted in 13; and in 37 instances
of the grandmother so dying, it was transmitted in 14.   This result cor-
roborates in some degree those arrived at from inquiries at the Brompton
Hospital.   As to consumption occurring in families as a companion-disease
to scrofula, Dr. Balman found of the 141 cases he investigated, that in 30
there were no ascertained family deaths from phthisis; in 60, in one branch
only; 40, in two branches; 9, in three; 1, in four; and 1, in five branches.
The general infirmity of constitution renders the scrofulous liable to other
diseases of a grave character.   Thus in 27 there were 18 deaths of father
or grandfather from apoplexy, paralysis, or epilepsy; out of 27 on both
sides, 6 grandfathers and 1 grandmother died of insanity, 5 of the latter of
cancer, 3 of the former of stone; in all instances a much larger ratio than
occurs in the general population.

We pass from Dr. Balman's not very satisfactory statistics, to his more sa-
tisfactory pathology.   On this head, his views are those of Phillips, Glover,
&c.   He has submitted *blood* taken from strumous persons to microscopic
examination, and the only deviation from the healthy condition which he
has been able to make out, is an unusual increase in the number of the
normal colourless corpuscles.   He has " very frequently counted as many
as sixty and eighty in the field with a fourth-of-an-inch object-glass,
whilst ordinarily only five or six are observable.   They appeared very
variable in size, some being less than half the size of the coloured
corpuscles, whilst the majority exceeded the blood-discs in diameter."
He has found the *urine* of the strumous to average about, sp. gr., 1·012,
and " it had often a strong odour of cod-liver oil in cases where this medi-
cine had been taken for any length of time."   In 19 (of the 32 cases in

which the urine was examined), he found octohedral crystals of the oxalate
of lime, the average specific gravity being 1·020. Of the 13 remaining
cases, 4 showed the phosphates in excess; 2 of these had very large
glandular swellings excited by syphilis; in the other 2 these glands had
suppurated. With the laudable view of testing the value of the oxalate-
urine as a pathological indication, Dr. Balman examined the urine in
various other diseases. In 117 instances of this kind, the oxalates were
present in 16, unmixed with any other deposit, in 26 mixed with lithates,
in 3 with phosphates: 21 cases of phthisis gave 10 with oxalates; 12
" affections of the skin" gave 8, of these 4 were cases of *impetigo*. Dr. Bal-
man did not find any unusual frequency of the oxalates in dyspeptic cases.
From these researches it would appear that the oxalate of lime occurs more
frequently in the urine of the strumous, than of those affected with other
diseases. Dr. Balman makes the proportion to be 74 per cent. to 38 per
cent.; but it is manifest that if phthisis and impetigo be deducted from the
instances of general disease and added to the strumous, we should have a
somewhat different ratio—namely, 64 to 32 per cent. Dr. Balman found
the oxalate deposit showing itself in strumous cases, " for weeks or more,
commonly for months, uninfluenced by diet or medicines of any kind."
This he ascertained by careful and repeated experiment and observation.
He also found the oxalates more abundant in the urine passed at night,
than in that of the morning. As the health became more and more
deteriorated, and the powers of the system reduced, the earthy phosphates
appeared, the oxalate of lime being then seldom met with. On the other
hand, as the health became re-established, the urine ceased to exhibit traces
of the oxalates.

Dr. Balman devotes his third chapter to the consideration of the
" Symptoms, Progress, Duration, and Varieties of Scrofulous Tumours."
His familiarity with the disease is well manifested by the truthfulness of
his delineations. We make some practical *excerpta*.

" One peculiarity in these tumours, which I have repeatedly noticed, is the almost
sudden variations in size which they appear sometimes to undergo. Thus, after excite-
ment or exercise of any kind, as running or walking fast, they sometimes become dis-
tended to twice or thrice their usual size. The same thing, in a less degree, is observ-
able in the morning on getting out of bed (arising, probably, from some temporary
impediment to the return of venous blood from the head and face), which gradually
subsides during the forenoon. One may sometimes notice, too, another feature of
interest—that is, the reciprocal influence between it and the healthy and regular
performance of the digestive and blood-making process, on the one hand, and the
partial arrest or temporary derangement of these functions on the other. Thus,
suspend, vitiate, or impair by neglect of hygienic means, unwholesome or insufficient
food, the process of digestion, and the disease either increases or remains stationary ;
remove the person from an unhealthy locality, and supply him with good and
nutritious food, and the swelling will in all probability diminish, and the general
health correspondingly improve." (p. 97.)

It has been remarked by various practical writers, that an external
deposit of tubercular or scrofulous matter is apparently prophylactic, in its
influence on the system, against the more serious deposit internally in the
lungs or other viscera. In a clinical lecture, published a few years ago
(1846), Dr. Laycock mentions this general fact as probable, from various
circumstances. Referring to a case under the notice of his class, he

observes:—"She is thin, meagre, and has enlarged glands in the axillæ and neck; and it is well for her that they are there, for I am inclined to think they will be her defence against the tubercular deposit in the lungs; at all events they will much delay the fatal termination. I have witnessed three or four cases of tubercular phthisis of this kind with the peculiar complexion—(a muddy grey)—the enlarged scrofulous glands in the neck and axillæ, and tubercular deposit in the lungs going on, but very slowly indeed, the patient lingering on from day to day in really a wonderful manner. I see that Dr. Glover, of Newcastle, in his recently published work on Scrofula—a work containing the result of original investigations,—states it to be a popular belief that scrofulous cervical glands prevent the internal disease becoming manifest; and he quotes an observation by Sydenham, somewhat like that I have just made. I certainly cannot say I have seen phthisical persons *cured* concurrently with these scrofulous glands; but I am sure their presence in good large masses (and if suppurating, so much the better) is the guarantee for a prolonged existence to your patient. I have such an example under my care at this moment. How this happens can only be theorized on, but it is not unreasonable to suppose that there is a derivation from the lungs to the glands; that what tubercular matter is deposited in the glands would have been deposited in the lungs had there not been glandular irritation to attract it there. With the humoral pathology the old terms of that pathology will come into use, and we may say, with regard to scrofulous diseases and deposits, *ubi irritatio ibi fluxus.* The old methods of treatment adopted by the old humoral pathologists will come again into vogue; and I cannot doubt that in the gouty and scrofulous cachexies, *derivants,* such as issues, setons, and perpetual blisters, may be, and are, of advantage."*

We have given this quotation at length, because it very accurately expresses the results of Dr. Balman's experience; active scrofulous tumours in the neck or elsewhere, serving, in his opinion, as a safety-valve to the lungs. He details one or two cases illustrative of the prophylactic efficacy of the external deposit, in cases of tubercular phthisis, and has found a seton below one of the clavicles attended with beneficial results, as regards both the local and general symptoms. Dr. Balman has a theory explanatory of the fact, which is based upon the probable functions of the lymphatic glands. Referring to the resemblance, on several points, between the latter and the thymus, he observes:

"If their function be, as is now very commonly entertained by physiologists, to elaborate and prepare nutritious matter to meet the additional requirements of the system during the active period of growth, when nature is employed in building up the structures, appears in my mind [*sic in orig.*] to go some way in explaining the reason why these glands should be more liable to be affected with strumous disease previous to the age of puberty and manhood, than at any subsequent period of life. The growth of the body being now perfected, the functions of these organs would be simply limited to the maintenance and support of the animal body; and those morbid and heterogeneous products which would otherwise be attracted to the surface in growth, have now a greater tendency to fix themselves in some internal organ, especially the lungs, causing pulmonary consumption. This view is, I conceive, strengthened by some of the remarkable phenomena before alluded to, as regards a certain amount of antagonism between two diseases: for example, external glandular

* Dr. Laycock's Clinical Lecture on Scrofula, London Medical Gazette, Nov. 1846.

swellings, either whilst gradually increasing in size, or in a state of suppuration, seem to protect the lungs from being invaded with tubercles; and, on the other hand, the presence of tubercles in the lungs is seldom followed by the deposition of a similar product in the external glands." (pp. 109, 110.)

Dr. Balman illustrates the causal relation between syphilis and scrofula by detailed cases, and then points out the influence of other circumstances on the progress of the disease. We do not here find, however, anything worthy of special notice; and we therefore pass on to Dr. Balman's chapter on the *treatment* of strumous glandular swellings, where we again find nothing novel. The tumours in the early stage should be "dispersed," if possible; and to this end, Dr. Balman recommends "pencilling the part lightly with the solid nitrate of silver a few times, at intervals of a week or ten days." When suppuration has taken place, an early incision is recommended, &c. We think Dr. Balman would find constant endermic medication of the tumours useful in an early stage, combined with constitutional treatment. We apply a lotion of the iodide of potassium to the tumours, taking care to keep the pledget well covered with a piece of gutta-percha sheeting or oiled-silk, so as to retain the moisture.

Dr. Balman adds what he terms an "Appendix," which is simply the republication of some cases of cystirrhœa, &c., from the 'Medical Gazette.'

As a whole we cannot express any decided approval of this work, and we are inclined to think, that as the greater portion had been so very recently published in the 'London Medical Gazette,' there the few new facts Dr. Balman has brought forward might very properly have found a place. We have no doubt of the zeal and industry of the author, but we think it is obvious that the execution falls far short of the plan laid down, and that the results will hardly bear examination after the very recent works of Mr. Phillips and Dr. Glover, to which we called attention in late numbers of one of our predecessors.*

Art. XV.

Leucocythemia, or White Cell-Blood, in relation to the Physiology and Pathology of the Lymphatic Glandular System. By John Hughes Bennett, M.D., F.R.S.E., Professor of the Institutes of Medicine and of Clinical Medicine in the University of Edinburgh, &c. &c. With two coloured lithographs, and numerous woodcuts.—*Edinburgh*, 1852. Royal 8vo, pp. 131.

Those of our readers who are also readers of the 'Edinburgh Monthly Journal,' will have noticed that, for some time past, Professor Bennett has been drawing attention to a remarkable class of cases, in which a peculiar alteration of the blood, essentially consisting in an extraordinary multiplication of its white or colourless corpuscles, has been found associated with a morbid condition of the spleen or of other glandular bodies belonging to the lymphatic system. The work before us chiefly consists of the papers which have already appeared in successive numbers of our contemporary, but it is not on that account less worthy of a full and commendatory notice; for it contains a series of facts of an order altogether

new, collected with that zeal and industry which Professor Bennett has
already displayed so conspicuously in other departments of pathological
investigation; and digested with a care and discrimination which contrast
most favourably with the crude and hasty speculations, wherein we too
often find the observers of what they deem novelties indulging.

The mode of publication which Professor Bennett has adopted, is one
which, in a matter of this kind, seems to us peculiarly advantageous. Had
he kept his first discovery to himself, and simply waited for fresh cases of
the same kind, he would have been very long in accumulating such a series
as could enable him to appreciate the real pathological value of the phe-
nomenon,—that is, to decide whether it had an essential or a merely acci-
dental connexion with the coexistent organic lesions. But by making
known his observations as soon as they had become sufficiently numerous
to afford a clue to further investigation, he has enlisted a large number of
intelligent *aides* in his service, and has so largely profited by the materials
they have contributed, as to have now collected no fewer than twenty-
seven certain, and at least six probable cases, of a remarkable alteration in
the structural or histological character of the blood, whose existence has
only been known within six or seven years past. And the collection and
publication of this body of evidence, with his own deductions from it, in a
complete and systematic form, now enables the profession at large to judge of
the value of the new pathological facts, and of the physiological doctrines to
which they are considered by Professor Bennett to lead.—Our task will in
the first instance be the very simple one, of placing before our readers a
summary of the facts which the observations of Professor Bennett and his
coadjutors have brought to light; and this task is rendered easy by the
mode in which the materials have been already digested by the author,
whose own language we shall in great part employ.

In the *Introduction*, Professor Bennett gives the following account of
the history of the discovery; from which it will appear that in the two first-
observed cases, the colourless corpuscles, which had undergone unusual
multiplication, were mistaken for pus-corpuscles:

"On the 19th of March, 1845, I examined the body of a man, who died under
the care of Professor Christison, in the Royal Infirmary, labouring under hyper-
trophy of the spleen and liver, and whose blood was crowded with corpuscles which
exactly resembled those of pus. The case was published in the 'Edinburgh
Medical and Surgical Journal,' for October, 1845, under the title of 'Case of
Hypertrophy of the Spleen and Liver, in which Death took place from Suppuration
of the Blood.' Dr. Craigie, who was present at the dissection, recognised its
similarity to one he had had under his care four years previously, the blood of which
had been examined microscopically by Dr. John Reid, who found 'that it con-
tained globules of purulent matter and lymph.' It was from the January number
of Schmidt's 'Jahrbücher,' for 1848, I first learned that Virchow had dissected a
body in the Charité Hospital of Berlin, on the 1st of August, 1845, in which the
blood examined microscopically presented the same increase of colourless cor-
puscles, also associated with enlargement of the spleen. This case was originally
published in the 780th number of Froriep's 'Notizen,' under the name of white
blood, or 'Leukhemia.' On the 31st of December, 1845, a man was received into
St. George's Hospital, London, in whom Dr. Fuller detected, both before and after
death, the increased number of colourless corpuscles in the blood. This man, like
the other individuals, had great hypertrophy of the spleen. A notice of the case is
inserted in the 'Lancet,' for July, 1846. Since then, several similar cases have been

met with, in which this morbid condition of the blood has been determined to exist, by an accurate examination with the microscope ; and a reference to the records of medicine has shown the previous occurrence of like cases.   In these last, it is true, the blood was not physically proved to contain an unusual number of colourless corpuscles, although now, on looking back upon the facts which are mentioned in regard to them, we can have little doubt that such was the case.

"The term 'Leukhemia,' or white blood, given to this disease by Virchow, is objectionable, because, in the first place, as is correctly stated by Dr. Parkes, the blood is not white, but presents its usual red tinge when drawn from the arm. The colourless clots occasionally observed, will certainly not warrant the application of this term to the blood generally, as they are frequently present without the morbid condition under consideration.   Besides, the same name (white blood) has been given with more propriety to the fatty blood, examined by Drs. Traill, Christison, and others, which presents a milky, opalescent appearance.   What is required to be expressed is, that the blood abounds in colourless corpuscles, and this is, I think, done by the term Leucocythemia—from λευκὸς, white ; κυτος, cell ; and αἷμα, the blood—literally, white cell-blood, which expresses the simple fact, or a pathological state, and involves no theory." (pp. 5, 6.)

The *First Part* contains a history of all the cases of leucocythemia, of which Professor Bennett has been able to obtain any account.   These cases are arranged under the following heads :

1. Cases which have fallen under Professor Bennett's own observation, or have been communicated to him ; these amount (with the cases stated in the Appendices to Parts I. and II.) to 19 in number.

2. Cases which have been published by various authors, 8 in number.

3. Probable cases, in which, though the blood was not examined microscopically, it still presented such appearances as to warrant a belief that leucocythemia existed ; of these we find abstracts of 6, and others referred to.

To these are added 4 cases of enlargement of the spleen, in which leucocythemia was proved *not* to exist, for the purpose of showing that such enlargement may take place without necessarily occasioning in the blood the peculiar condition in question.

As we do not think that it will serve any good purpose to give the details of any of the cases described in this Part, since the chief symptoms presented by them will be presently brought under systematic review, we shall pass on at once to the Second Part of the work, which contains what mathematicians would term the "discussion" of the observations collected in the first.   This opens with the following passage, which will serve to impress our readers with the importance of the inquiry which it introduces.

"These cases serve to establish the occasional existence of a morbid condition, eminently calculated to arrest the attention of the physiologist and pathologist. That the blood may be loaded with a multitude of cells, exactly resembling those of pus ; that such blood may circulate in the human subject for months, or even years, without destruction to life, and that this condition is always associated with disease in those organs, the functions of which have hitherto been involved in the greatest obscurity, constitute facts which seem calculated to exercise an important influence on many views that have been long agitated in science.   The constitution of the blood itself ; the origin of its morphological elements and chemical proximate principles ; the importance of the lymphatic system ; the functions of the spleen and other blood-glands ; the nature of purulent infection, and other diseases of the blood, may be expected to be more or less elucidated by a study of the accompanying phenomena, causes, and results, of leucocythemia." (p. 83.)

The first section of this Part relates to the *symptomatology* of leucocythemia. On this point, however, it must be confessed that the information afforded by Dr. Bennett's collection of cases is far from satisfactory; since, although the symptoms noticed in the subjects of them were severe enough, they were for the most part, if not entirely, those occasioned by the local diseases of the liver or spleen, of which the affection of the blood might itself probably be regarded as one of the consequences. From Dr. Bennett's analysis of the *age* of the patients, in the undoubted cases, he infers that the disease is *most* common in adult life, but more frequent in advanced age than in youth. No allowance is made in this statement, however, for the diminution in the whole number of individuals living at advanced ages; and as we find that the numbers of cases between 30 and 40, between 40 and 50, between 50 and 60, and between 60 and 70, were respectively 7, 4, 3, 3, whilst the numbers of persons alive at these periods respectively approximate pretty closely in the respective ratios of 13, 10, 7, 5, the disease, on Dr. Bennett's own showing, is at least *as* common in advanced age as in middle life, and indeed rather more so. This point is very frequently left out of view in the estimation of relation of diseases to age. Of course, the contrast between aged and young subjects in the frequency of the disease is rendered much stronger by this mode of comparison. Of the 27 undoubted cases recorded by Dr. Bennett, 18 occurred in males, and 9 in females; so that, as far as this collection enables us to judge, the male sex is twice as liable to it as the female.—The only symptoms which can be at all referred to the state of the blood, are a disposition to sanguineous or serous effusion. In no fewer than 15 cases there was hæmorrhage of some kind—namely, epistaxis alone in 7, hæmatemesis in 1, hæmorrhage by stool in 4, hæmoptysis in 1, flooding after delivery in 1, and bleeding from spongy gums in 1; epistaxis being often combined with the bleeding from the gums or bowels in the latter cases. Dropsical effusions were present in 13 cases; but these were, of course, partially attributable to the mechanical pressure of the abdominal tumours. From the frequency of splenic enlargement, it might be supposed that the disease was connected with intermittent fever; but that this had been ever the case seemed very doubtful; for in three cases only could the previous occurrence of ague be made out, and in two of these it had existed at very remote periods. An unusual pallor of the surface, resembling that of anæmia, was observed in several of the cases; the conjunctivæ, also, were of a peculiar light blue tint.

The second section is headed *Condition of the Blood in cases of Leucocythemia;* and the third treats of the *Chemical Composition of the Blood in cases of Leucocythemia* We should have thought that the "chemical composition" of the blood is one element of its "condition," and that Section III. should consequently have been a subordinate division of Section II.; but we presume that by "condition," Professor Bennett meant only the histological state of the blood, as the excessive proportion of the colourless corpuscles is the only point adverted to. Out of the 27 cases of undoubted leucocythemia, the existence of this condition was determined in 8 during life only, in 10 after death only, and in 9 both during life and after death; thus it has been detected in the living body in 17 cases, and in the dead body in 19 cases. The following is Professor Bennett's account of the distinctive appearances which the blood presents:

"On examining the blood of living persons (which is most readily accomplished by extracting a drop from the finger by pricking it with a needle, and then examining it between glasses under the microscope in the usual way) the yellow and colourless corpuscles are at first seen rolling confusedly together, and the excess in number of the latter is at once perceived. This, however, becomes more evident after a short time, when the coloured bodies are aggregated together in rolls, and leave clear spaces between them, which are more or less crowded with the colourless ones. Means are altogether wanting to enable us to determine with exactitude the relative proportion of the two kinds of corpuscles in different cases. In some the colourless corpuscles are only slightly increased beyond their usual number. In one case they are described as five times as numerous as those in health. They are also said, in particular instances, to be 'greatly increased,' 'one third as numerous,' and 'as numerous' as the coloured corpuscles. In all these statements there is nothing exact. Perhaps the best method of judging, is to regard the spaces or meshes left between the rolls or aggregations of yellow blood-corpuscles. When these are completely filled up, the colourless bodies do not, in fact, amount to one-third of the coloured ones, on account of the large number of the latter which may exist in a small space, in the form of rouleaus.

"The size of the colourless corpuscles in the various cases given, differs considerably. Even when at first sight they appear to be of tolerably uniform size in any one case, it may be observed, when they are magnified highly and carefully measured, that some are twice the size of others, with all the intervening sizes between them. In some cases, though comparatively few in number, they are described as being three or four times larger than the coloured corpuscles, and in two cases they were in one about the same size, or somewhat smaller, and in the other of two sizes, one larger and the other decidedly smaller.

"In the nineteen cases in which the blood was carefully examined after death, the same variations with regard to number and size of the colourless corpuscles were found to exist, as have just been referred to in blood drawn fresh from the finger. It was always observable, however, that they were most numerous in the clot; and when they existed in any number, they communicated to the colourless coagulum a peculiar dull, whitish look, and rendered it more friable under pressure. When less numerous, portions of the colourless coagulum from the heart and large vessels might be seen to present a dull cream colour, easily distinguishable from the gelatinous and fibrous appearance of a healthy clot, and such altered portions always contained a large number of the colourless bodies." (pp. 87—89.)

Prof. Bennett goes on to remark upon the fact, that in no instance had the commencement of this change in the blood been observed; nor had its condition been ever seen to undergo any marked change, after the excess of white cells in it was discovered. This blank, however, was subsequently supplied by one of the cases recorded in the appendix, in which a patient having been admitted in September, 1851, suffering under chronic enlargement of the spleen and liver, no change in the normal proportion of red and colourless corpuscles could be at first perceived ; but at the beginning of January, 1852, "a decided increase of the colourless corpuscles was observed;" and this augmentation continued, so that, "during the whole of February, considerable groups of these bodies could be seen between the rolls of colourless discs in a demonstration under the microscope;" the patient, however, has been since lost sight of.—The chemical analysis of leucocythemic blood has hitherto been performed in only eleven cases ; a number far too small to afford any satisfactory results. The facts most generally observed, however, were, a marked diminution in the corpuscular element of the blood, with an increase of the fibrin, the solids of the serum presenting but little change ; so that the total amount of the solid matter

was usually considerably under the normal proportion. We are not told, however, save in general terms, in what manner the analysis was conducted; or what special means were adopted to separate the fibrin from the colourless corpuscles, which are well known to adhere to it so obstinately as in great part to be removed by the ordinary process of defibrinization. Hence all these statements with regard to the augmentation of the fibrin in the blood of leucocythemic subjects, must be received with some hesitation. No relation could be detected between the (supposed) excess of fibrin and the diminution of corpuscles; thus in one case the fibrin amounted to 6, and the corpuscles to 67·5; whilst in another the fibrin was 7, and the corpuscles 101·6. In two cases in which the inorganic constituents of the blood were analysed, the proportion of iron was found to be extremely low, as might be expected from the diminution in the proportion of red corpuscles.

The fourth section contains a summary of the information which Professor Bennett has collected, respecting the *Morbid Anatomy of Individuals* affected with leucocythemia; and this is of great pathological value. Of the 27 undoubted cases of this diseased condition, which have been recorded, the body has been examined after death in 19; and 13 other cases are referred to, in which valuable information was supplied by *post-mortem* inspection, although the existence of the disease, for want of the requisite examination, had not been determined. The organs which have been most uniformly found to be diseased are, the spleen, the liver, and the lymphatic glands; other lesions found in the brain, lungs, heart, kidneys, &c., were evidently consecutive or accidental. Of the condition of the *Spleen*, Professor Bennett tells us:

" Of the nineteen cases of leucocythemia in which the body was examined after death, the spleen was found to be more or less enlarged in sixteen. In the other three, although it was healthy, the pulp in one is said to be 'a little more compact than usual;' in a second its condition after death is not alluded to, although an encephaloid tumour occupied the left side of the abdomen; and in a third, the spleen was 'healthy.'

" Of the sixteen cases in which the spleen was increased in volume, it weighed above 9 lbs. in three; above 5 lbs. in two; above 3 lbs. in one; above 2 lbs. in four; and nearly 1 lb. in one case. In four cases it was not weighed. The greatest weight of a spleen was 7 lbs. 13 oz., and the largest measurement 16½ inches long, and 9½ inches broad. The texture of the organ varied in different cases, in some being of unusual density, in others natural, and in a third class more or less soft and pulpy. In a few cases it contained yellowish masses, apparently a form of deposit, but in reality degenerated tissue. The structure was examined microscopically in seven cases, in all of which it was demonstrated that the cell and nuclear elements were increased, while the fibrous portion of the organ was apparently normal.

" In four cases in which the existence of leucocythemia is probable, changes similar to those just stated occurred in the spleen, and in Dr. Hodgkin's cases similar lesions were found associated with enlarged lymphatic glands.

" It is clear, however, that mere enlargement of the spleen is not necessarily connected with white cell-blood, for in case 31 it was simply hypertrophied, and weighed three pounds and a half; and in numerous other cases, where this organ has been undoubtedly enlarged, it has been proved by careful examination, that the blood was normal. From the observations I have made on the structural differences in the spleen under these two circumstances, it appears to me that when enlarged in leucocythemia, its corpuscular elements are proportionally increased in number.

When enlarged in other cases, it is apparently owing to simple hypertrophy, increase of the fibrous element, or congestion of blood. All these various alterations may be mingled more or less together, in different ways and degrees." (pp. 92, 93.)

The condition of the *Liver* appears to have been less constantly altered; for disease was only observed to exist in it in 13 cases out of 19. In 10 out of these 13 cases, the organ was greatly enlarged; but it is obvious that the morbid condition which caused the enlargement must have been far from uniform, since we are told that "its consistence varied from great firmness to a degree of softening almost amounting to diffluence." Of the other three cases, cirrhosis existed in two, and cancer in one. In the six probable cases of leucocythemia, the liver is stated to have been greatly hypertrophied in four.

The following, according to Professor Bennett, was the condition of the *Lymphatic Glands*:

"Of the nineteen cases examined after death, the lymphatic glands were more or less diseased in eleven. Indeed, it is very probable that they were affected in a larger number, as in most of the other cases they were in no way alluded to, and may possibly have escaped observation from an unacquaintance with the importance which, as we shall see, ought to be attached to them.

"Of the eleven cases, the lymphatic glands throughout the body were greatly enlarged in four, and more or less cancerous in three others. The mesenteric glands were especially affected in two; the thyroid and epigastric glands in one; and the solitary and aggregate intestinal glands in one. In some cases they were soft, presenting on section a granular whitish appearance, and yielding a copious turbid juice on pressure. In other cases they were more indurated; and in one there were slight calcareous deposits. The glandular structure was carefully examined microscopically in eight cases, and in all exhibited increase of the normal tissue, the juice abounding in cell or nuclear elements. In two cases, cancer-cells were mingled with the healthy textures of the glands.

"In the 17th volume of the 'Medico-Chirurgical Transactions,' Dr. Hodgkin has recorded seven cases in which the lymphatic glands were more or less enlarged, and at the same time associated with increased size of the spleen. He considers the enlargement of both structures to be allied, and to depend upon a primary lesion unconnected with inflammation or adventitious structures. The appearance of a bloody serum in the thoracic duct and absorbents struck him in two of these cases, but the blood itself was not apparently noticed. At the time Dr. Hodgkin wrote (1832), the microscope was not much employed in pathological investigation; but had the blood been examined in these cases, I cannot resist the conviction that the discovery of leucocythemia would not have been reserved for the year 1845." (pp. 94, 95.)

It is obvious that, in the present state of our physiological and pathological knowledge (or rather of our ignorance), the chief interest of these phenomena lies in the causative relation which they indicate, between certain diseased states of the spleen, liver, and lymphatic glands, and the increase in the relative proportion of the colourless to the red corpuscles of the blood; this increase arising, not only from the positive augmentation of the former, but also from a positive diminution in the latter. And if such a causative relation be admitted to exist, it is obvious that a like dependence may be probably inferred, between the continual production of new blood-corpuscles, and the normal functional activity of the organs in question. As an introduction to this inquiry, Professor Bennett devotes his fifth section to an examination of the *Relation existing between the Colourless*

and Coloured Corpuscles of the Blood; his object, however, being, not to enter into a general discussion of the question as treated by others, but to state the views to which he has been led by his own observations. Our readers are probably aware that the current of physiological opinion has strongly set, of late years, towards the doctrine that the red corpuscles are formed from the colourless; some observers (as Mr. Paget) maintaining that the latter bodies are directly transformed into the former; whilst others (as Mr. Wharton Jones) affirm, that whilst this is the case in the oviparous vertebrata, the red blood-disc of Man, as of Mammalia generally, is the liberated *nucleus* of the colourless, which assumes more or less of the character of a cell. The latter of these opinions is adopted by Professor Bennett, chiefly on the strength of the observations which he has made on the corpuscles in cases of leucocythemia; but we do not see that the greater part of these have any direct bearing on the question. He has frequently observed the nuclei of the colourless corpuscles in the progressive stages of subdivision into twos and fours; but this obviously establishes no relation between these bodies and the red corpuscles. In one case he met with two orders of colourless particles in the blood, the smaller having all the appearance of free nuclei, such as could be observed within the latter; but no colouration was noted in the small free particles. The only observation that seems to us directly to bear on the question, was the following :

"On several occasions the blood, when crowded with colourless corpuscles, was removed from the arm by venesection; and it was observed, that after standing twenty-four hours these variously-shaped nuclei had become of a straw colour, and exactly resembled the coloured discs in tint. It was immediately apparent that they had imbibed the colouring matter of the blood, leaving the cell which surrounded them perfectly transparent." (pp. 96, 97.)

One such observation, in a morbid condition of the blood, is of course insufficient to establish the *factum probandum;* and we do not see that much is added to the evidence by the following experiment :

"A rabbit was killed, three hours after having eaten a meal. The thorax was rapidly opened, and a ligature placed round the pulmonary artery, to prevent the corpuscles coming from the thoracic duct passing into the lungs. The abdomen was then pressed gently for a few moments, to favour the flow of chyle, and then a ligature placed round the large vessels, and the heart removed by cutting above it. On examining the blood in the right ventricle, it presented an unusually large number of colourless cells, the nuclei of which, on the addition of acetic acid, exhibited all the transition stages figured. On examining the blood in the left ventricle, the colourless cells were normal in amount." (p. 97.)

The question of the *mode of conversion* of the colourless cells into the red blood-discs, is, however, of comparatively little moment, if the fact of such conversion be admitted; which, we apprehend, may now be regarded as all but conclusively established, since, among the oviparous vertebrata in particular, transition-stages may be very frequently detected. It is quite another question, however, whether *all* the colourless corpuscles normally go on to be developed into the red. For ourselves, we believe, with Kölliker, that there are colourless corpuscles in the blood, which never pass into the condition of the red; and that while both may have a common origin in the lymph-globule, the one mode of development is inconsistent with the other. And the phenomena of leucocythemia seem to us to support this view; *a diminution in the red corpuscles* being coincident with *the increase in the colourless.*

In the sixth section, Prof. Bennett points out the bearing of his obser-
vations on the question of the *Origin of the Blood-corpuscles*; as to which he
maintains that the doctrine of Hewson, of the production of the corpuscles
in the lymphatic and vascular glands, is really the correct one, although it
has been rejected by the greater number of succeeding physiologists.   We
think that he goes too far, however, in affirming that the doctrine of Hewson,
" though supported to a greater or less extent by some German authors,
has been repudiated by all British physiologists up to this time;" for
whatever may have been held with regard to the spleen, thyroid and thymus
glands, and supra-renal capsules, the share of the Absorbent system in
generating the blood-corpuscles has been more generally recognised than
Professor Bennett seems to think.   For that the *colourless* corpuscles of the
blood are identical with the corpuscles which float in the chyle and lymph,
and which are most abundantly found in the fluid drawn from the glandulæ
of the absorbent system, is a doctrine which has latterly come into pretty
general acceptation; and the origin of the *red* corpuscles is of course
traceable back to the same source, if their derivation from the colourless
be admitted.   Nevertheless, we fully recognise the value of the additional
evidence which cases of leucocythemia afford, of the direct passage of the
parenchymatous cells of the " vascular glands" into the sanguiferous system;
and we believe that a very strong case has been made out in favour of
this view, which applies to the spleen in common with the thymus, thyroid,
and supra-renal bodies, and also (according to Œsterlen) to the pituitary
and pineal glands.   The essential similarity which prevails among these
bodies, both as to their structure and development, has now been clearly
made out; the best observations on the second of these heads being those
of Mr. H. Gray, of which we gave an account in our last number (p. 541).
They all consist, as Professor Bennett well remarks, " of a fibrous stroma,
enclosing spaces lined by a structureless membrane, which spaces are filled
with colourless molecules, nuclei, and cells, in all stages of development,"
the cells, when fully developed, bearing the closest possible resemblance to
the colourless corpuscles of the blood.   And they agree in the absence of
excretory ducts; so that if the corpuscles formed in them are to leave the
organs in which they originate, it can only be by the lymphatics or by the
veins.   The former of these modes of exit seems improbable; for according
to the researches of Kölliker, and other most careful anatomists, the dis-
tribution of lymphatics to these organs is, for the most part, rather scanty
than otherwise, and is mainly confined to their surface.   That there is a
direct passage of the corpuscles, on the other hand, into the venous circula-
tion seems probable, from the large proportion of colourless corpuscles
which presents itself in the blood of the splenic and portal veins; a fact
that has been frequently asserted, but seems to have been now fully sub-
stantiated by the careful observations of Funke* and Kölliker.†   According
to the former of these observers, the number of colourless corpuscles some-
times amounts to a quarter, or even to a third part, of that of the red; and
he believed that he could trace the transitional forms from the one set
into the other.   The following is the additional evidence to this effect, which
is furnished by cases of leucocythemia:

  "In leucocythemia, we observe that when these glands are hypertrophied, and
their corpuscular elements are multiplied, the colourless corpuscles of the blood

* Henle's Zeitschrift, 1851, p. 172.   † Mikroskopische Anatomie, vol. ii. part 2, p. 279.

are increased in number. Two very carefully made observations, however, appear to me sufficient in themselves to determine the connexion of these lymphatic glands with the cells of the blood. Thus in case 34, where the thyroid body was enlarged, its cells and their included nuclei were considerably smaller than usual, and it was ascertained that the colourless bodies in the blood and their nuclei were smaller also. In case 35 it was seen that the colourless corpuscles in the blood were of two distinct sizes, the smaller corresponding with the nuclei of the larger ones, and the lymphatic glands were found to be crowded with corpuscles, also of two distinct sizes, exactly corresponding to those in the blood. From these facts, we can have little doubt that the colourless corpuscles are formed in the lymphatic glands, and from thence find their way into the blood. . . . . The limitary membrane which surrounds the sacular [vascular ?] glands is exceedingly delicate; indeed, so much so, that its existence has been denied by some observers. When distended, therefore, it may easily break, and the contents be poured into the pulp, surrounding stroma, or bloodvessels. Dr. Sanders has lately shown that the Malpighian sacs of the spleen are traversed by very large vessels. But it must be acknowledged, that notwithstanding the certainty which exists as to the connexion between the closed lymphatic glands and the bloodvessels, and the passage of corpuscles from one to the other, the method by which this is accomplished has not yet been demonstrated. I cannot help thinking, however, that there must be a direct venous communication." (pp. 102, 103.)

With regard to the mode in which the corpuscles are formed within the vascular glands, two theories exist, each of which may have analogical evidence adduced in its support.

"One is, that they are thrown off, in the form of epithelium, from the membrane which surrounds them; the other, that they originate in an organic fluid, by the production of molecules, the successive development and aggregation of which constitute the higher formations. I have long been of opinion, that the latter theory is the more consistent with known facts; and certainly all that I have seen during repeated investigations into the structure of the various lymphatic glands, is in harmony with it. Nowhere have I seen the nuclei and cells of these glands attached to, or apparently given off from, a membrane, still less from supposed fixed germs—but everywhere pervading a molecular fluid within the closed sacs." (p. 104.)

There are considerable difficulties in the determination of the truth on this point; and Professor Goodsir, as is well known, regards the development of the cells which fill the intra-glandular lymphatics, from germinal spots in the subjacent membrane, as one of the strongholds of his theory of " nutritive centres." We incline, however, to the belief that the mode of development from free nuclei, indicated by Professor Bennett, is that which really takes place.

The remainder of this section is occupied by a collection of anatomical and physiological arguments, adduced by Professor Bennett, to show that the doctrine for which he is contending is in harmony with such a large body of facts, as thence to derive a strong *à priori* probability in its favour. Among these we may notice, as a novelty, that the numerous nuclei found in the spleen, whose size varies in different animals, are stated always to correspond in dimension with the nuclei of their blood-corpuscles. This fact was first pointed out to Professor Bennett by Mr. Drummond, his former assistant; and he states that he has himself confirmed it " in man and various mammals, in the frog, and in the newt." We venture to think, however, that more evidence of the fact is required, than is afforded by such a limited comparison; and even supposing it to be established, it would prove

little or nothing, since every microscopist well knows that the size of other elementary parts of the body, such as the bone-lacunæ and muscular fibrillæ, usually bears a definite relation to that of the red corpuscles of the blood. No such evidence can be accepted as proof, therefore, of the identity of the splenic nuclei with the corpuscles of the blood; but the fact, if it should be confirmed on further investigation, may be admitted as concurring with others to establish such a probability.—In the last part of this section, Professor Bennett mentions some of the objections which have been raised against the doctrine he advocates, and combats them, in our opinion, successfully. On the whole, we think that a strong case has been made out in favour of the production of the blood-corpuscles in the glandulæ of the Absorbent system (and also, we would add, in the vessels themselves, since there are no glandulæ in fishes, and but few in reptiles, and corpuscles are found, though in small number, in the lacteals which have not yet passed through them), and also in the bodies that are known as "vascular glands," from which they find their way directly into the sanguiferous current, rather than through the lymphatic vessels, as formerly supposed. And thus, alike in the preparation of the corpuscles, as in other acts of assimilation—especially in effecting that intimate union between the albuminous and the oleaginous constituents of the alimentary materials, which seems to be requisite for the production of the proper histogenetic pabulum—we may look upon the whole Absorbent system (as Dr. Carpenter long since pointed out) in the light of a glandular apparatus, the tubuli of which are unravelled and spread through the body (just as are those of the liver in insects), instead of being coiled up together in a compact mass; this apparatus having for its purpose to prepare new blood from the crude alimentary substances which have been reduced to solution in the digestive cavity, and from the unused materials which have been poured out for the nutrition of the tissues: whilst, on the other hand, the Vascular glands are compact masses of cells, disposed in various convenient situations, which seem to carry-on during the whole of life the blood-making functions that are originally performed by the blastodermic membrane.

In the seventh section, we find the *Ultimate Destination of the Blood Corpuscles* briefly discussed, with reference, in the first place, to the function attributed to the spleen by Kölliker and Ecker, and in the second, to the production of fibrin. It is well known that, in the spleens of all animals, there may be observed groups of blood-corpuscles surrounded by an albuminous deposit closely resembling a cell-wall; and these, when first brought into special notice by Kölliker, were believed by him to be aggregations of degenerating red particles, thus brought together for the purpose of being (so to speak) melted-down; and this melting-down of effete red corpuscles was considered by him to be the peculiar office of the spleen. It was soon found, however, that similar bodies present themselves elsewhere, especially in spots in which any extravasation of blood has taken place; and thus the idea of their special relation to the spleen was proved to be untenable. Very conclusive evidence to this effect has been recently given by Dr. Sanderson,[*] who has shown it to be probable that the abundance of these bodies in the spleen is simply due to the readiness with which extravasations of blood take place into its substance. But Professor

[*] Edinburgh Monthly Journal, Sept. and Dec. 1852.

Bennett seems to think that two different entities have been confounded together; and that, of what have been supposed to be aggregations of effete red corpuscles, some are real cells of the lymphatic glands, which, under especial circumstances, assume a power of increased development, with endogenous multiplication of nuclei, these acquiring within parent cells the red hue which they ordinarily present only after having escaped from them.

" These are common, not only in the spleen, but in the mesenteric and other lymphatic glands, especially when hypertrophied from neighbouring irritation, the result of inflammatory or cancerous exudations, and especially in typhoid fever.   A similar increased power of development may occasionally be observed in the epithelial cells of the pulmonary air vesicles in certain pneumonias; in those covering the choroid plexus in hydrocephalus; in those of the epidermis in epithelial cancer; and in pus.   On the other hand, that extravasated blood-corpuscles may assemble together in groups, and subsequently be surrounded by an albuminous deposit closely resembling a cell-wall, is a fact of great pathological importance.   It is true they closely resemble the lymph-cells, with multiplying nuclei, but may, I think, be separated from them by possessing more colour.   I have seen them, not only in the spleen, but in other glands, and especially in the brain, following spontaneous and artificial sanguineous extravasations.   But surely it will not be maintained, that the normal function of the organs, in which these accidental formations occur, is to dissolve the blood-corpuscles.   Besides, from the numerous facts which have been referred to, I trust it has been made apparent, that the spleen is much more probably a blood-forming than a blood-destroying gland." (pp. 109, 110.)

Further observation is required to determine if this distinction be well-founded.

We are surprised that so good a physiologist as Professor Bennett should not only adopt Zimmermann's strange notion that the fibrin of the blood is " a result of the metamorphosis of the textures, and constitutes so far effete matter," but should go so far as to affirm that the arguments in support of this view seem to him unanswerable.   We have already answered, and we think refuted, a great part of these, when advanced by Mr. Simon;* and what force the others may seem to possess, appears to us to be entirely lost, when the uses of fibrin are viewed in a correct light.   We will own, for ourselves, that Dr. Zimmermann's arguments, taken in connexion with certain recent chemical discoveries, have induced us to limit the *histogenetic* value of fibrin to a certain class of tissues—namely, the gelatinous: but we are as far as ever from regarding the fibrin as an *effete* product of the metamorphosis of the tissues; and we will give what appear *to us* unanswerable arguments against such a notion.   All the products of the metamorphosis of the tissues with which we have any acquaintance, possess an entirely *inorganic* character; and the excretory apparatus is adapted to remove them from the blood as speedily as possible.   It is only whilst *en route* to their appropriate channels of elimination, that their presence in the blood can be detected.   Now, we should like to know what evidence we have that fibrin is a product of the retrograde metamorphosis of the tissues, which is to be compared with that which we possess respecting urea, uric acid, creatine, creatinine, sugar, lactic acid, &c. &c.   But further, that blood is to be regarded as most pure, which is most thoroughly freed from these products; and if they should accumulate in it, even in extremely

small amount, they speedily manifest the injurious influence exerted by
their presence, in the morbid phenomena they engender. Now, will any
one be hardy enough to affirm, that fibrin is to be placed on a footing with
these substances? that the blood would be purer for being freed from it?
that the functions of nutrition would be better performed if it were elimi-
nated, instead of its presence being allowed in the proportion of 2 or 3
parts in 1000? To such reasoners we would simply point out, that not
only does the presence of fibrin maintain, by its merely physical properties,
that physical condition of the blood which is most favourable to its free
movement through the vessels, and to its due retention within their walls;
but we find that on its vital power of fibrillation does the cessation of
hæmorrhage from even the most trifling injuries depend; that the limitation
of purulent effusions by the consolidation of the surrounding tissue, and
the safe separation of gangrenous parts, can only take place in virtue of the
same property; and that the adhesion of incised wounds, still more, the
filling-up of breaches of substance, require as their first condition, that
either the blood or matters exuded from it should be able to assume, by a
simple change of form, the condition of a fibrous tissue. Let Prof. Bennett
and our readers only consider what happens, when there is an extreme
deficiency in the fibrillating power of the blood. Let them picture to them-
selves a case of severe purpura, in which uncontrollable hæmorrhage, gra-
dually draining away the *pabulum vitæ*, follows the extraction of a tooth or
even the scratch of a pin. Let them recal those terrible cases of purulent
infiltration, in which the unhealthy condition of the blood prevents the
limitation of the pus to the spot in which it was at first effused, so that it
makes its way through the tissues, corrupting and degrading them as it
spreads, and producing the most fearful extent of disorganization. Or,
again, let them picture to themselves the separation of a slough, in which
the vessels of the living parts have not been previously blocked-up by
coagula, and the violent and perhaps fatal hæmorrhage which then ensues.
And, lastly, let them think of the condition of those pale, flabby-edged
wounds, in which there is no disposition to adhesion, or to filling-up with
granulation-structure, because the blood is deficient in that " plastic" pro-
perty which is entirely due to its fibrin. If, after taking these facts into
due consideration, they will still maintain that the fibrin of the blood is an
effete substance, and is therefore to be regarded as foreign to the constitu-
tion of pure wholesome blood, we can only say that we hope they will try
the experiment of defibrinizing (à la Magendie) a few *dogs*, before they
carry their principles into practice by purifying the blood of their *human*
patients from this noxious ingredient. We feel sure that they will soon
return to the orthodox faith; and that whatever they may think of the
origin of fibrin, they will admit that its presence in the blood has as
important a relation to its purposes in the system, as that of any of its
components, not excepting the corpuscles, can possess.

We have intimated that our own opinions on the subject of the desti-
nation of fibrin have undergone some modification since we last adverted
to the subject; and the change is simply this. In common with many
other physiologists, we formerly held the opinion, that fibrin represents
the intermediate stage in the assimilating process between crude albumen
and the living tissues, and that it is, in fact, that element of the blood

which is immediately drawn-upon in the operations of nutrition. This opinion rested in part upon the current doctrine that fibrin is the constituent of muscle, and in part upon the assumption, that, as fibrin is more endowed with vital properties than any other of the liquid components of the blood, so as to be capable of passing by itself into the condition of an organized tissue, it must be the one most readily appropriated by the various parts of the solid fabric, as the material for their growth and development. But it has been recently shown by Liebig, that, so far from there being any evidence of the identity of the fibrin of blood with the substance of muscles, the evidence is entirely the other way; the substance of muscle bearing more resemblance to coagulated albumen, or even to the globulin of the corpuscles, than to the fibrin of the blood. On the other hand, all the evidence derived from careful observation of the organization of plastic effusions, tends towards the conclusion that a fibrillating fluid poured forth from the blood comes at last to present the structural characters of the white fibrous tissue; and there are also chemical indications of the gradual conversion of the fibrinous into a gelatinous substance, the points of purely chemical difference between albumen and fibrin being such as indicate a relation in the latter to gelatin (so that Liebig speaks of the blood-fibrin as perhaps " albumen half converted into gelatin"), and the basis of false membranes and the skin of the fœtus—both of them fibrous tissues in an incipient stage of formation—being composed of a substance still more intermediate in its characters between these two classes of compounds. We seem fully justified, then, in regarding fibrin as the special pabulum of those connective tissues, whose physical offices in the economy are so important, whilst their vital endowments are so low; and we see it serving by its own vital endowments for the generation of these tissues, whenever and wherever there may be a demand for them. They may be said, indeed, to exist *potentially* ready-formed in the blood; and no intermediate stage of cell-development is requisite for their production. On the other hand, there is as complete an absence of evidence, that the tissues of the cellulo-albuminous type depend in any way upon fibrin for the material of their growth. They seem to develop themselves at the expense of the albuminous constituents of the blood, without any intermediate preparation, unless it be such as is effected by the floating corpuscles; drawing this from the fluid which escapes from the capillaries and permeates the tissues, and which, save in cases of extreme congestion or of inflammation, is not fibrinous.

We might dilate at much greater length upon this question; but we think that our readers will consider that we have already sufficiently taxed their patience; and we shall therefore withhold, for the present, some remarks we had intended making in regard to the relation of fibrin to the inflammatory process and to other disorders of nutrition. We would simply direct Professor Bennett's attention to those inquiries of Mr. Paget's, heretofore alluded to by us (vol. vii. p. 481), which, taken in connexion with the researches of Rokitansky, appear satisfactorily to prove, that so far from there being an excess of fibrin (as stated by Professor Bennett) in " weakly, phthisical, and chlorotic individuals," and a low proportion of it in " robust persons," the proportion, as indicated by the characters of inflammatory effusions, is precisely the other way,—such effusions in

robust persons showing the greatest tendency to perfect fibrillation, whilst
it is in cachectic subjects that they are most corpuscular, and show the
greatest proneness to pass into the purulent form.   The whole subject of
the relative proportion of fibrin in different states of the blood, and in the
blood of different subjects, needs, in fact, to be re-investigated, with refer-
ence, not merely to the *amount* of the colourless coagulum, but to its
composition; for not only, as Mr. Paget has remarked, is the apparent
increase in fibrin often due in reality to an augmentation in the colour-
less corpuscles; but even what is really fibrin may, though augmented in
quantity, be so deteriorated in quality, as shown by its very imperfect
fibrillation, that its excess cannot be rightly said to indicate a greater
plasticity in the blood.

It is curious that, in his eighth section, on *Leucocythemia viewed in rela-
tion to Inflammation,* Professor Bennett should distinctly recognise fibrin
as a normal and essential element of the blood; for he repeats and enforces
the statement formerly made by him, that " the essential phenomenon of
inflammation is exudation of the *healthy liquor sanguinis* through the
walls of the capillaries."   Now, " healthy liquor sanguinis" differs from
serum in no particular that we are aware of, save in the presence of
fibrin in solution.   The points especially dwelt on by Professor Bennett in
this section, are the inconsistency of the facts brought to light by the dis-
covery of leucocythemia, with those theories of Inflammation, which would
place its essential character either in the multiplication of colourless cor-
puscles in the blood (Drs. Addison and C. J. B. Williams), or in the in-
creased spissitude of the fluid and adhesion of the coloured corpuscles to
each other and to the vascular walls (Mr. Wharton Jones).   For, as Pro-
fessor Bennett remarks, " here the colourless corpuscles *are* increased in
number in the smallest vessels" (which he denies to be essentially the case
in inflammation), " and yet, instead of a universal inflammation, persons
live in that condition for months and years, without any obstruction of the
vessels whatever."   And again, though " the fibrin was augmented in one
instance to 7·08 parts in a thousand, and in another to 6·0 in a thou-
sand," and although, " on examining the blood immediately after its
abstraction from the living body in several cases of leucocythemia, the
coloured corpuscles were seen to aggregate themselves together in the
manner so accurately described by Mr. Wharton Jones," yet " in none of
these cases were the smaller vessels and capillaries obstructed, or the phe-
nomena peculiar to inflammation induced." (p. 115.)   We need scarcely
add, that we quite agree with Professor Bennett in the conclusion, that " it
is in vain that physiologists seek in the alterations of the vessels on the
one hand, or in morbid changes of the blood on the other, for the primary
cause" of inflammation; it being in a derangement of the proper nutritive
relations between the blood and the tissues, that the essential departure
from the normal condition consists.

The ninth section, entitled *Leucocythemia viewed in relation to Purulent
Infection,* contains matter of very special interest.   We have seen that the
multiplication of colourless corpuscles in the blood has produced in it an
appearance which has been mistaken for purulent infection; and it seems
necessarily to follow from the phenomena presented by this pathological
condition, " that all explanations of purulent infection founded upon the

mechanical impaction of these bodies in the minute capillaries must be erroneous," the size of the corpuscles which freely floated in the blood having in some instances been considerably greater than that of ordinary pus-cells. We cannot admit, however, that these phenomena also prove that " good and laudable pus" may be absorbed into the circulation without deleterious consequences; for however similar the corpuscles of leucocythemic blood may be to pus-corpuscles in " general appearance, size, structure, and behaviour in the addition of re agents," still they are *not* pus-cells; differing from them in the very important feature, that whereas pus-cells are characterized by their tendency to degeneration, the colourless corpuscles of the blood are characterized by their tendency towards a further development. Still we readily admit the cogency of *other* arguments on this point; and are disposed to believe that " healthy" pus produces a deleterious effect upon the blood only when introduced into it in large quantities, or when it finds there some ferment which occasions its rapid decomposition—just as, according to Frerichs and Dr. G. Budd, urea and biliary matter are comparatively innocuous so long as they retain their original characters, but immediately become poisonous when they undergo degrading changes in the current of the circulation. In reference to Mr. Henry Lee's statement, that pus mingled with recently-drawn blood occasions its rapid and firm coagulation, Professor Bennett says:

" Now it is worthy of remark, that in decided cases of leucocythemia the blood is more highly coagulable when drawn from the arm, and after death it often presents firm coagula filling the vessels, as in case 1. The same occurred in case 2 ; and yet, during the life of the patient, the blood, loaded with the colourless corpuscles, rolled through the vessels without impediment or the formation of coagula. It does not follow, then, that because dead pus is mingled with recently-drawn blood about to coagulate, that therefore it should induce coagulation of living blood in the vessels of an animal. Indeed, numerous experiments by Lebert and Sédillot show that such does not take place; for, although in some cases death followed, in others the animals lived, and the pus-corpuscles were dissolved. Hence, although the fact to a certain extent must be admitted, that when pus is mingled with blood the coagulum formed is more firm, it by no means follows that it produces coagulation of *living blood*, and is the cause of phlebitis or purulent infection." (pp. 117, 118.)

We cannot but think it very strange that Professor Bennett should allow no weight to what we deem the incontestable proof afforded by the experiments of Mr. Lee, that injection of " pure healthy pus" into the vessels does cause coagulation of the living blood. It is quite true that, as in the experiments of Lebert and Sédillot, the animal may recover; but this is because the local coagulation has effectually prevented the pus from mingling with the general current of blood, so that its deleterious influence is purely local. Let Prof. Bennett look again at Experiments VI., VII., and VIII., in Mr. Lee's Essay, and we think he will be satisfied that the coagulation of the blood in the vessels into which the pus was injected must have been immediate and complete.

The title of the tenth section, *Leucocythemia viewed in relation to Phlebitis*, is very promising; but the performance is rather disappointing, only a page and a half being devoted to this subject. The interest of the relation here indicated lies, on the one hand, in this,—that the blood in leucocythemia frequently shows a peculiar disposition to the formation of coagula in the

veins after death, which strongly resembles at first sight those which occupy veins that have been inflamed; and, on the other hand, in the resemblance which the condition of the blood in leucocythemic subjects bears, as just pointed out, to that of blood contaminated with pus. The most remarkable illustration of the former condition is furnished by the case which first directed Professor Bennett's attention to this alteration of the blood, and which was considered at the time as one of purulent infection.

"*Blood*.—The blood throughout the body was much changed. In the right cavities of the heart, pulmonary artery, venæ cavæ, vena azygos, external and internal iliac veins, and many of the smaller veins leading into them, it was firmly coagulated, and formed a mould of their size and form internally. In the cavities of the heart and venæ cavæ, the blood, when removed, was seen to have separated into a red or inferior, and a yellow or superior portion. The red portion was of a brick-red colour; it did not present the dark purple smooth and glossy appearance of a healthy coagulum, but was dull and somewhat granular on section, and when squeezed, readily broke down into a grumous pulp. The yellow portion was of a light yellow colour, opaque and dull, in no way resembling the gelatinous appearance of a healthy decolourized clot. When squeezed out of the veins, as was sometimes accidentally done where they were divided, it resembled thick creamy pus. In some portions of the veins, the clot was wholly formed of red coagulum. In others it was divided into red and yellow. In a few places the yellow formed only a streak or superficial layer upon the red, or covered the latter with spots of various sizes. Whether this coagulum existed in all the veins could only have been ascertained by a complete dissection of the body. It was seen, however, that the femoral veins, after passing under Poupart's ligament, were empty and perfectly healthy as far down as the sartorius muscle. The external and internal iliac veins, as well as the pelvic veins, were full and distended. The azygos, both axillary and jugular veins, were full, also the longitudinal, the lateral, and other sinuses at the base of the cranium, and veins ramifying on the surface of the brain. In this last situation some of the veins appeared as if full of pus, whilst others were gorged with a dark coagulum. In the aorta and external arteries were a few small clots, resembling those found in the veins. These vessels, however, were comparatively empty. The basilar artery at the base of the brain was distended with a yellow clot." (pp. 8, 9.)

Such a state, however, is readily distinguishable from that which results from phlebitis, by the absence of any alteration in the walls of the vessels, and the non-adhesion of the clot; in the case just cited, the coagula lay so perfectly free within the vessels, that they could be readily drawn out through a small orifice. The following observations by Professor Bennett, on the relation between purulent infection and phlebitis, are well worthy attention:

"From the numerous cases of phlebitis observed, especially when it originates in the uterine veins, the same general symptoms are produced as in the so-called cases of purulent infection. This indeed has been considered by many as the source of the pus-corpuscles which mingle with the blood. But it is by no means shown, that, under such circumstances, the pus-corpuscles actually circulate in the blood, much less that, if they did, the fatal result can be attributed to them. On the other hand, from the epidemic nature of the disease in puerperal women, and from its contagious character, a point which seems to be well established among practical obstetricians, it is more probable that here also a toxic effect is occasioned, which operates on the blood altogether independent of the pus-corpuscles.

"There can be no doubt that when, owing to phlebitis, a coagulum forms in the vessel, and obstruction of the blood occurs, that the clot softens, and is converted into pus. I have frequently seen such softened clots in veins, and on the internal

surface of the cardiac cavities, to be composed of colourless cells, presenting all the characters of pus-corpuscles, floating in a slightly molecular fluid. In most cases these corpuscles are prevented from entering the circulation, on account of firm fibrous coagula existing between the diffluent portion of the clot and the moving blood. But it is maintained, that occasionally the whole suppurates, and, on joining the circulation, causes the symptoms of purulent infection. If so, I argue the effect must depend upon either the toxic power of such pus, or upon fragments of the coagulum being carried into the circulation, and acting mechanically, as the mercury did in the experiments of Cruveilhier. This point, however, in the history of phlebitis, requires further investigation, as well as the separation of such mechanical effects, should they occur, from the poisonous influence of altered or putrid pus." (pp. 119, 120.)

It is obvious that the whole subject will require re-investigation, under the guidance of the knowledge now acquired, that a state of the blood closely resembling, to all appearance, that produced by an admixture of pus-corpuscles, may be produced by the undue multiplication of the ordinary colourless corpuscles.

The last section of Professor Bennett's monograph treats *Of the relation between Morbid Conditions of the Lymphatic Glandular system and of the Blood.* This, although brief, contains the results of a large number of important observations; and as we cannot compress these further than the author has already done, we shall present the greater part of them in his own words:

"It is a matter of common observation, that the lymphatic glands and vessels swell in the neighbourhood of an irritating wound, and that the former are especially liable to become the seats of cancerous and tubercular matter, apparently from the result of absorption from primary sores or lesions. The nature of this enlargement in lymphatic glands has not hitherto been very clearly understood. Generally speaking, it is attributed to secondary inflammation, which, if not subdued, advances towards suppuration, and in its turn becomes the source of similar lesions in the next series of lymphatic glands.

"On examining glands which become enlarged from the result of irritation from a neighbouring ulcer, we find them to be soft, and readily to yield on section a dirty turbid fluid. If we examine this fluid under a magnifying power of 250 diameters linear, we find it to be crowded with naked nuclei and the cell-elements of the gland, some of which last are frequently enlarged, and contain a considerable number of nuclei. It would appear that, under these circumstances, the nuclear and cell elements not only increase in number, but that some of the latter assume a power of development which they never present in a state of health. For instance, instead of there being one nucleus, it multiplies fissiparously, so that there are two, four, or even a greater number.

This condition is remarkably well observed in the enlarged mesenteric glands which accompany typhoid ulcerations in the intestines. They are then greatly distended, varying in size from a hazel-nut to that of a hen's egg. They are externally vascular, of a bright red or purple colour, are soft and pulpy to the feel, and, on section, present a slightly granular surface, of greyish or fawn yellow colour, and frequently exhibit commencing softening. . . . . .

"This power of increased development may be observed, not only in the mesenteric, but in the spleen and other lymphatic glands. In a case of large epithelial ulcer of the leg, I examined the glands after death with great care, and found them greatly enlarged, dependent apparently on the excess of naked nuclei and increased number of cells they contained. I have observed the same alteration in the axillary and cervical glands. Its occurrence in the spleen has been previously noticed. This enlargement and softening of glandular organs is strictly analogous to what occurs in articular cartilages, from the increased development of cells, and a multiplication of nuclei within them, as observed and accurately figured by Dr. Redfern.

"In certain morbid conditions, I have seen the ordinary epithelial or epidermic cells of an organ present the same tendency to multiplication. Thus, in the lung in certain cases of typhoid pneumonia, I have seen the epithelial cells exhibit the same multiplication, with increase of nuclei.

"In the epidermic cancroid disease observed in the lips, and in the scrotum of chimney sweeps, the epidermic scales increase far beyond their normal size; the cells and nuclei also enlarge, and the latter often exhibit a disposition to multiply fissiparously, as in the accompanying figure, where an enormous cell of this kind was found in a labial ulcer.

"In certain tumours of the mammæ, the ducts also may be observed to become distended with epithelial cells, as in the following figure, constituting an increased growth of cell-elements, which obstruct the tubes. A similar fact may be exemplified in tumours of the parotid, and in certain cases of cerebral meningitis affecting the ventricles, when the epithelium covering the choroid plexus is not only greatly increased in thickness, but many of the individual cells exhibit an increased number of nuclei, altogether distinct from fatty degeneration.

"From all these facts, therefore, it is evident that, under certain conditions, the growth of cell elements in an organ, or on the surface of membranes, may be increased, and constitute diseases, the symptoms of which have been long known to medical men, although we are only commencing to understand their nature. In the lymphatic glands, as we have previously seen, these cells are frequently formed, and many of them enter the blood, and are visible there, constituting leucocythemia. An extensive inquiry is thus thrown open to the histological pathologist, having reference to the questions, how far do structural alterations in the lymphatic glands affect the blood? and, how far do alterations of the blood re-operate upon the glands?" (p. 123—125.)

Certain peculiar alterations in the spleen, liver, and kidneys, are then noticed by Professor Bennett, which he considers to be of the same order with those just described. And he concludes with the following recommendations, which we gladly transfer to our pages, in order that they may be made as extensively known as possible :

"In conclusion, I would direct the attention of my medical brethren to an extended study and investigation of the blood clinically. The means of doing this, with the assistance of the microscope, are now most easily attainable, and the following points deserve attention :

"1. In some cases of typhoid fever, where there was every reason to expect disease of the intestinal glands, the colourless corpuscles have been stated to be increased in number. The importance of this observation will be recognised, when it is remembered, that, according to Brucke, these structures constitute the first series of the lymphatic glands. Further observations on an extended scale, however, are required, especially in France and Germany, where intestinal fevers are so common.

"2. An examination of the blood in cases of bronchocele seems advisable, especially in places where it is endemic.

"3. In all cases where the blood glands are extensively affected, whether from hypertrophy, scrofula, or cancer, it is possible that the blood may be found more or less altered in the relation of the colourless to the coloured corpuscles. If so, further research may explain the contradictory statements which have been advanced, having reference to the actual detection of cancer-cells in the blood.

"4. In all cases of abdominal tumours, as a general rule, it may be well to examine the blood microscopically.

"5. This should also be done in cases of purulent infection, puerperal fever, small-pox, glanders, syphilis, and all other disorders where the blood is affected on the one hand, and the lymphatic glandular system on the other. *

"6. A chemical analysis of the blood in such cases is desirable, as it is only by a multiplication of such observations that fair average results can be arrived at.

" 7. There are some cases of anæmia which kill, apparently without obvious cause. In these it may be well to examine the lymphatic glands. In a boy who so died in Heriot's Hospital, under the care of Drs. Christison and Andrew Wood, the only lesion discovered after death was induration and cell-disintegration of the mesenteric glands.

" All this constitutes a series of researches, which can only be carried out by interesting different individuals, especially those engaged in pathological investigation in large hospitals. Such, however, may be considered as absolutely necessary information to be obtained, before the laws of nutrition and the diseases connected with their derangement can be fully understood." (pp. 126, 127.)

In bringing to a close this notice of Professor Bennett's valuable monograph, we must take leave to express a doubt which occurs to us with regard to one of his inferences. He seems to consider that there is an absolute increase in the *production* of colourless corpuscles, under the circumstances to which he refers, because they bear an unusually large proportion to the red. Now it is to be remembered, that the proportion of red corpuscles is at the same time diminished to such a degree, that *the whole aggregate* of corpuscles is beneath, instead of exceeding, the normal average. Now, if it be true that the normal course of development of the white corpuscles tends to the production of the red (in one or other of the modes already specified), is not the accumulation of colourless corpuscles more probably to be attributed to *an arrest of development*, than to an augmented production? Supposing that we met with a population, not unusually dense (or rather the reverse), but presenting an extraordinary proportion of individuals under the average size; should we not rather infer that some circumstance had operated to check their growth, than that there had been an unusual multiplicity of births? It seems to us, too, that this view of the case is more accordant with the condition of the blood-making organs to which the morbid condition of the blood is attributed; for that condition seems usually to be one rather of perverted and degraded vital activity, than of an excess of normal action; and all analogy favours the idea, that the products of such organs should be disposed to pass into early degeneration, without going through their full course of development. In cases where the blood has been so milky in appearance, and has presented so little colour, as to resemble chyle rather than normal blood, it seems obvious that the morbid condition is due rather to an arrest of the normal transmutation, than to an excessive production of chyle. We recommend this view to Professor Bennett's consideration, as well as to that of our readers. That we have not misapprehended his meaning, in attributing to him the idea of *increased production* of colourless corpuscles, as the essential condition of leucocythemia, will be obvious, we think, from the following passage:

" In certain hypertrophies of the lymphatic glands, their cell-elements are multiplied to an unusual extent, and under such circumstances find their way into the blood, and constitute an increase in the number of its colourless cells." (p. 112.)

We do not deny that this may be the case in some instances; but it will be obvious, we think, on Professor Bennett's own showing, that the increased production of *colourless* cells must *normally* tend to an augmentation in the proportion of *red* corpuscles in the blood; for he states, only a few lines above,—

" That the blood-corpuscles of vertebrate animals are originally formed in the lymphatic glandular system, and that the great majority of them, on joining the circulation, become coloured in a manner that is as yet unexplained. Hence the blood may be considered as a secretion from the lymphatic glands, although in the higher animals that secretion only becomes fully formed after it has received colour by exposure to oxygen in the lungs." (p. 112.)

Hence, even if an increased number of colourless cells should find its way into the circulation, an arrest of their development must take place, in order that they should accumulate *as such* in the blood.

In conclusion, we would again express the pleasure which we feel, in welcoming this product of Professor Bennett's labours in a field which he has so peculiarly made his own; and look forward with satisfaction to the results of these more extended inquiries, in which he will be efficiently aided, we doubt not, by the band of zealous histologists whom he has the opportunity of training under him, and whose contributions to anatomical and pathological science have already been such as to reflect great credit on their instructor. But we would urge it upon our readers, generally, to promote Professor Bennett's researches by the communication of any facts which can throw light upon the numerous obscurities that remain to be cleared up; and would insist upon it as the duty of clinical teachers more especially, that they lose no opportunity of examining the structural and chemical characters of the Blood, in connexion with the history of Disease.

Art. XVI.

1. *Researches into the Pathology and Treatment of Deformities in the Human Body.* By JOHN BISHOP, F.R.S., F.R.C.S., &c. — *London,* 1852. 8vo, pp. 266. With numerous wood-engravings.

2. *Lateral Curvature of the Spine, its Causes, Nature, and Treatment.* By R. W. TAMPLIN, F.R.C.S., &c. &c. — *London,* 1852. Royal 8vo, pp. 44. With four plates.

3. *The Spine: its Curvatures and other Diseases, their Symptoms, Treatment, and Cure: to which are added some Remarks on Paralysis.* By CHARLES VERRAL, M.R.C.S., &c.—*London,* 1851. 8vo, pp. 234. With a plate, and woodcuts.

4. *On the Prevention and Cure of Spinal Curvatures and Deformities of the Chest and Limbs; being the result of many years' experience.* By MRS. GODFREY.—*London,* 1851. 8vo, pp. 90.

EACH successive year produces its crop of books on deformities, just as regularly as strawberries appear in June. The burden of the whole of them is very nearly the same; there may be an exception here and there, but the vast majority unmistakably speak as follows:—" Deformities are very common and very painful imperfections of the human frame. Of these, affections of the spine are the most frequent; indeed, one form of disease—lateral curvature of the spine—is an almost necessary concomitant of civilized and elegant life. Females are especially subject to it; and so insidious is its progress, that parents would do well to consult

some spinal doctor every now and then, just to see that their daughters' backs are kept straight. However, no matter what progress the disease has made, something can always be done to check and relieve it. The author has met with unexampled success in this department of his profession; and after days and nights of sleepless toil, and an expenditure of brains equalled perhaps, but not exceeded, by Mr. Adams in those calculations which resulted in the discovery of a new planet, has invented (some of them call it *discovered*) an instrument which he can guarantee will screw the most crooked back straight again. Many other persons profess to do this also, and perhaps honestly think they can do it; and far be it from the author of this veracious treatise to insinuate that they are quacks. He owes a duty, however, to the public; and he regrets to say that a short time ago he saw a deplorable instance of the mischief which had been produced by the machine of Mr. A., of B Square, and that recently he was consulted by a lady, whose daughter had been under the care of Messrs. C. D. E., and also the aforesaid A., but who had derived no benefit from their prolonged and expensive attendance. It was his better fortune, however, owing to the more scientific principles upon which he went to work, to achieve a more favourable result; and in this instance, one of the worst he ever witnessed, the patient has been restored to her sorrowing friends, and is now the life of her social circle, the most active among the active, and the sweet dispenser of the best form of charity, in the shape of advertisements and recommendations to the author and his establishment."

The first thing that strikes one in these treatises is, that they mutually contradict one another. Either deformities of the spine get well in spite of the most contradictory treatment; or they are often said to exist, when in truth they do not exist at all; or the cases published are much exaggerated, or are entirely false. It is quite impossible to come to any other than one of these conclusions; but to which of them, our sagacity has not yet decided.

Of the books at present before us, that of Mrs. Godfrey has least pretension and most piety; and we give it the preference which she may claim as the privilege of her sex. Mrs. Godfrey does not profess to science, but trusts in " Divine Providence" and rubbing with " animal oil." She lives in Renshaw-street, Liverpool; and carries on a business originated by her late husband. At the time she began, few works had been written on curvatures of the spine, and none in which she could altogether concur; so, " human aid being unattainable," she " was led to seek wisdom from God"—and began to study anatomy. " She was much delighted by the discovery that every muscle terminated in a tendon, and each tendon was inserted in a bone; as this discovery gave her a clear idea of the plan of treatment to be adopted, and was the lever of her hope." (p. 6.) Charming and affecting simplicity! But what a picture of the neglected education of the wife of a medical man! Surely such a confession implies nothing else, than the total exclusion of the whole feathered tribe from the Godfreyan dinner-table. Even a rabbit or a hare must have been interdicted, lest it should prematurely reveal the secret to the enquiring lady. However, the discovery being once made, an opportunity was soon afforded of impressing the fact upon her mind. A lady presented herself, in whom there was a displacement of the joints of the vertebræ, two being inclined

to the right and two to the left, from the fourth to the tenth dorsal.   In a little time, from the use of the medicated vapour-bath, and we presume also of the animal oil, " the muscles were relaxed, the joints readily yielded to pressure, and were restored to their right position, and the pain left her." Mrs. Godfrey never met with a similar case before, and has never seen one since.   It was clearly miraculous, and expressly sent to convince her that muscles did terminate in tendons, and tendons in bones.

Since then, she has gone on briskly and successfully; she rubs people's backs, thumbs their vertebræ, and anoints them with oil; she tried flour, but it did not answer; it was cleaner, to be sure, but it did not afford the same nourishment.   She never uses artificial supports.   " Some eminent surgeons have kindly suggested them to her, as an auxiliary to the carrying out of the other plan ;" but she was proof against the temptation ; she never could consent to adopt them, because she thinks them positively injurious.   And why should she?   Surely no instruments could have done more than she has done without them.   A little boy, four years old, came to Mrs. Godfrey with the upper part of his legs bowed outwards— " through being put on a rocking-horse when too young, so that the knees met; the two bows forming a circle."   She bathed him in salt-water, rubbed him with the oil, and then rolled him up in linen.   She taught him to kick straight out when lying on his back, and did so many coaxing things, that the bones could resist no longer; they fairly gave in, abandoned their circular ideas, and in three months the boy was " straight, strong, and healthy, without the aid of irons or other supports."

Mrs. Godfrey considerately warns the reader not to be led wrong by her success; but to take example by the mother of one of her patients, who devoted four hours daily to the friction of her daughter's back, but each month the deformity increased.   She requested to have the same oil as Mrs. Godfrey used; " but," *naïvely* observes the good lady, " *the secret was not in the oil.*"

In the second part of her book, the authoress exposes the incompetence of monthly nurses, the ignorance of mothers, and the shocking consequences of rough play with papas and uncles.   We have not the presumption to dispute with her on either of these points.   We cannot, however, refrain from giving increased publicity to the direful effects which followed a rough game in a little girl now under treatment.   We are told of this interesting little creature (we quote the words) that " in one of her playful freaks with an uncle" (a perfect Richard, we fear), " two of her short ribs were pushed up under the angle of the long ones, which caused an inclination to one side, thereby displacing two of the joints of the dorsal vertebræ." (p. 64.)   As a set off to this uncle, the little girl was blessed with a mamma with " a vigilant eye," who instantly took her to Renshaw-street ; and thus lateral curvature has been avoided.

Such of our readers as have been brought up on the buttoned-breeches plan, have reason to feel thankful that they have escaped the perils attendant upon that popular mode of dress.

" When," says Mrs. Godfrey, " children are a little older, they are made to wear trowsers fastened by a large button, which has been a frequent cause of curvature. If the button is pressed against the side of the bone, the curve will be lateral ; if upon the top, it will be anterior, causing a projection above the bone, which is flattened."   (p. 40.)

One caution more, and we take a respectful farewell of this observant lady. Mothers, make it a point to examine your children's pockets!—the germs of spinal curvature may be there concealed. Mrs. Godfrey, on examining a little girl, discovered four of the ribs on the left side of the sternum elevated, and four on the right inverted. There was nothing to account for this; the dress was properly made, and the child's position had been carefully watched; at last, however, the pocket was searched, and in it were discovered pence, pebbles, and a heavy knife. No wonder that the ribs rebelled against such an overloading.

We have carefully gone through Mr. Bishop's work, but without succeeding in eliciting from it a satisfactory reply to the inquiry, why he has written it? Our expectations have been kept on the continual stretch, as page after page was anxiously scanned, in the hope of eliciting his secret. It is hardly fair in Mr. Bishop to write an elaborate dissertation on deformities, fortified by a good deal of science, and supported by an amazing array of mathematical formulæ, for the express purpose of proving that all methods hitherto devised for the cure of distortions, whether of the trunk or extremities, are founded on false principles, and in practice attended with injurious consequences. We scarcely think a man is justified in overthrowing all our fond hopes, all the cherished experience of years, without substituting something more satisfactory than a simple demonstration that we are in error. True, the knowledge of a fault is half way to its cure; but we should owe a very qualified debt of gratitude to sanitary reformers, if they left us with the uncomfortable belief that we are all slowly dying from unnecessary causes, and yet did nothing to assist us to cleanse our dwellings and purify our streets.

Mr. Bishop, truly, hints occasionally that he does know something worth telling; but he never for a moment lets us behind the scenes; or offers a practical suggestion of any moment. He, too, concludes his book with narrating an example of the consequences resulting from a young lady's wearing crutches to her stays, under the advice of " an unprofessional empiric," and adds, " the body is now dreadfully distorted, and the opportunity has been lost for effecting a cure; which might have easily been accomplished, had proper treatment been applied in the earlier stage, instead of trying the experiments of an unprofessional empiric." (p. 265.) That is to say, in other words, Mr. Bishop could have cured the young lady had she consulted him earlier. Such, then, being the case, we have a right to ask, why does not Mr. Bishop tell us what means he would have applied for the purpose? or are we and others to accept his word for it, and to send our patients to Bernard-street, London, instead of to Renshaw-street, Liverpool? We would not for a moment impute to a gentleman of Mr. Bishop's honourable position in the profession, any motives so unworthy; on the contrary, we give him full credit for his desire " to induce the members of the profession to advance from the employment of mere mechanical experiments, to a sound and judicious practice, founded on scientific data;" and our remarks are only intended to point out the natural tendency of the mode of exposition he has adopted. His own mathematical attainments are well known to be of a high order; but he appears to forget how few of his readers are likely to be able to follow

him in the application of the principles which he lays down.   Yet the following passage from his preface, which follows the one just cited, would seem to recognise such a probability.   " Should any of them doubt, or find it difficult to understand these principles, the author is ready to refer them, for that practical satisfaction which results from high authority, to some of the most eminent surgeons of the metropolis, who have seen and approved the results of their application."   We can only wish that Mr. Bishop had given them the opportunity of judging for themselves, by a clear enunciation of his mode of practice.

The first point that strikes us, in a critical examination of Mr. Bishop's treatise, is an assertion at page 3, *àpropos* of the neglect of parents to apply with their children at an early stage of spinal disease, which we cannot but believe to be greatly exaggerated.

" It is, unfortunately, too true, that many young persons (especially among the lower ranks of society) are neglected during that period of the disease when the adoption of methods for relief might have been attended with the most beneficial effects ; for the parents, being generally unconscious of the mischief working in the frame of their offspring in the early stages, do not become aware of their melancholy condition until, perhaps, several bones of the spine having been absorbed, anchylosis has taken place, and the distortion consequent upon it is irremediable.   Cases of this kind frequently come under the observation of the surgeon." (pp. 2, 3.)

Now we appeal to any person familiar with hospital practice, whether the facts are not rather the other way?   Whether such extensive disease as is here alluded to, extending over a long period of time, and cured by the process of anchylosis, must not necessarily have proclaimed its existence long before the events here described could possibly have taken place?   Whether, in truth, children are not very often brought to us at quite the early stage of caries of the spine,—in whom the disease advances in spite of treatment, and terminates at last, to our great satisfaction (since no better termination can be looked-for), in the production of that very condition of deformity in which they are described as first applying for relief?

The second chapter on " the Vertebral Column" is very interesting, and instructive in an anatomical point of view.   In speaking of the form of the spinal column, Mr. Bishop observes, that in a mesial section of the body, the elasticity of the intervertebral cartilages and ligaments will cause the several parts to be distorted from their normal position, unless certain precautions are taken ; and thus an erroneous impression will arise as to the real shape of the spine.   The brothers Weber, aware of this, removed from a dead body the intestines and such of the muscles only as they found necessary, without disturbing the ligaments of the spine and thorax, and then poured plaster of Paris into the cavities of the abdomen and chest ; as soon as this was set, and the parts fixed in their natural position, a mesial section was made, and we have thus obtained the only true outline of the figure of the spine which has hitherto been produced. " The figure," then, " of the spine is an irregular curve, the superior and inferior portions of which, viewed in front, are convex, and the central portion is concave." (p. 22.)

The chief advantage of this curved form, we are informed in a note, is

(as we have always been accustomed to hear and to teach), that the shocks and jars applied at either end of the spine tend to bend the axis, while they would, if the spine were straight, crush some of the vertebræ, in consequence of the force acting on them in a perpendicular direction. It is a complete mistake, however, to suppose, as Rollin and Magendie have done, that the spinal column is stronger in consequence of this arrangement; on the contrary, Mr. Bishop satisfactorily proves that it is much weaker. The practical result of this chapter seems to be, that in delicate persons, owing to the vertebræ, in common with the other bones of the body, having a smaller quantity of earthy matter in their composition than they ought to have, the bones lose their normal elasticity, and becoming compressed by the weight of the body, do not readily regain their proper shape, and thus a permanent alteration of figure is brought about. This conclusion is illustrated by an engraving of a distorted spine from the Hunterian Museum; of which we are told, that " it is a case compounded of unequal pressure on the intervertebral cartilages, softening of the bodies of the vertebræ, and, consequently, of unequal muscular action, all arising from a disturbed condition of the mechanical and chemical forces which act on the body in its natural state." (p. 43.) Our readers will probably think with us, that this diagnosis is sufficiently vague; of the proper treatment of such a case we are told just nothing.

Mr. Bishop next passes to the muscles; and from a review of the physiology of muscular action, concludes that in spinal curvature there is no advantage to be gained by the division of the sacro-lumbalis and longissimus dorsi muscles, which cannot be obtained by other means;—an opinion in which most surgeons of repute would coincide. But Mr. Bishop goes a good deal further than this; and sneering at the award of 5000 francs by the Académie Royale de Médecine, to Stromeyer and Dieffenbach,—the first for proposing, and the second for executing on the living subject, the operation for strabismus, observes, that

" It would be more desirable to offer double that sum for the discovery of the means of curing strabismus without the section of muscles and tendons; and we ought not to despair of attaining this end, seeing that the present practice is directed against effects, or at least against secondary instead of primary causes." (pp. 59, 60.)

Further on he says, that if muscles are stretched by any force long enough applied, they will not regain their primitive condition until after a lapse of time of greater or less duration, to be determined by future experiments; and that therefore, when the contracted muscles are situated in the back and lower extremities, we have ample means of promoting their extension, and have no excuse for dividing them by subcutaneous section. Finally, he thus concludes:

" On a careful and rigid examination of the subject of myotomy and tenotomy, it appears that these operations cannot, in the majority of cases, be deemed expedient for the purpose of curing deformities, as nothing can be obtained from them which may not be effected by other and less objectionable means. The excepted cases, if any really exist, are strabismus, and other analogous affections; but even these are to be regarded with great suspicion. The best surgeons of the French school have already decided against the practice, and few of our most eminent English surgeons adopt this empirical plan of mutilating the body." (pp. 61, 62.)

It is well known that Mr. Bishop has devoted much time and attention to these subjects; and as he takes upon himself to censure a practice which everybody knows to have been attended with speedy and most beneficial results in a vast number of cases, it is not too much to expect that he should support his opinions by some practical results, which might demonstrate their correctness. We search the volume, however, in vain, for anything so reasonable; and are constrained to remark, that while everybody else is condemned, and the author and his principles alone held up for commendation, there is not a shadow of evidence presented to us, whereby we may form an independent opinion.

The next chapter is on the Mechanical Causes of Distortion, and is illustrated by many woodcuts, explanatory of the subject. *Apropos* of the "effects of wooden legs," the author justly praises the admirable contrivances which of late years have given to the victims of amputation so many advantages not hitherto obtainable, and remarks—

"When we reflect that every person who is obliged to wear the common wooden leg, not only loses the power of quick walking, but becomes deformed, it is a matter of great importance that surgeons should be familiar with these consequences, more especially as it is too much the custom at our hospitals, as well as in private practice, for them to consider their duty at an end when they have amputated the limb, healed the stump, and directed the patient to an instrument-maker. On the contrary, a very important duty still remains to be performed—namely, that of promoting the future welfare of the patient by prescribing a proper substitute for the natural limb; and the immense funds subscribed for the support of most of our hospitals might surely afford some small allowance to be appropriated for the purpose of supplying poor patients with such improved wooden legs as would enable them to perform all the ordinary occupations of life without difficulty or distortion. This is a subject that army and navy surgeons, more particularly, would do well to take into consideration; since, with the assistance of such a wooden leg, soldiers and sailors might be enabled to discharge most of the common duties of the service, instead of being dependent, as they now are, from the moment they are deprived of a natural leg." (p. 79.)

Passing over a good many pages, we find that the author holds the opinion, contrary to that generally entertained, that the white fibrous tissues are elastic. He states that he has verified the conclusions to which reasoning had brought him, by experiments on the lower animals; and that he has found the ligaments of the joints to possess a very high degree of elasticity, but a very limited amount of extensibility (p. 122, note). We are not informed, however, how these experiments were made.—In the succeeding pages we find the same condemnation of tenotomy, and the same absence of any proof that it can be dispensed with; unless, indeed, such a statement as this can be allowed weight.

"The author has been called in to complete the cure of cases of tenotomy, which, although the subjects were young persons, required the use of instruments many months after the division of the tendon. This circumstance furnishes an additional argument against tenotomy in general, since in many cases the cure is not hastened by that mode of treatment." (p. 143.)

Let us now pass to the "Pathology and Morbid Anatomy of Deformities." On the subject of caries, cancer, and enchondroma of the bones, we find the author affirming very positively, on the authority of Lebert,—what is, in our opinion, exceedingly doubtful,—the occasional co-existence of tubercle and cancer. We are still disposed to believe in the incom-

patibility of the two diseases; and shall continue to hold our ancient faith, until more satisfactory evidence shall have been adduced of its incorrectness.

We find nothing of particular moment in the chapter on Rickets, save the general doctrines with which it opens, whose special application to the case of rickets we altogether fail to discern.

"In tracing the cause of rickets, the first step will be a physico-pathological inquiry into the development of animal organs. According to Schelling, 'The peculiar character of organization is, that the matter is only an accident of the thing itself, and that the organization consists of form alone. But this form, by its very opposition to matter, ceases to be independent of it, and is only in idea separable. In organization, therefore, substance and accident, matter and form, are completely identical.' Dr. Whewell has defined organic life to be 'a constant form of circulating matter, in which the matter and the form determine each other by peculiar laws, that is, by vital forces.' It is further supposed by him, that the vital forces by which these changes are effected may be distinguished from chemical and mechanical forces, inasmuch as the latter tend continually to produce a final condition, after which there is no further cause for change. Mechanical forces tend to produce equilibrium; chemical forces tend to produce composition, or decomposition; and this point once reached, the matter in which these forces reside is altogether quiescent. But an organic body tends to constant motion, and the highest activity of organic forces shows itself in continuous change. Again, in mechanical and chemical forces, the force of any aggregate is the sum of the forces of all the parts: the sum of the forces corresponds to the sum of the matter. But in organic bodies the amount of effect does not depend on the matter, but on the form: the particles lose their separate energy, in order to share in that of the system; they are not added, they are assimilated. These views, if admitted to be correct, will form a basis upon which to found an examination of the origin of the disproportionate growth in rickets, and may at some future period lead to the solution of the question, whether the vital forces themselves, or the materials on which they act, are in fault." (pp. 205, 206.)

As the three concluding chapters of the volume are devoted to an examination of the mechanical means employed for the cure of deformities, we shall dwell somewhat upon them.

The various kinds of machinery are classed under the three heads: 1st. Machines designed to stretch the body either in a horizontal or vertical position; 2nd. Those which tend to prop the body and limbs; 3rd. Those which have for their object the reclining the body either obliquely or horizontally in prone and supine positions.

The *stretching plan* is very common all over Europe; and was resorted to by Ambrose Paré, on the supposition that distortions of the spine are dependent on the dislocation of one or more of the vertebræ. It is still actively employed in Paris, and apparently on the same erroneous principles; but whether used to reduce supposed dislocation of the vertebræ, or with whatever object, it is condemned *in toto* by Mr. Bishop. As was before shown, the spine has three curves, the largest of which is designed to give space for the great organs of respiration, circulation, and digestion; and as stretching must, if it does anything, diminish this curve, of course it seriously interferes with the due accommodation and action of these important viscera. A general notion also prevails, that many spinal curvatures are produced either by the weight of the superincumbent parts, or by the abnormal contractions of the muscles of the back; but as this view is erroneous, except in those cases in which there is disease of the

bodies of the vertebræ, the extension cannot effect a cure, and must still more evidently fail in cases where the bones of the spine have permanently assumed a wedge-like form.

Propping and Supporting Systems.—Stays, says Mr. Bishop, must be employed from an idea that the human body is not a self-supporting machine; and he proceeds to enforce their injurious consequences, by the usual arguments. We shall quote all that he here remarks upon the employment of such supports when the spine is actually curved.

"If corsets are so detrimental to the figure and health of young persons as all experience proves them to be, and if they lead to debility and deformity of the trunk, how is it that they are introduced to correct the evils they produce? Is there really existing in this case an illustration of Hahnemann's dogma—'Similia similibus curantur'? A strict examination into this matter will dispel the illusion, and show the practice, although somewhat plausible, to be really unsound. Stays of different kinds have long been introduced with a view of supporting the spinal column, and correcting its distortions, under an impression that curvatures of the vertebral column arise either from its being unable to support the superincumbent weight, and its yielding in the same way as any other elastic column of metal or wood bends when overloaded; or from the muscles on one side of the spine acting with greater force than those on the other, and so pulling the spine into abnormal curves. It is supposed that if the trunk be encompassed with a corset which props up the columns, and at the same time presses against or pushes in the prominent parts, both the tendency to distortion, and the distortion itself, will be removed. Accordingly, we have Hossard's lever belt, Tavernier's lever belt, with inclination busk, Amesbury's, Biggs', and a large variety of stays, all of them designed for the object just mentioned." (pp. 230, 231.)

The *supine* and the *prone* position of the body next come to be considered; and in these the author seems to admit that there is some value. The triple inclined plane of Mr. Earle is the best of these inventions, because it has the advantage of changing the position of the body as often as may be required, without calling for great exertion. The pith of Mr. Bishop's experience is contained in the following quotation:

"The result of a number of observations, without entering into details, is this—namely, that in cases of curvatures of the spine arising from disease and absorption of the bone, the distortions do not increase while the body is kept in horizontal, supine, and lateral positions; but they do increase when the body is allowed to move and be erect; and that, moreover, when patients are confined to the prone position, so far as the author's experience goes, the curve of the spine is progressive, for which there are obvious mechanical reasons. For instance, in all cases, both of diseased bone and curvature, the superincumbent pressure cannot be wholly withdrawn in any oblique position; and where the curvature is in a plane or planes intermediate between the mesial and transverse, as generally happens, the deformity may often be increased by the tendency of the unsupported curved position towards the transverse plane." (p. 236.)

The author fortifies his conclusions by relating the case of a girl who was confined night and day to a prone couch for nearly two years, but in whom the vertebral disease rapidly advanced, and the general health materially suffered. A most telling woodcut illustrates the patient's condition; and in the following remark few will hesitate to coincide:

"Since no part of the body can be subjected to continual pressure without ulceration being produced, it is clear that whatever state of recumbency may be adopted, it ought not to be persevered in for any length of time; but relief should be afforded by changing the position, which, as we have seen, may be done without detriment." (p. 240.)

The concluding chapter is on the Physical and Psycho-Physical Treatment of Children; but contains nothing that has not many times been said before, with at least equal force and propriety.

In order to justify our complaint of the vagueness of Mr. Bishop's directions for the practical treatment of deformities, and of the occupation of so large a part of his book with the discussion and refutation of the methods of others, we have been much inclined to cite the whole passage commencing at p. 244 and ending at p. 249, which contains all that we can find of his own; but we are so fully satisfied that after the perusal of this, our readers will be very little wiser than they were before, that we refrain from making the extract, in the belief that our space may be better occupied.

The two remaining books on our list may be dismissed with but few comments. They proceed from the pens of two gentlemen who are practising in this metropolis, in the special department of surgery upon which they write. One of them at any rate (Mr. Tamplin), belongs to an institution that ranks amongst the most useful with which we are acquainted, and which can appeal to hundreds of cases which disprove the wholesale condemnation passed upon orthopædic surgery by Mr. Bishop. We are far from agreeing with all that is advanced by many persons in favour of the purely mechanical treatment of deformities, and still less can we join those who constitute themselves the champions of indiscriminate tenotomy; but we ourselves have had too great opportunities of witnessing the results of the treatment inculcated by Mr. Tamplin, to doubt the correctness of the principles upon which it is founded. His present publication is a reprint of the substance of his Lectures on Lateral Curvature, in his work on the ' Nature and Treatment of Deformities;' and has for its special object to recommend an instrument which he has constructed to assist in the cure of these curvatures. The most questionable passage in the book, is the recommendation that the instrument be worn night and day; and although we have Mr. Tamplin's assurance that the general health frequently improves under this severe management, we must be permitted to agree with what is said by Mr. Verral on this head.

" Upon no occasion have I found that I gained any advantage from the application of mechanical contrivances, when used for more than five or six hours daily; indeed, about six hours should be fixed as the maximum period for which they can be advantageously worn, any time beyond this serving only to debilitate the patient, and give rise to a dangerous amount of bodily prostration; moreover, I think that such a time will be quite sufficient to enable the patient to engage in an ample amount of exercise and recreation for keeping her in strong and vigorous bodily health; and at all other times she will be deriving infinitely more benefit, from the employment of gymnastic exercises, or lying in the prone or supine position." (p. 90.)

It would be impossible to describe in words the spinal instrument recommended by Mr. Tamplin; and those who have not seen it, can refer to the diagrams placed at the end of his volume.

Mr. Verral's book is somewhat ambitious, but, on the whole, very well executed; and even if it were not so, the humility of his Preface would almost disarm criticism. His great panacea is the prone couch; a useful invention, which we have often employed, but the value of which he certainly overrates.

He has also constructed an instrument, partly on the plan of the one employed by Mr. Tamplin, which, with somewhat of unnecessary parade, he formally makes over to the profession. We dare say it is very useful, and it certainly is very ingenious. There is, however, a something in Mr. Verral's book, which we would gladly see removed—a sort of writing for the public; indeed, in one place he fairly enters into some elementary explanations for the benefit of the non-professional reader who may chance to refer to his pages. Perhaps this defect is almost inseparable from a work on so popular a subject as deformities; but certain it is, that we rarely do meet with any author of this class, who is entirely free from this imputation. Mr. Bishop constitutes an honourable exception; and the fault we find with him is, that his work presupposes too much learning on the part of his readers, and requires, for its beneficial use, a capacity for applying principles to practice, which is, we fear, considerably beyond the average of our profession.

Art. XVII.

Chapters on Mental Physiology. By Henry Holland, M.D., F.R.S., F.R.C.P., &c. &c.—*London*, 1852. 8vo, pp. 301.

The title of this volume is one with which captious critics might find fault, and not without a show of reason. We have no wish, however, to rank under such a category; and readily adopt our author's explanation of it, more especially as we own ourselves incapable of showing how it could be changed for the better. By the term *Mental Physiology*, Dr. Holland wishes to express "that particular part of human physiology which comprises the reciprocal actions and relations of mental and bodily phenomena, as they make up the totality of life." And he has adopted the title, *Chapters* on Physiology, "partly to avoid the profession of a complete treatise, which this is not; partly to indicate that most of these topics, and even their titles, are taken from another work, the first edition of which was published thirteen years ago." Those of our readers who have enjoyed the profitable pleasure of perusing Dr. Holland's 'Medical Notes and Reflections,' will doubtless remember certain very interesting chapters on the "Effects of Mental Attention on Bodily Organs," "On Time as an Element in Mental Functions," "On the Relations of Dreaming, Insanity," &c., "On the Brain as a Double Organ," "On Phrenology," and "On the Present State of Inquiry into the Nervous System," which are dispersed through different parts of the volume, in the midst of subjects of a more strictly medical character. Thus separated from each other, they were deficient in the sequence and connexion naturally belonging to the topics of which they treat, and indispensable to a proper consideration of the entire subject; and feeling that there are circumstances, at the present time, which render it peculiarly desirable that the subject *should* be placed prominently before the view both of the profession and of the public, Dr. Holland has brought these chapters together, enlarging them where addition could be profitably made, and re-writing them where alteration had become requisite, in consequence of the progress of knowledge; whilst he has added three entirely new chapters, "On Mental Consciousness, in its

Relation to Time and Succession," "On the Memory, as affected by Age and Disease," and "On Instincts and Habits," which were originally intended for a second series of his former essays. He has thus embodied within the compass of a very moderate-sized volume, a philosophical digest of the products of observation and reflection that have been habitually carried on throughout his professional life, on topics of the highest interest alike to the medical practitioner and to the scientific physiologist and psychologist.—The following extract from the preface will convey an idea of the author's general plan and purposes, to those who are not familiar with his former work :

" Much of course will be found in this volume which is familiar to those who have studied the subject, especially of late years. But, if I do not deceive myself, there are still certain facts not heretofore duly recognised or defined, and certain relations of phenomena requiring fuller illustration than they have yet received. These I have sought to embody in the ensuing chapters, in the order which seems best calculated to give connexion and unity to the whole. To arrange under new combinations what is already known to us, is often in itself a source of fresh knowledge, or a valuable means of correcting previous error. Various instances to this effect will, I trust, occur to the reader in his progress through the volume. I may add, that in the discussion of the subjects, though obliged to adopt certain divisions for the sake of clearness, I have kept in constant view that great law of continuity, which equally governs all mental and material phenomena. No conclusions are more secure, or more profitable, than those drawn from a careful notice of continuous relations; and of those gradations of change, which bring extreme cases within common laws, and reconcile anomalies with facts familiar to experience. To this I would advert, as a principle I have largely applied in every part of the volume.

" The topics treated of are such in their nature as perpetually to bring us to the very confines of metaphysical speculation. Except in the case of one great question, which could not be put aside, I have carefully avoided passing over this boundary. Convinced of the general truth of the maxim, that 'it is safer and easier to proceed from ignorance to knowledge than from error,' I have endeavoured throughout to separate what is known from that which is unknown—what is capable of being reached by the human understanding from that which is presumably unattainable by it. The close adherence to this principle will probably expose me to the charge of having surrounded the subject with unresolved doubts and difficulties. But I think it far better to incur this imputation, than to assume ■ knowledge not yet possessed, or to cover the deficiencies of reason by any mere artifices of language." (pp. viii., ix.)

One of Dr. Holland's main objects has been to point out the mode in which the mesmeric phenomena and doctrines, and the topics collateral to them, which have drawn so largely upon public attention of late years, should be scientifically examined. We are glad to find his views in such complete accordance with our own on this point. We are convinced that the profession has done discredit to its own character, and has lost no inconsiderable amount of public confidence, by the tone of ridicule which it has generally assumed with regard to the class of phenomena in question; and that it will not recover its lost ground, until it shall have carefully and candidly examined them, with due attention to their relations to known physiological and pathological actions, in the mode and spirit of which Dr. Holland gives in this volume so excellent an example. The only fault we have to find with his treatment of it is, that it is too general and deficient in explicitness, so that it is likely to be accounted vague and unsatisfactory by those who have not been prepared by their previous

studies and inquiries for apprehending the whole meaning which is con-
veyed in some of Dr. Holland's philosophic abstractions.   And this, we
fear, will operate against the extended appreciation of the present volume,
as it has done against the full recognition of the merits of the former col-
lection.   Men are usually more struck by new facts, than by new aspects
of facts previously familiar to them; and finding but few of the former in
Dr. Holland's essays, they are apt to ask themselves what they have gained
by their perusal.   In many instances, Dr. Holland has drawn just that in-
ference from the collocation and comparison of facts, which they would have
themselves done *if* those facts had been brought before them under a like
arrangement; and they have hence regarded the merit of that inference as
being their own, rather than Dr. Holland's, altogether overlooking the fact
that it was to him they owed the means of making it.   And even where
he has propounded doctrines possessing more obvious originality, these
have been generally advanced with such an avoidance of the dogmatic
form, and with so many cautious reservations, that they have taken no
definite grasp of the mind of the reader; which may, nevertheless, have
received from them such a direction and impulse, that it works out the
same conclusions in a form of its own at some future time, and may fancy
them to be original, unless any circumstance should occasion a recurrence
to Dr. Holland's suggestive pages.   This is what has happened to our-
selves in numerous instances, and we know that it has occurred also to
others.   And our author must be content to look for his reward, rather in
the consciousness of having successfully laboured to lead others to the
formation of correct and philosophical views, than in the direct recognition
of the original source of those views,—rather in the good which he has
done, than in the applause which he will receive from it from the public.
But this, to a man who loves truth as Dr. Holland has shown that he does,
can be no real disappointment.

Each of the chapters of which we have enumerated the titles, might well
serve as the text for a critical discussion; but upon such a discussion our
limits forbid us to think of entering; and we must content ourselves with
attempting to give an idea of the mode in which Dr. Holland treats the
subjects now for the first time introduced, and with selecting a few passages
for extract.

In the introductory chapter *On Medical Evidence*, which has been trans-
planted from the former collection, we find the following additional pas-
sage, which conveys warnings that are alike needed by the public and by
the members of our own profession:

"Here, again, I must advert to a circumstance which renders strict attention to
the laws of evidence a matter of peculiar obligation at the present time.   This is
the tendency, so marked in modern physiology, to carry its researches into the more
abstruse questions connected with vitality, the nervous power, and the relations of
mental and material phenomena,—inquiries justifiable in themselves, but needing
to be fenced round by more than common caution as to testimony, and the conclu-
sions thence derived. Yet here especially it is that such precautions have been
disregarded; partly, it may be, from the real difficulty and obscurity of the sub-
ject—still more, perhaps, from the incompetency of many of those who have taken
it into their hands.   For these researches, no longer confined to a few as heretofore,
have become the property and pursuit of many who wander merely on the confines
of science, believing they are within its pale; and whose speculations on what they
see are little checked by collateral knowledge, or by a due estimate of the laws and

limits of scientific inquiry. The mystery of the subject is in itself a charm and seduction to the mind. They see, and give attention for the first time to, those wonderful phenomena, which, though inherent in the constitution of man, and in the relations of his mental and corporeal nature, are not familiar to common observation. The feelings are thereby excited even more than the reason ; and belief is hurried on, and results accredited, with little care for the sufficiency of proof, or knowledge of the many facts which otherwise explain, or contradict, the conclusion. However earnest the desire for truth, imposture rarely fails to mix itself with inquiries so conducted ; and increases the practical evil which always more or less results from error. Every philosophical physician is bound to watch over these events as they pass before him ; never refusing inquiry, because what is put forward is new or strange ; but requiring evidence in proportion to the unusual character of the facts ; sifting closely that which is offered, and rejecting all conclusions not founded on this basis." (pp. 9, 10.)

In a note to this passage, Dr. Holland adverts to the great diversities which prevail in the intellectual constitution of man, as regards the readiness with which different individuals allow themselves to be convinced,—one man concluding upon proof, which to another has neither weight nor pertinency,—one mind pursuing a subject throughout all its relations, whilst another follows but a single line to the result. We have often had occasion to observe, in reference to this subject, how remarkably the adage is frequently verified, that " extremes meet." It is an old observation, that complete scepticism in religion is often allied with extraordinary credulity in other matters ; and we have a remarkable verification of this at the present time, in the authors of ' Man's Nature and Development,' of one of whom it has been not unaptly or untruly remarked, that " she will believe anything that *is not* in the Bible." This relation is frequently exemplified in the sudden conversion of the utterly incredulous opponent of Mesmerism, who begins by maintaining it to be a tissue of humbug, delusion, and imposture, into the thoroughgoing advocate, whose motto seems to be, *credo quia incredibile est*,—for whom no prodigy is too marvellous, no impossibility too staggering. No sooner does such an individual become convinced that there is " something in it," than he yields himself entirely and indiscriminately to the seductions of the novel and mysterious phenomena which are presented to his admiring attention,— the very same deficiency in the power of critical analysis, which made him *reject* everything in the first instance, causing him to *swallow* everything in the second. Our own observations have led us to the conclusion, that the men of candid, discriminating, philosophical minds, who are really qualified by intellectual ability, and by the habitual subjection of their feelings to their sense of right, to form an accurate judgment in matters of this kind, are very few indeed. And it happens unfortunately for the interests of true science, that the men of this class are usually of a temperament which indisposes them from putting themselves prominently forwards, to take the inquiry out of the hands of those who have hitherto conducted it ; and who shrink from coming into collision with the violence and vulgarity, which is alike manifested by the advocates and by the opponents of the doctrines in question. Yet it must be by the verdict of the philosophical inquirers, that the question must ultimately be decided ; and we think that the present aspect of Mesmerism is one which strongly calls for a full, fair, and searching investigation, with all the assistance

which can now be afforded to the inquiry, by the study of those remark-
able states, with which the public has been of late familiarized (not, how-
ever, that we mean to aver that everything is *real* which has been exhi-
bited by itinerant performers—far from it), under the designations of
Electro-biology and Hypnotism. For it is by the thorough examination
of those peculiar states, which are capable of being artificially induced,
without even the semblance of any external agency or influence, that we
may best hope to learn what is true and what is false in Mesmerism; to
eliminate all that may be probably or even possibly attributed to the
influence of *suggestion,* acting through the conscious mind of the subject
of the experiment; and thus to separate the phenomena, if such there be,
which are inexplicable by any known agency, and which must be held to
prove the existence of powers hitherto unrecognised by scientific psycho-
logists, physiologists, or physicists.

Dr. Holland's chapter on the *Effects of Attention on Bodily Organs,* is
one which has received more notice than perhaps any other; and quite
deservedly, since it affords the clue to the investigation and explanation of
a vast number of those phenomena, of which it has hitherto been consi-
dered most difficult to afford a satisfactory solution, and which have
accordingly been referred to the undefined categories of "faith," "imagina-
tion," &c. In his present reproduction of it, we find many valuable
additions; and from these we shall first cite a passage, in which he more
distinctly defines the state of mind to which he refers, than he had pre-
viously done.

" Before entering on the subject, however, I must premise a few words as to
this act of mind, which we designate *attention;* a term apparently needing no
definition, yet which is open to some difference of interpretation, depending on the
different functions, of mind and body, with which it is associated, or by which it
is brought into action. The phrase of *direction of consciousness* might often be
advantageously substituted for it; but here again the same explanation is required
as to the especial manner in which the consciousness is thus called into exercise.
Limiting the question, as we do at present, to the effects on the bodily organs, we
have to note the several distinctions between attention directed to any part by
express effort of the will—attention solicited by sensations derived from the part,
and independent of all volition—and attention suggested or excited by some
mental state, having reference to the part, but in which the will is equally uncon-
cerned. These cases, though ever graduating into one another, as is common
with all mental phenomena, yet are essentially distinct; and even where scarcely
divisible to observation, do still severally modify, more or less, the results in which
they concur. The function of the will, exercised in the first case, and only par-
tially, or not at all, in the others, best defines the separation, and has greatest
influence on all the phenomena." (pp. 14, 15.)

We are not sure that we rightly understand our author's meaning in
the last clause of the preceding quotation. That which seems to us to have
the greatest influence on the result is, not the *voluntary* direction of the
attention, but the state of *expectation* of a certain result, which, when the
mind is completely absorbed in the contemplation of it, seems to act
as an independent originating power. It is, as it appears to us, when
the voluntary power is in the most complete abeyance, that this state of
expectant attention (to use a phrase which has long been familiar to us,
and which Dr. Holland elsewhere employs) produces the most powerful
effects, both upon the animal and the organic functions. And these effects

may be produced, even where the will is exerting itself to antagonize them. Of this we cannot have a better illustration than that which is afforded by the " swing-swangs" which have been a fashionable amusement during the last twelve months, among those who had not the wit to see through the fallacy of their results. If a man suspends a button by a string from the end of his finger, and attentively watches its vibrations, with the conviction that they will assume a definite direction, they will presently do so, notwithstanding that he exerts his utmost voluntary power to prevent any movement of his hand. If, then, another person lay hold of his disengaged hand, or he take into it a homœopathic globule, or a piece of metal be placed beneath the button, or any other change be made which *leads him to expect* an alteration in the direction of the vibrations, *such alteration will take place,* quite independently of his will, which may be exerting the same resisting power as before. It is a manifestation of the very imperfect analysis which is commonly made of such phenomena, that from the moment when they are found to be referrible to a physiological principle, instead of demonstrating, as they were at first supposed to do, the existence of a new force, they seem to lose all their interest for those who had previously watched them with eagerness, and to be set down as illusory, or as the product of the imagination, notwithstanding that they are as *real* in the one case as in the other. From the time when these phenomena were brought under our notice, we have steadily maintained that they are referrible to the principle of *expectant attention;* the fixed idea that a certain result would ensue, being sufficient, in those individuals whose minds can be brought to entertain it, to produce those insensible movements of the hand which bring it about; so long—that is—as the eyes are fixed upon the vibrating body, and the requisite *guiding sensations* are obtained through them. And it has afforded us no little amusement to watch the various stages of belief in many of our friends,—commencing with complete incredulity, and ending with the full reception of the odylic force which these experiments were supposed to demonstrate,—and then to be able to upset the whole fabric, by the simple proof that these results are entirely due to the *mental* state of the performer, that they depend upon his constant watching of the vibrations, which lose their regularity as soon as he takes his eyes from them, and that *any* change which he can be led to expect may be thus induced, whether the appropriate physical influences be employed, or not.* But the facts are not, when thus accounted for, one whit the less curious and interesting; and they afford one of the best possible proofs, that a state of mind which is neither volitional nor emotional, but which simply

* See the account of Dr. Henry Madden's experiments on Mr. Rutter's magnetometer, at Brighton, in the Lancet, for Nov. 15, 1851.—We are not without hopes that Dr. Madden's good sense may lead him to inquire, whether the operation of homœopathic globules upon the living body is not due to the same principle as that which he found to govern the movements of his suspended button—namely, the state of expectant attention in the minds of those who employ them. We think that a few experiments, candidly and carefully made, will lead him to suspect that such may not improbably be the case. On this point Dr. Holland well remarks : " We may reasonably refer to the same principle some of the alleged facts in homœopathy ; such as the long train of symptoms, sometimes amounting to hundreds, which are catalogued as proceeding from infinitesimally small quantities of substances, inert or insignificant in other manner of use. The attention urged to seek for local sensations has no difficulty in finding them. They generate one another ; and are often, as we shall afterwards see, excited by the mere expectation of their occurrence." (p. 20.)

consists in its complete engrossment by an *idea,* may express itself in muscular movements, which must be accounted *automatic* in their character, although originating in the cerebrum, not in the spinal axis. To the actions of this class, Dr. Carpenter has recently given the name of *ideo-motor;* and has shown that they constitute a group which must take rank in physiology above the *sensori-motor,* as these last do above the *excito-motor;* the cerebrum, the sensory ganglia, and the spinal cord, being the instrumental agents in these groups respectively.* This view was extended by Dr. Carpenter to *all* those states of mind, in which the voluntary control over the current of thought is suspended, and in which the mental state is determined entirely by the re-action of the mind of the individual upon external impressions,—the states, namely, of dreaming, somnambulism, hypnotism, electro-biology, &c.; and he showed how the various phenomena of these states, strange as they at first sight appear, are reducible to this one simple principle, the possession of the mind by *a dominant idea,* of which it has no power to divest itself, and of which all the bodily actions performed during its continuance are the expressions. Although Dr. Holland is less explicit in his statements, yet it is obvious that this is also his view; and some of his most valuable additions to this chapter relate to the influence of the will in directing the current of thought, and to the automatic character of the mental operations when this power is withdrawn or suspended.

" In speaking of the automatic, however, as opposed to the voluntary power, we must not limit the former to what are commonly termed the parts and functions of organic life,—those which depend on the spinal and ganglionic systems, and the direct and reflex actions therewith connected. We can hardly apply any other term to those states of the sensorial organs, in which there is the loss, partially or completely, of voluntary power over the ideas, images, or creations which successively traverse the mind; as well as of the influence of the will over the voluntary muscles of the body. The rational governance of these acts is enfeebled or gone; and the influence of organic or material causes comes in, producing those vague and inconsequent trains of thought and imagery, which we have just denoted as strongly expressed in the states of sleep and disease; but which are of constant occurrence, in one degree or another, in all the passages of life. In many remarkable cases, the ordinary perceptions from the senses are wholly disturbed and perverted by the condition of the sensorium receiving them. Muscular motions occur from other causes than volition; and past images and memories rise up unbidden to perplex both sensations and acts by mingling with them, without control or direction of the rational will.

" We can hardly go further in deciphering this most obscure part of our nature, present though its effects ever are to our consciousness. But these effects are too rapid in sequence, the changes in the power and direction of the will too frequent and transient, to be submitted to analysis. We recognise the main fact of its limitation by the automatic parts of our structure; we see and feel it to be variously controlled by impressions from without and states of mind within; we observe the influences of these several causes in creating habits of action, mental as well as bodily, in which the power of the will is so far lost that they become almost like the instincts of inferior life." (pp. 28, 29.)

In applying these considerations to the states already alluded to, he proceeds :

" Here the attention, no longer guided,—or if at all, partially and interruptedly, —by the rational will, becomes submitted to the vague and almost automatic condi-

* See the Report of his Lecture at the Royal Institution, March 12, 1852; contained in the Athenæum, May 15, 1852.

tions of the sensorium just described.   The actions upon it from without, and the reflected acts and movements thereby induced, form a very extraordinary page in the history of the human mind.   It is here, in fact, that we must look for explanation of those strange results in animal magnetism, electro-biology, &c., which have served so greatly to perplex all ordinary observers, and even some who come better prepared for the scrutiny of such phenomena.   Without referring in detail to things often described and very generally known, I will at once point out the questions which concern us in the present inquiry ; and which govern, in truth, every other question and argument in relation to the topics just mentioned.   Are these phenomena—admitted by all to be singular and striking—derived from a peculiar agent or influence, transmitted from one human body to another by certain modes of communication ?   Or are they the effects of various external excitements on the sensorium and nervous system of persons of a peculiar temperament, analogous in nature and origin to phenomena with which we are more familiar in sleep, trance, hysteria, and other forms of cerebral or nervous disorder ?

" These questions, involving the very reality of the mesmeric theory, must ever be kept before us in all observation or reasoning on the subject.   It is singularly important that this should be done wherever experiment is concerned ; inasmuch as they suggest those particular tests which are essential to complete evidence, but which have been, for the most part, unaccountably neglected.   In putting these questions, moreover, we indicate the absolute need, for the right prosecution of this inquiry, of familiarity with the natural phenomena of health and disease just adverted to.   Without this knowledge, and without the just perception of what constitutes scientific evidence, we might as well be gazing on the feats of a conjuror at a public exhibition, as on those of animal magnetism in similar assemblies." (pp. 30, 31.)

As to what have been termed the "higher phenomena" of Mesmerism, Dr. Holland makes the very just remark that the asserted powers of clairvoyance, prophecy, &c., require a very different kind and degree of evidence for their establishment, than that which is needed to verify those other conditions which are so closely allied to the ordinary changes in health and disease.   The more carefully all those phenomena of mesmerism are examined, which present themselves with any tolerable constancy, as well in the presence of philosophical sceptics, as in that of true believers, the more completely are they found to be really akin to those, which occur under circumstances that demonstrate the *absence* of the very agency for which the Mesmerists contend.   This has been fully brought out by the investigation which the electro-biological phenomena has received from scientific enquirers, especially in Edinburgh ; and upon this point Dr. Holland makes the following observations, with which we fully coincide :

" Another frequent error of belief it is also important to remove.   The phenomena which have been produced under the name of electro-biology, have manifestly close relationship to those of Mesmerism, and are connected by common opinion with the same mysterious cause.   Yet this remarkable class of facts does in fact contradict what we have seen to be the main assumption of the Mesmerists ;—that, namely, of a bodily influence of A. upon B., by which the latter is brought into what could only be considered a new state of existence ; mental as well as bodily, active as well as passive.   The results exhibited by the biologists—analogous in kind and equally striking—are not alleged to proceed from any such mysterious agency ; but come before us fairly as the very curious effect of excitement of various kinds upon certain peculiar temperaments ; and, as such, well illustrate some of the topics under discussion in this chapter, and are reciprocally illustrated by them.   These relations have been ably examined and commented upon ; and proof given that all the more credible results of animal magnetism (and the

experiments of Baron Reichenbach come under the same class) may be obtained from a more natural source than that in which Mesmerists profess to believe." (pp. 32, 33.)

The " higher phenomena," however, seem only to be vouchsafed to those who are already either believers, or who are disposed to become so, or who are in that state of dogmatic incredulity, which, as we have just observed, is closely allied to the unquestioning reception of the whole series of mes- meric marvels. The philosophical sceptics, who require satisfactory evidence of the genuineness of what they see, and who insist upon probing and examining into all possible sources of fallacy, are unfortunately just the very people whose " atmosphere" is unfavourable to the manifestation of the clairvoyant and prophetic powers, just as the neighbourhood of a wet blanket prevents an electrical battery from retaining its charge. And until some neutralizing agency shall be discovered, we are afraid that the Mesmerizers must despair of ever convincing these very troublesome cus- tomers of the reality of the alleged phenomena; since, whilst they are ready to receive upon the testimony of others such facts as are conformable to their previous knowledge, they assert their right to decline admitting as of equal cogency, the testimony of the same individuals in regard to phenomena which are altogether beyond their own experience, or in complete contravention of it. Thus, for example, we have always felt ourselves fully justified in refusing to believe that Mr. Lewis, the negro Mesmerizer, lifted a man *off his feet* by the magnetic " traction;" notwithstanding that no less eminent an authority than Professor Gregory assures us* that this experiment has been satisfactorily performed. All that Professor Gregory himself saw was, that " when Mr. Lewis stood on a chair, and tried to draw Mr. H—— without contact from the ground, he gradually rose on tiptoe, making the most violent efforts to rise, till he was fixed by cataleptic rigidity;" a phenomenon by no means surprising when properly interpreted. But, naïvely continues Dr. Gregory, " *Mr. Lewis said,* that had he been still more elevated above Mr. H——, he could have raised him from the floor without contact, and held him thus suspended for a short time, while some spectator should pass his hand under the feet. Although this was not done in my presence, yet the attraction upwards was so strong, that I see no reason to doubt the statement made to me by Mr. Lewis, and by others who saw it, that this experiment has been successfully performed." And he goes on to assure us that he *did* himself see " this subject kept by Mr. Lewis's influence for some time in a position leaning backwards, in which he could not have remained for a second without falling, in his usual state, and in which he instantly fell, when Mr. Lewis's influence was removed." Now what says Mr. Lewis himself on this point ? Having subsequently visited Aberdeen, where his pretensions were systematically investigated by several of the professors of the University, Mr. Lewis *distinctly disclaimed* the possession of any such power as that attributed to him by Professor Gregory, and stated " that he could only influence a person lying on the ground so as to make him start up, though others were endeavouring to hold him down."† What, now, are we to think of Dr. Gregory's

* Letters to a Candid Inquirer, pp. 351, 2.
† See Dr. Redfern's valuable statement of " Examination of Mr. Lewis's Experiments on Mes- merism," in the Edinburgh Monthly Journal, Feb. 1852.

expression of belief? and what credit can we attach to any one statement in his book, that goes beyond the bounds of our ordinary experience?

We must not dwell any longer on this topic, but must pass on to notice briefly the contents of the first of Dr. Holland's new chapters, "On Mental Consciousness, in its Relation to Time and Succession." The question which he proposes for discussion is as follows:

"Is our mental existence, as interpreted by consciousness, best viewed and understood as a series of acts and states, single at the same instant of time, succeeding each other with more or less rapidity of change, but in absolute and unbroken sequence?—or as a wide and mixed current, in which various sensations, thoughts, emotions, and volitions do actually coalesce and co-exist as to time, and are simultaneously testified to us by this common consciousness?" (p. 48.)

This subject, he justly remarks, has not yet been so clearly defined or explicitly treated as its importance merits:

"No adequate attention has been given to the singleness or exclusiveness of particular acts of mind—or to the rapidity of their succession—or to the conditions which produce and govern their change of state—or to the influence of the will, as the most important of these conditions. Yet each one of these points involves conclusions of great interest; and though the nature of the subject, which becomes a sort of analysis of mental existence, forbids the hope of certainty in such conclusions, yet is the approximation sufficient to warrant full inquiry. Whatever the power of comprehension of the mind at each instant of time, it is clear that there is a limit to the number of objects coexisting to the consciousness. How near this limit approaches to unity can never, perhaps, be proved or defined; but we may proceed far in the direction towards it, with constant reference to Time as one of the most important elements in the question." (pp. 49, 50.)

After pointing out the mode in which the enquiry must be conducted, he applies it, first, to the investigation of the sequential perception of sensations from without, and then to that of intellectual operations and emotional states; and arrives at what we believe to be the undeniable conclusion:

"That the consciousness, which gives identity to our mental existence, consists in a series of states incessantly pressing upon one another—from causes, and under conditions, which are in part external to ourselves, in part depending on the operations of the mind itself; but all so far subordinate to time, that the further we analyse them the more do we abridge their probable duration, and reduce them to a more single and exclusive form." (p. 60.)

He then goes on to what we regard as the most important part of the whole enquiry—viz., the power which the will possesses, of determining and controlling the succession of these states of consciousness; a power which, he justly says, "is very various in degree, but which, in its full possession, and in due exercise, involves all the highest intellectual attainments of which man is capable." In a subsequent page, Dr. Holland extends this statement to the moral part of Man's nature, affirming that his power of self-elevation depends upon the exercise of this control over the succession of his ideas. In this, too, we fully accord with him; having been long since assured, by the analysis of our own consciousness, that the influence of the will in the direction of the conduct is chiefly, if not solely, exerted, in *modifying the relative force of the motives* which are the real springs of action, by fixing the attention upon some of these, so as to keep them in a strong light before the mind's eye, whilst it withdraws the attention from

others, and thus virtually throws them into the background,—just as Dr. Holland well shows that it can do with regard to external objects of sensation.   When a man is struggling with a temptation, and the motives to good and the motives to evil are nearly in equilibrium, like weights in the two scales of a balance, it does not appear to us that, as some would affirm, the will, acting as an independent power or additional motive, makes either scale preponderate at its bidding; but rather that it imparts an augmented gravity (if we may so express ourselves) to the weights on one side, by directing the attention to their value, whilst it diminishes the force of those on the other, by preventing the mind from occupying itself about them. And it is thus that " second thoughts" so often come to be best ; and that haste in action so often leads to a wrong course.   The disparity in the degree in which this power is possessed by different individuals, is a very common source of diversity, both in mental action, and in the conduct of life which proceeds from it.   Thus, as Dr. Holland remarks :

"We every day meet men whose conversation is made up of rambling incongruities ; who can hold to no subject consecutively ; and who seem, and actually are, incapable of regulating the series and association of their ideas.   Such minds are a curious subject of study ; and often yield more to examination than those higher intellects which have gained, either from nature or exercise, the dominion wanting to the former. An argument with persons thus deficient,—fruitless, probably, in every other respect,—becomes a sort of analysis, by which we can discover the sudden and strange ˙aberrations of thought, the faulty associations, and the disturbances from external impressions, which, unconsciously to themselves, perplex their whole intellectual existence."   (pp. 66, 67.)

There is another phase, which our author does not notice; that in which there is a full—perhaps even an extraordinary—measure of intellectual power, but a want of that determining control over it, which is necessary to keep it steady to any fixed purpose.   Of this combination we find a typical example in Coleridge, who might be almost described as a thinking machine that would run on automatically to an unlimited extent when once wound up and set going, but who yet possessed scarcely any power of self-direction, either as regarded the course of his thoughts, or the actions of his life.

The practical conclusion of this chapter is most important, and cannot be too strongly or too early impressed on the mind of every one. The discriminating practitioner is so constantly witnessing the lamentable results of the want of early acquirement of habits of self control, that he cannot but regard it as one of the most important objects to be kept in view in the education of the young; no amount of intellectual acquirement being comparable in value with that power of self-direction, the possession of which constitutes, in our apprehension, the most essential difference between the psychical nature of Man and that of the more rational among brutes, and gives to him such ability as he possesses of striving to attain a higher elevation :

"Can this voluntary power over the course and succession of mental states— thus varying in different individuals, and limited in all,—be exercised and cultivated in such way as to enlarge its scope, and give it greater energy to resist the causes which control it ?   Experience answers at once, and unequivocally, that it may be so.   The faculty in question is given us not merely to use, but to educate and exalt.   It is eminently capable of cultivation by steady intention of mind and habitual exercise ; and, rightly thus exercised, it becomes one of the highest per-

fections of our moral and intellectual being. By no quality is one man better distinguished from another, than by the mastery acquired over the subject and course of his thoughts—by the power of discarding what is desultory, frivolous, or degrading; and of adhering singly and steadily to those objects which enlarge and invigorate the mind in their pursuit." (pp. 67, 68.)

In the chapter *On Time as an Element in Mental Functions,* we do not find much additional matter. The following observation, however, is one of much interest; and many of our readers will be able, we doubt not, to confirm it from their own experience.

"In many cases of affection of the sensorium—as in the progress of recovering from apoplectic seizure, or generally in cases of partial coma,—a certain and often considerable time may be observed to elapse between a question asked of the patient and his reply. And this seemingly without any uncertainty as to the answer to be given, or any apparent fault in the act of articulation, except slowness and greater effort—but, rather, as if the mind received the perception more tardily than is usual or natural—or more slowly put itself into action through the external organs in reply. Occasionally, though aware of the fact from former experience, I have been led by the length of the interval to ask another question before the first was answered; this answer following afterwards, as if no such second question had intervened. Several cases I have noted where a full minute has passed before the organs were put into motion for articulate reply." (p. 70.)

The chapter *On Sleep* is one that is peculiarly fertile in valuable suggestions; and we attach particular importance to those that have reference to the doctrine which it is Dr. Holland's main object to enforce and illustrate—namely, that sleep must not be regarded as "a unity of state," but is "a series of fluctuating conditions, of which no two moments perhaps are strictly alike," "these variations extending from complete wakefulness to the most perfect sleep of which we have cognizance either from outward or inward signs." It is surprising, as Dr. Holland justly remarks, that this fact, brought constantly under notice by the most familiar experience, should be so little regarded in all common reasoning on the subject. The ordinary phenomena of that transition from the waking to the sleeping life which occurs every day of our lives, pass unheeded on account of their *naturalness;* and we satisfy ourselves with the result of the various influences which tend to produce this transition, without studying their *modus operandi.*

"But let it happen," continues our author, "that similar conditions are produced by mesmeric passes or other similar means, and the phenomena are looked upon with astonishment and awe. The deep interest which rightly belongs to sleep in its ordinary state, is excited for the first time by the unwonted manner in which it is brought on; and a great function of our nature, ever open to rational enquiry, is thus mystified and obscured by the manipulations of art." (p. 83.)

The so-called mesmeric sleep, he justly remarks in another part, does not differ so much from some forms of natural sleep, as these differ among each other; "the intermediate gradations express that general law of continuity which pervades and explains all these phenomena." And, with ourselves, he attaches great value to the phenomena of hypnotism,—the peculiar sleep induced by the fixed gaze at an object so closely approximated to the eyes as to require a considerable convergence of the optic axes, as was first discovered by Mr. Braid,—and to those of electro-biology, which is a state of reverie rather than of sleep, brought on by the same

fixed gaze at an object more remote.   Observation of such states fills up
the lacuna that would otherwise exist; and shows us how close a relation
there is between the phenomena which present themselves in these various
gradations, and the modes in which they are respectively induced,—a rela-
tion which may be legitimately extended also to the mesmeric sleep, and
deprives it of all that in the eyes of the indiscriminating and uninformed
enquirer, most strongly indicates a power brought to bear upon the subject
of it by some external agency.—The whole of this chapter is well worthy
of attentive study; as is also that which succeeds it, *On the Relations of
Dreaming, Insanity,* &c.   From this, however, we shall only extract the
following passage, in which Dr. Holland notices the illustration which may
be found of the phenomena of dreaming and insanity, in the waking moods
of our minds.

" Dreams appear inconsecutive in the series of impressions and thoughts which
compose them; and are so, in fact, in different degrees, according to the varying
condition of sleep.   But let any one follow with consciousness or immediate recol-
lection the ramblings and transitions of the waking state, when the mind is not
bound down to any one subject, and no strong impressions are present to the
senses—and he will often find these no less singular, abrupt, and rapid in change;
though the effect of such irregularity is here subordinate to certain regulating
causes, which are absent during sleep.

" The admission of external sensations is amongst the most important of these.
Their influence in correcting aberrant trains of thought is marked in numerous
familiar instances; still more remarkably when causes of actual disorder are pre-
sent.   A person on the verge of intoxication feels confusion of thought rapidly
coming on him when he closes his eyes, which is lessened or removed when opening
them again; and such alternation may repeatedly occur.   A patient under low
rambling delirium will often pause from this when a question is asked him, or when
any distinct impression is made on the senses; relapsing almost instantly again into
the same state.   Examples of this kind show how slight the line is, if line there
be, which separates the healthy actions of mind from those of morbid nature.
(pp. 125, 126.)

The title of Dr. Holland's chapter *On the Memory, as affected by Age
and Disease,* includes, as he remarks, some of the most curious phenomena
which come under the notice of the physician.   But the subject, both in
its physiological and in its psychological aspects, is one which seems
almost to baffle enquiry, so strange and diverse are the changes which this
faculty undergoes.   Dr. Holland's main object, here as elsewhere, is to
connect the perversions which we see it to undergo from disease or
accident, with its normal operations; and he points out many very inter-
esting relations of this kind, which are well worthy of careful consideration.
He draws an important distinction near the outset, which we have our-
selves frequently had occasion to make, but which has been very com-
monly overlooked, between *simple memory,* and the act or faculty of
recollection,—between the mere assemblage and aggregation of materials in
the mind, and the power of recalling and combining them by a voluntary
effort.   It seems probable that no change ever takes place in our con-
sciousness, without leaving an *impression* behind it, which may be revived
at some future time, however remote, either by suggestions independent of
the will, or by a voluntary act of recollection (which we believe to be
nothing else than a revival of the desiderated impression by suggestions
purposely brought to act upon it), or, it may be, by the return of a certain

set of physical conditions which act more directly than through the channel of suggestion, as in the delirium of fever. And that our recollection of even the most familiar things is due to the power of voluntarily directing our thoughts, is most curiously shown in the (so-called) " biologized" state, in which the subject, on being assured that he cannot remember the most familiar thing—his own name for example—loses his power of recalling it, through the very conviction which for the time possesses his whole mind, of the impossibility of the act. Now, this is only an intensified condition of a state of which almost every one must be occasionally conscious, in which the anxious doubt of success itself becomes the cause of failure, because " it interrupts by its presence those trains of association upon which recollection depends;" whilst " restored confidence repairs the failure, by excluding this cause of disturbance, and enabling the mind to concentrate itself again upon its object."

In the chapters *On the Brain as a Double Organ*, and *On Phrenology*, we find so little that is new, that we need not stop to comment upon them. That *On Instincts and Habits*, on the other hand, being entirely new, and presenting the subject in an aspect which is in many respects novel, seems to call for an extended notice. Our limits, however, do not enable us to say more, than that the subject is treated in a thoroughly philosophical spirit; a broad basis being laid in a comprehensive survey of the phenomena; and the relations between the different groups of these being pointed out with that discrimination and sagacity which are so abundantly displayed in Dr. Holland's other writings. The essential correspondence between the purely instinctive actions of animals, and the various gradations of automatic action in man, is the point upon which he most dwells.

A large portion of the last chapter, *On the Present State of Inquiry into the Nervous System*, is altogether new; and much of what has been transferred from the volume of which the chapter originally formed part, has been rewritten, in accordance with the present state of our knowledge. Of the new matter, a considerable proportion bears upon those *dynamical* relations, which the progress of enquiry seems to indicate that nerve-force possesses, on the one hand, to the physical forces, and on the other, to mental agency: this is a question, however, which we propose to consider in our next number; and we shall therefore postpone until then any notice of Dr. Holland's cautiously-expressed but very suggestive views.

In taking leave of our author for the present, we again tender him our thanks for this very seasonable contribution to a department of enquiry, which no one possesses such opportunities of promoting, as the physician who is prepared for it by his previous studies and habits of thought; and which no physician with whom we are acquainted, has shown more ability than himself to pursue.

PART SECOND.

Bibliographical Notices.

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Art. I.—*Pathology of the Human Eye.* By John Dalrymple, F.R.S.,
F.R.C.S.   Fasciculi viii. & ix.—*London*, 1852.

The satisfaction with which we should have announced the completion of
this unrivalled work, is overclouded by the regret which we feel, in com-
mon with all who were acquainted with its distinguished and estimable
author, at his early decease.   For some time previously to his death, he
had performed his professional duties with increasing difficulty, in conse-
quence of severe pulmonary affection; but he still laboured in his vocation
with zeal and success, and has left behind him a reputation, acquired within
a comparatively short period, such as most men would consider it an ample
reward to have attained in a long life.   That his practice was based on a
thorough knowledge of the anatomy, physiology, and pathology of the
eye, we need scarcely inform our readers.   His treatise on the " Anatomy of
the Human Eye," published in 1834, was the first work specially devoted
to that subject, that was at all worthy of it, in the English language ; and
it still remains the most complete and comprehensive.   And of his intimate
acquaintance with the pathology of the organ, no better evidence could be
afforded, than that which is contained in the work now brought to a conclu-
sion.   Among the benefits which Mr. Dalrymple has conferred upon
ophthalmic practice, it has not been among the least that he did much to
keep it from the hands of those who would have carried it on upon an
empirical basis, and to maintain its place as a branch of surgical science and
art.   Himself the son of a distinguished provincial surgeon, and thoroughly
educated in the whole range of surgical acquirement, he constantly kept
before his mind the fundamental relations which ophthalmic surgery bears
to every other branch of practice, all being offsets from one common stem ;
and although his labours were latterly restricted to this sole department, it
was simply because his time was so fully occupied in it, that he had none
to spare for any other.   His professional position as a *surgeon*, and not a
mere *oculist*, was honourably recognised by his election, not many months
before his death, to the Council of the College of Surgeons.
  The *first* plate of the Eighth Part is chiefly devoted to the illustration of
Strabismus; a convergent and a divergent form of that complaint being
represented.   With regard to the operation for the cure of this affection, Mr.
Dalrymple remarks that the failure in a large number of the results of its
performance has caused it to fall into undeserved discredit; but that the
impossibility of anything like a certain prognosis of success, in any

individual case, should prevent a surgeon from undertaking it without a full explanation to the patient (and to the parents, in the case of a young subject) of the possible consequence of failure. In his own practice, he never operated on children under fourteen years of age, but recommended that a trial should be given to the constitutional and special treatment by which the affection is sometimes cured or ameliorated. The other subject of this plate is Ptosis, which is dependent upon a partial paralysis of the third pair of nerves, whose existence is usually indicated previously by some degree of palsy of the iris and of strabismus divergens.—The *second* plate is intended to illustrate palsy of the facial nerve; but it is, we think, less happy than most of Mr. Bagg's delineations; since, until we understood its meaning, we conceived it to be of a very different nature, the paralysed side being the one that *appears* in the most natural state. The subjects of the third and fourth plates are somewhat miscellaneous. Three figures of the *third* plate are occupied upon cysts of the anterior chamber, whose occurrence is by no means rare, although true entozoa so seldom present themselves in that situation, that Mr. Dalrymple states that he has never witnessed a case of the kind. Two such cases have been seen and described by Dr. Mackenzie, of Glasgow; and we have ourselves met with another. The fourth figure represents a singular form of Chemosis, due to deep-seated inflammation of the areolar tissue of the orbit, one of the most painful and even dangerous diseases to which the organs of vision are liable; of this disease, Mr. Dalrymple narrates two striking cases, in one of which the eye was saved, whilst in the other it was very rapidly destroyed. And the fifth figure very faithfully delineates that change of position of the globe of the eye, known as Exophthalmos, in which it is thrust forwards by the pressure of tumours &c.—In the *fourth* plate, conical cornea is the first subject represented, or rather attempted, it being "particularly difficult," or rather, we should say, impossible, "to convey in a drawing the very peculiar aspect presented by a living specimen of this disease." Another figure more successfully portrays a form of that curious affection, Microphthalmus, in which, instead of a defect of size of the entire globe of the eye, there was deficient development of the cornea and iris in proportion to the rest of the organ, the sclerotic being prolonged considerably over the margin of the cornea, so as almost to give the appearance of arcus senilis. The opposite condition, general hypertrophy of the globe or Buphthalmus, is next illustrated; this differing from Hydrops oculi, with which it has been confounded, in the absence of disease or of alteration in the normal relations of the parts of the organ. And lastly, we have two figures of Coloboma iridis; one representing the most common form, in which the pupil is a perpendicular slit in the iris, resembling that of a cat; whilst in the other, which is a rarer malformation, the pupil is elongated in the horizontal direction, as in ruminant animals.

Of the Ninth and concluding Part it is but right for us to state, that its entire material was prepared before Mr. Dalrymple's death; the plates having been already lithographed and in the hands of the colourer, whilst the letter-press was in possession of the printer. And it is no small matter of congratulation to the publisher and the purchasers of this admirable and beautiful production, that such was the case. This fasciculus is entirely devoted to the *Malignant Diseases of the Eye;* which are delineated with a

beauty and accuracy that we venture to affirm have never been surpassed,— we much doubt if they have been equalled. The forms of cancer described by Mr. Dalrymple as affecting the eye, are medullary sarcoma or fungus hæmatodes, melanosis, and carcinoma or scirrhus. He has never seen colloid cancer in or about the eye; and considers osteo-sarcoma as probably only a modification of medullary cancer, dependent on the nature of the tissue implicated. Of all forms of this frightful malady, he states that the white medullary fungus is the most common: this, in the great majority of instances, affecting children between two and ten years of age, but adults not being entirely exempt from its ravages. On the contrary, true carcinoma never presents itself in the very young, and rarely until after the middle period of life; and melanosis, though occasionally seen in early adult life, is more common towards fifty or sixty years of age.

Mr. Dalrymple has had the opportunity of watching a large number of cases of medullary sarcoma; and the result of his experience is most unfavourable to operative interference. He has never known an instance in which the disease has not returned after extirpation of the eyeball; and this usually within so short a period, as to make it doubtful if the operation had even succeeded in *prolonging* life. Sooner or later, the disease invariably proves fatal by extension to the brain; as is evidenced by the *post-mortem* discovery of tumours in the course of the optic tract, about the base of the brain, or even upon the surface of the cerebellum, in those cases in which an examination has been made; and by the symptoms preceding death, when such examination has not been permitted. The extirpation of the eye, even in a very early stage of disease of that organ, offers no security whatever against such extension; for Mr. Dalrymple has seen cases in which death took place from fungoid tumours of the brain, whilst as yet the ocular disease was in its very first stage, being limited to the posterior part of the globe, and not having produced any enlargement of the organ. In no case of genuine medullary fungus of the eye, therefore, would he either have performed, or have sanctioned, the extirpation of the eye ball. He subsequently assigns the difficulty of diagnosis, in the early stages of the disease, as an additional reason for the non-performance of the operation at that period; for the metallic lustrous reflection seen at the fundus of the eye, which is almost always the first marked symptom of medullary cancer, is by no means peculiar to it, this effect of the development of a stratum or of tubercles of cancerous structure in the retina being simulated by a deposit of simple exudation-cells in fibrinous dropsy of the eye, in acute or chronic choroiditis, and in scrofulous inflammation; as was first pointed out by Mr. Travers, whose views on this subject have been fully confirmed by Mr. Dalrymple's observations. There can be little doubt that some of the cases in which extirpation has been considered to have been successfully performed, have been of this latter kind. The *first* plate contains two figures representing the early stage of medullary sarcoma, in which it is as yet limited to the retina; and a third figure illustrating the condition of the eye when the fungous growth has advanced forwards, producing enlargement of the globe, disorganization of its internal structure, and pressure upon the sclerotic coat and cornea, which occasions the former to yield in staphylomatous bulgings, and the latter to slough, forming an opening from which the fungous granulations sprout forth. Three other

figures are from preparations which show the extension of the fungous disease along the course of the optic nerve to the brain, before the globe of the eye had been seriously affected; in one of these cases, it would have been difficult to have pronounced before death that any other than scrofulous disease was present, for the cornea and lens were clear, the iris unscathed, and the white exudation behind presented no appearances that may not be assumed by a fibrinous effusion; and yet not only was the theca of the optic nerve distended with cancerous deposit, but there were numerous fungoid tumours about the base of the brain.   The *second* plate is entirely occupied with a single illustration, and this a singularly faithful and beautiful one, of the advanced stage of this terrible disease; the case was one in which the morbid growth had apparently been entirely removed by the late Mr. John Scott, but reappeared within three or four months; and at the time the drawing was made—about six weeks before the patient's death—it projected from the orbit to the size of a large orange, partly covered by the livid and extended skin of the eyelids, in a manner that has been represented by the artist with a fearful truthfulness.   As the tumour increases in size, its surface softens, and portions of it die and form sloughs, which are again and again detached as brown and dead *débris;* this stage, also, is beautifully represented in the first figure of the fourth plate.   So far from the bulk of the tumour, however, being diminished by this partial destruction of its substance, it continues to increase in consequence of its inherent activity of growth; and the patient dies exhausted by frequently-recurring hæmorrhage, or drained by the discharge, unless more speedily carried off by encephalic disease.

The *third* plate represents a case of scirrhus of the eyeball, occurring in a woman seventy-four years of age.   This form of cancer may commence, not merely in the globe of the eye, but also in the lachrymal gland, or even amongst the cellular and fibrous contents of the orbit.   When the morbid growth is limited to the lachrymal gland, there seems adequate ground for its extirpation; cases having occurred in which the operation has been apparently successful.   And Mr. Dalrymple would not absolutely forbid the removal of the globe when this is involved in the disease, provided that there is no indication of its existence elsewhere; although its return must be seriously apprehended.   No other save palliative treatment can, in his opinion, be productive of anything but an injurious result.   His commentary on this plate contains some interesting remarks on the general pathology and therapeutics of cancerous diseases.   In common with most writers who have recently examined the question in all its bearings, he considers that the malignant cachexia is rather the cause than the consequence of the localized growth; and that the "reproductive law of the cancer-cell is, in all probability, closely connected with the peculiar condition of the constitutional powers of the individual," this condition being more favourable to the multiplication of a low type of cells, than to their development into a higher form.   Hence he considers, that "if the science of medicine should ever so far advance, as to suggest a cure for malignant diseases, it will be in the direction of such agents as shall increase the vital powers."   Unfortunately we are acquainted with no other agents of this kind, than such as exert a general hygienic influence, and can scarcely think it probable that any such will be discovered.   Here, as elsewhere,

we have the means of prevention more certainly within our reach, than those of cure. It is, we believe, a general opinion, that malignant diseases are on the increase; and it behoves us diligently to study the conditions under which they are most prone to develop themselves, that we may determine for them, if possible, what has already been determined in great degree with regard to the nearly-allied group of scrofulous diseases.

The *fourth* plate contains two illustrations of Melanosis of the eye; a form of disease which is regarded by Mr. Dalrymple as distinguished from ordinary cancer (some pathologists considering it as an incidental association of dark granular pigment with ordinary cancer-cells), by its inferior malignancy; its extirpation by operation being unquestionably less likely to be followed by the re-appearance of the disease, than is that of the morbid growths already adverted to. There is also a representation of an orbital tumour, the nature of which was for a long time extremely doubtful, but which is considered by Mr. Dalrymple to have been of a carcinomatous nature. And, lastly, there is a beautiful delineation of a malignant tumour at the base of the brain, which had been the source of Amaurosis. No attempt has been made by Mr. Dalrymple to include illustrations of the last-named disease in his series of delineations; because, as he justly remarks, the objective symptoms are scarcely subjects for the artist, the dilated pupil owing its pathognomonic signification rather to its immobility than to its size, while the peculiar brightness of the organ can scarcely be expressed in a drawing. " Moreover," he continues, " the history of amaurosis has yet to be written; it is among the most difficult subjects in ophthalmic medicine, and well worthy the labour of a life to investigate, explain, and supply a rational and successful therapeia."

In bringing our notice of this work to a close, it is but right that we should make our readers clearly understand what it does, and what it does not, profess to teach. Its author designed it to comprise " selected specimens of those forms of disease of the organs of vision most frequently met with in practice;" and this intention he has fully carried out. Though our own experience has not only made us familiar with all the commoner forms of ophthalmic disease, but has also brought us acquainted with most of the rarer, we cannot say that, with the exception of Cysticercus in the anterior chamber, we have met with a single disease capable of this kind of illustration, which is not here faithfully portrayed. Mr. Dalrymple did *not* intend, however, to make his work a complete treatise on the Morbid Anatomy of the Eye, comparable to that of Professor Von Ammon; and whilst we may regret that no such treatise exists in our language, we still cannot but feel that the plan which he has so ably and satisfactorily carried out, is the one which, in the present state of ophthalmic practice in this country, is likely to be far more beneficial than any other would have been. For we have had many occasions of seeing how erroneous is the diagnosis too frequently made by those who have not specially attended to ophthalmic surgery, and how injurious, either by omission or commission, has been the treatment pursued in consequence of that faulty diagnosis; and as those who have no opportunity of studying these diseases on an extensive scale, may be most effectually aided by characteristic delineations of them, we rejoice that a work now exists

which may be most conscientiously recommended for this purpose. Still it must be borne in mind, that the very best figures and descriptions are far less instructive than the actual observation of the diseases themselves in their successive phases; and no student ought to consider himself prepared for general practice, who has not taken the opportunities which metropolitan or large provincial institutions abundantly afford, of familiarizing himself with the latter.

Art. II.—*The London Dispensatory: a Practical Synopsis of Materia Medica, Pharmacy, and Therapeutics.* By the late Anthony Todd Thomson, M.D., F.L.S., &c., &c. Eleventh Edition. Edited by Alfred Baring Garrod, M.D., Professor of Materia Medica and Therapeutics in University College, London, &c., &c.—*London,* 1852. 8vo, pp. 1230.

The recent alterations in the London and Dublin Pharmacopœias, to which we have directed the attention of our readers on former occasions, of course rendered it necessary that all the works which are principally based upon them should undergo revision; and the re-editing of Dr. Thomson's well-known Dispensatory, the value of which has been attested by the sale of no fewer than ten editions, has been entrusted to Dr. Thomson's successor, Dr. Garrod, who has performed his task in a manner which, in our opinion, reflects great credit upon his judgment, as well as upon his industry and accuracy. We shall extract the chief part of his preface, for the purpose of showing the nature of the alterations which he has made:

"It has been the great aim of the editor to preserve as much as possible of the matter of the author, and only to make such alterations as the changes in the Pharmacopœias and the progress of science have rendered necessary. In order to accomplish this, the arrangement of the work has been preserved, with a few unimportant exceptions. Part I. will be found considerably reduced in extent; but it is believed that the diminution is not due to the removal of any important matter. In the various editions through which the work had passed, much material had gradually accumulated, which the editor considered might be advantageously removed, consisting of speculative discussions on the constitution of matter and other subjects, which, although valuable at the time of its appearance, have now ceased to be so; all that is essential has been embodied in Parts II. and III., or added to the Appendix. In Part II. the important alterations are as follow:—In the first place, any new drugs contained in the lists of materia medica in the new Pharmacopœias have been added. A few of the drugs not now officinal, and which have fallen into disuse, have been omitted. Again, certain articles have been introduced, which, though not officinal, have recently gained considerable reputation as remedies. Little or no alteration has been made in the botanical description or history of the drugs, except where change of name has rendered such necessary. The descriptions of these by the author have long been considered as constituting one of the most valuable portions of the work, and have been freely made use of by various compilers of dispensatories. In describing the composition of the various articles in the materia medica, such additions and alterations have been made as the progress of chemistry has rendered essential; at the same time the whole has been simplified by the omission of matters and discussions which have now ceased to be of interest. In the treatment of the medical properties and use of drugs, but little alteration will be found, little progress or change of opinion having taken place since the last edition was issued; and the editor did not consider himself called upon to give his own opinions, even when differing from those of the author;

it being his aim that the present edition should be strictly a work of Dr. Thomson, and not one representing the editor's opinions on the subject of therapeutics. The character of the work, likewise, which does not profess to enter into much therapeutical detail, prohibits the introduction of all controversial discussions on the action of medicines. . . . . In Part III., which contains all the officinal preparations of the London, Edinburgh, and Dublin Colleges, the greatest alterations have been made, rendered necessary by the appearance of the new Pharmacopœias. As the value of a dispensatory, both to the medical practitioner and also to the dispenser, depends upon its accuracy, the editor has spared no pains to render this part of the work correct, so that it may be appealed to with confidence; and though aware of the great difficulty existing, on account of the discrepancies in names, composition, &c., of various preparations in the three Pharmacopœias, he trusts he will not be found to have fallen far short in effecting his object."

We have every confidence that the present edition of this Dispensatory will be found well worthy of the reception which has been accorded by the profession to so many of its predecessors; and that Dr. Thomson's useful labours in this and other fields will continue fruitful, so long as they shall be as ably followed up as they have already been.

ART. III.—*A Popular History of the British Ferns and Allied Plants, comprising the Club-mosses, Pepperworts, and Horsetails.* By THOMAS MOORE, F.L.S., Curator of the Botanic Garden of the Society of Apothecaries. With Twenty Coloured Plates.—*London.* Square 16mo, pp. 354.

THERE is no group of plants more pleasing than that of Ferns, as regards the elegance of their forms and the gracefulness of their port; and as our British species are neither very numerous nor very inaccessible, and are for the most part very easily cultivated, and do not lose their interest when dried for the herbarium, they present a very attractive object of study for those who seek a healthful occupation which brings them into contact with Nature, and who do not set store either by gaudy flowers or by varied foliage. For the physiologist, again, they have many points of peculiar interest; among others, that it has been from the study of their development, that the sexuality of the higher Cryptogamia, which is now coming to be generally admitted among botanists, was first put upon a tolerably certain basis.—The little work before us is rather addressed, however, to those who collect and cultivate Ferns for their amusement, than to scientific botanists; being one of the series of illustrated popular works on various branches of natural history, which do so much credit to Mr. Reeve's press, and of which we have noticed most of the members as they have successively appeared. The author has endeavoured to familiarize his readers with the plants of which he treats, without entering upon abstruse discussions about specific identity and diversity, or other dubious and debateable matters, on which different enquirers have been led to diverse opinions, and which are only interesting to the advanced student; and for this purpose his work is extremely well adapted, his illustrations being appropriate and well drawn, and his descriptions clear. We might suggest, however, that a few figures of the fructification, more highly magnified than any which the plates contain, would have been useful in explaining the description of its parts which are given in the chapter on the "Structure of Ferns;" and

would have also been, in a merely ornamental point of view, no inappropriate addition to the excellent series which the volume includes, since there are few among our microscopic " objects," that are more beautiful than the *sori* of some of our commonest ferns.

ART. IV.—*A Handbook of Organic Chemistry; being a new and greatly enlarged edition of the ' Outlines of Organic Chemistry,' for the use of Students.* By WILLIAM GREGORY, M.D., F.R.S.E., Professor of Chemistry in the University of Edinburgh.—*London*, 1852.   12mo, pp. 532.

THE new title under which Professor Gregory's ' Outlines' now appears, is fully justified by the altered character of the work, to which, as now enlarged, its former designation would have been inappropriate.   This enlargement is not due so much to the accumulation of minute details, as to the embodiment of general principles, in the development of which the researches of the last few years have been peculiarly fruitful.   The most important of these researches, however, receive their full share of notice; particular stress being laid on those which are suggestive of further progress.

"For example, the brilliant discovery (predicted in 1839 by Liebig) of the new volatile bases homologous with ammonia, by Wurtz, and so admirably followed out by Hofmann in the latter part of his splendid series of papers on the artificial bases; Hofmann's own most recent discovery of the singular class of ammonium bases; the researches of a host of chemists on that striking series of volatile acids beginning with formic acid, and ending, for the present, with melissic acid, and on the oils and fats generally; the continued discovery of new compounds belonging to the methylic, ethylic, and amylic series; the researches of Frankland and Kolbe on the radicals of the ethylic group, and those of Frankland on the compounds of those radicals with metals; those of P. Rénard on compounds of phosphorus with the same radicals; the light shed on the doctrine of substitution by these investigations, and by those of the French school, by Laurent, Gerhardt, Cahours, and others; the admirable monograph on the bile by Strecker; the introduction of hyponitrous acid as a means of oxidizing organic compounds by Pirea and Strecker; the minute study of the products of decomposition of the albuminous compounds, by Bopp, Guckelberger, and others; the many additions to our knowledge of physiological chemistry by Liebig and many other chemists; and the rapid development of the doctrine of homologous series, as a practical guide in research." (p. viii.)

The section on the nutrition of plants and animals, also, has been entirely rewritten on a larger scale; and now constitutes probably the best outline of the subject which has yet been given.   Here, as in other chemical treatises, however, we meet with statements based upon the mere chemical relations of the components of the body, which are by no means correct physiologically; and we feel satisfied that, as Professor Lehmann has most successfully shown, the true import of the chemical changes which take place in the living system can only be made out by the study of its economy,—Chemical action being to be here viewed as in subordination to Vital, rather than Vital as a mere product of Chemical.

Art. V.—*The Natural History of Animals; being the substance of Three Courses of Lectures delivered before the Royal Institution of Great Britain.* By Thomas Rymer Jones, F.R.S., Professor of Comparative Anatomy in King's College, London. Vol. II., with 104 Wood-engravings.—*London*, 1852. Post 8vo, pp. 395.

We must own that we had nearly given up the continuation of this work, as one of those promises, liberally made, which are not destined to be ful-filled. The first volume was published no less than *seven years* ago, soon after the completion of the lectures of which the work professes to give the substance; and its purchasers were led to expect the speedy appearance of the remainder. The delay has certainly not resulted, as in some other cases to which we could point, from the necessity of bestowing a large amount of time on the prosecution of original researches to be embodied in the treatise; for we do not find in this volume the slightest evidence that any such labour has been bestowed upon the subject. In fact, for anything we have met with to the contrary, it might just as well have been published six years ago, without the difference of a word. We are certain that the delay cannot have arisen on the part of the publisher, who would have been only too glad, we feel assured, to bring the work to an early completion. And we are constrained, therefore, to fix the *onus* upon the author, who does not condescend to give any explanation of the breach of his implied though not expressed promise.

Having thus discharged ourselves of the reproof which we feel it a mat-ter of conscience to utter, on behalf of the book-buying public, in any case like the present, we have the more pleasing duty to perform, of saying that this volume fully bears out the character of its predecessor, in regard to its easy popular style, and the number and beauty of its illustrations. It is entirely devoted to the natural history of the higher Articulated animals,—insects, arachnida, and crustacea; and contains a body of well-selected information upon this subject, put forth in a strictly scientific method, but in a form adapted to the general reader.

We trust that Professor Jones will spare us the necessity of again having to qualify our praise with blame, by producing the remaining volume or volumes without further delay.

Art. VI.—*On Animal Electricity: being an Abstract of the Discoveries of* Emil Du Bois-Reymond, *Member of the Academy of Sciences of Berlin, &c. &c.* Edited by H. Bence Jones, M.D., A.M. Cantab., F.R.S., &c.—*London*, 1852. Fcap. 8vo, pp. 214.

The name of M. Du Bois-Reymond is probably known to most of our readers, as that of a zealous investigator into Animal Electricity; but they, like ourselves, may have felt a difficulty in estimating the merits of his experimental results, novel as many of them have been, and opposed as some of them are by the experience of physicists to whom we have been accustomed to look as the greatest authorities on the subject,—we refer espe-cially to Prof. Matteucci. It has been partly because no connected account of his researches has come before us, and partly on account of our uncer-tainty as to their value, that we have hitherto refrained from noticing

them. We have now had the opportunity, however, of witnessing some of the most interesting of these experiments for ourselves, and of forming our own judgment of the methods followed by M. Du Bois-Reymond, and of the sensitiveness of the apparatus which he employs; and we believe that we speak, not only our own impression, but that of all who have had the same advantage, when we say, that it is scarcely possible for any such experiments to be conducted in a manner more calculated to elicit truth and to eliminate sources of error. The galvanometer employed by M. Du Bois-Reymond is, we understand, of unrivalled sensitiveness; and its indications corresponded so precisely, in each case, with his predictions, that he fully satisfied us of his complete acquaintance with the conditions of the phenomena which he brought under our observation.

The little volume now before us is a translation, with some additions, of an outline-view of M. Du Bois-Reymond's researches, which has been drawn up by Dr. J. Müller, Professor of Physics in Freiburg. It has the great merit of giving such a comparison of his results with those of his predecessors and contemporaries, that the points of originality and of diversity are clearly brought out; and we have every reason to believe that this comparison has been made without partiality, and with a full knowledge of the facts of the case. The objects which M. Du Bois-Reymond has kept in view, throughout his researches, are the following:—
1. To obtain clear ideas regarding the existence of electric currents in muscles and nerves; 2. To determine whether during motion and sensation any changes in the intensity or the direction of the electric currents occur; 3. If possible, to trace the connexion between the electric changes, and the contraction or sensation which ultimately takes place. "The practical application of these results to pathology and the treatment of disease," as the editor justly remarks, "cannot yet be foreshadowed; but this at least may be said, that direct experiments on the action of electric currents upon the nerves and muscles themselves must precede any clearness and certainty in the employment of electricity as a remedial agent." Of this there can be no doubt whatever; we have no agent which possesses a power of modifying the vital activity of the nerves and muscles, in a degree by any means comparable to that in which electricity can affect it; and yet the results of its therapeutic application are so uncertain, as to baffle any confident prediction of its efficacy,—nothing but a trial of its effects, in each individual case, affording an adequate basis for its use or disuse. And although we may not yet see our way much better than we previously could, yet we feel sure that such researches as M. Du Bois-Reymond's must in the end make the path comparatively clear before us.

The late period of the quarter at which this little book came into our hands, must be our apology for not more fully noticing its contents on the present occasion; we shall return to the subject in our next number; but in the meantime we may remark, that one of the most remarkable of M. Du Bois-Reymond's discoveries consists in his having shown that *nerves* possess an electro-motive power, similar to that which has been long known to exist in *muscles*, and which has been particularly investigated by Matteucci.

We beg to tender our thanks to Dr. Bence Jones for this very seasonable publication, and for the very efficient mode in which he has performed the task which he imposed upon himself. Although such as merely "dip"

into the book may be appalled at the new terms and forms of expression which are copiously scattered through its pages, yet we cannot see that these could have been dispensed with, save by a most undesirable circumlocution; and those who will take the trouble to begin at the beginning, instead of at once turning to the conclusion, will have no difficulty, we feel assured, in comprehending the author's meaning. The descriptions, we should add, are illustrated by fifty woodcuts, illustrating the peculiarities of M. Du Bois-Reymond's apparatus, and of his methods of employing it; so that we are not only able to judge the better of the value of his results, but such as have the means of doing so can also test them for themselves, without those errors which so frequently arise from variations in the mode of experimenting.

ART. VII.—*Class-Book of Botany; being an Introduction to the Study of the Vegetable Kingdom.* By J. H. BALFOUR, M.D., F.R.S.E., F.L.S., Professor of Botany in the University of Edinburgh. With upwards of 1000 Illustrations.—*London,* 1852. 8vo, pp. 357.

THE object of this treatise is " to initiate the student into the study of the structure and forms of the various parts of plants;" and it is limited to Histology and Organography, or " a description of the tissues of which plants are composed, and of the various organs concerned in the processes of nutrition and reproduction." It is, no doubt, a vast improvement upon all the treatises which are constructed according to the old model, and is especially commendable for the beauty and copiousness of its illustrations; but still we cannot help wishing that Professor Balfour had thrown that model entirely aside, and had presented us with a treatise more worthy of the position he holds, in which the whole of the beautiful series presented by the Vegetable kingdom should be treated as a connected whole; instead of devoting nearly his entire attention to the Phanerogamic plants, and pushing aside the Cryptogamia as a sort of troublesome appendage, about which it is necessary to say something.

The recent researches of Hoffmeister have fully confirmed the views which we expressed on a former occasion* with regard to the sexuality of the Cryptogamia, and have established this point almost beyond a doubt; whilst they have at the same time demonstrated such a continuity in the essential nature of the reproductive organs, between the Coniferæ and the higher Cryptogamia, that the line of distinction between the two groups must henceforth be regarded as quite an arbitrary one. We purpose giving some account of these researches in our next number.

ART. VIII.—*The Vegetation of Europe; its Conditions and Causes.* By ARTHUR HENFREY, F.L.S., &c.—*London,* 1852. Fcap. 8vo, pp. 387. With a Map.

THIS compact little volume is the first of a series which Mr. Van Voorst purposes to issue, under the title of ' Outlines of the Natural History of Europe;' and it contains a general *resumé* of the labours which have been most

* Vol. iv. p. 341.

industriously carried on for some time past, by a large number of observers, into the geographical distribution of plants, and the circumstances which modify it,—some of these circumstances being the physical conditions afforded by climatic influences, and others the various modes of transport which bring about the migration of plants, and facilitate their radiation from original or from acquired habitats. These general and special influences are considered in two introductory chapters, which will amply repay an attentive perusal; and the vegetation of Europe is then described, as it presents itself in the ten principal provinces into which this quarter of the globe is marked out by the physical conditions of each part, rather than by actual boundaries.

The treatise, though small in compass, presents a large amount of well-selected information, conveyed in a very pleasing style; and Mr. Henfrey's well-known eminence as a philosophical botanist and a painstaking writer, is a guarantee of the accuracy of his digest.

ART. IX.—*Lectures on Histology, delivered at the Royal College of Surgeons in England, in the Session* 1850-51. By JOHN QUEKETT, Assistant Conservator of the Museum. Illustrated by 159 Woodcuts. *London,* 1852. 8vo. pp. 215.

THIS volume consists of a reprint, with some additions, of the lectures which have already appeared in the 'Medical Times,' and which many of our readers have doubtless perused with pleasure and profit. The first nine lectures are devoted to the histology of Vegetables, which Mr. Quekett has done wisely to introduce into his course ; since not only does the study of the minute structure and growth of plants throw great light upon that of animals, but in much that relates to manipulation and to the right interpretation of the phenomena examined, the young microscopist will find it the best possible introduction. This subject is very copiously treated; and all the points which are of special interest to the human physiologist and pathologist are very fully dwelt on. The histology of Animals is only commenced in this volume; the subjects included in the nine remaining lectures of the course, being the simple fibrous tissues, the various forms of cartilage, and fat.

It is almost superfluous to state, that these lectures are of the highest value, for the clear and concise expression which they give to Mr. Quekett's opinions on the various subjects of which they treat ; and that those opinions, based as they are upon a knowledge of the whole subject to which scarcely any other microscopist can lay claim, are of first-rate authority. The numerous illustrations are for the most part very characteristic, although not possessing the high finish with which we have been of late rendered familiar. And it only remains for us to express the hope, that Mr. Quekett will not follow certain bad examples we could name, in delaying the completion of his work, until those who may have purchased this first part have been driven to other sources of information on the subjects which remain to be treated.

We cannot refrain from expressing the pleasure we have derived from the recent appointment of Mr. Quekett as Professor of Histology to the

College of Surgeons; the Council of which body has done itself great credit by this recognition of the importance of the subject, and of the distinguished ability which Mr. Quekett has shown in the prosecution of histological enquiry.

Art. X.—*Chemical Manipulation and Analysis, Qualitative and Quantitative. With an Introduction, explanatory of the General Principles of Chemical Nomenclature, the Construction and Use of Formulæ, the Doctrine of Equivalent Proportions, and the Preparation and Management of Gases.* By Henry M. Noad, Ph.D., Lecturer on Chemistry at St. George's Hospital. Second Edition, considerably enlarged.— *London,* 1852.   8vo, pp. 431.

This work was originally prepared as part of a general treatise on Chemistry, which was designed to appear in a new series of the 'Library of Useful Knowledge.' The present " new edition" is a *reprint* of the portion which had been previously issued, with the addition of an Introductory Chapter on the Preparation and Management of the Gases, which had been previously somewhat strangely overlooked, and of an Appendix, in which some points of Quantitative Analysis are more fully dwelt on than in the body of the work. The whole has, consequently, a somewhat *patchy* character, which is not diminished by the difference in the style of printing between the introduction and appendix and the body of the treatise, the latter being in double-columns, after the fashion of the well-known series in which it first appeared. We cannot say that the book is by any means complete or satisfactory; but the portion of it which relates to the ordinary manipulations of chemistry will be found very useful to the student who is out of the reach of oral direction; as it enters much more fully into this subject, than do any of the systematic treatises on chemical science.

Art. XI.—*Climate of Italy in Relation to Pulmonary Consumption; with Remarks on the Influence of Foreign Climates upon Invalids.* By T. H. Burgess, M.D., &c.—*London,* 1852.   12mo, pp. 206.

Dr. Burgess justly remarks that the influence of climate upon health and disease is only to be determined by authenticated facts and positive observation; in comparison with which, vague assertions and traditionary fame should cease to exert any influence. In regard to the climate, or rather the climates, of Italy (for the different parts of that peninsula offer a wide variety both in temperature and in every other climatic influence), a great body of information has been furnished by the meteorological reports of Schouw; and his statements form the basis of the enquiries of M. Carrière, whose work on the climate of Italy has acquired a standard value. Dr. Burgess's own experience, acquired during a residence in Italy, is in accordance with the scientific investigations of these eminent authorities; and we are disposed, therefore, to attach great weight to the opinions he expresses, although they are opposed to various notions that are prevalent in this country. We think, however, that, in common with some other writers who take his unfavourable view of the influence of climate on

patients affected with pulmonary disease, he looks too exclusively at the *meteorological* conditions, and does not take sufficient account of the effects of change of scene and of habits of life, and of those *moral* influences which are engendered by free exposure to bright and sunny skies, in the minds of such as have previously suffered from the depression of spirits engendered by a monotonous and almost hopeless confinement.   Although the removal of a patient in whom phthisis has already developed itself fully, may be of little avail, yet our personal knowledge prevents us from entertaining a doubt, that many persons are able, not merely to live, but to enjoy life, in well-selected parts of Italy or in Madeira, who could not bear exposure to our ungenial winters and cold springs, and who would soon find their malady forced into a fatal activity by the *ennui* of a protracted restriction to an artificial atmosphere.

Still we believe, with Dr. Burgess, that vast mischief is done by the faith reposed in popular notions upon the efficacy of change of climate in consumption; and we gladly welcome his work as a valuable contribution towards a more exact appreciation of the subject, both on the part of the public and on that of the profession.   In common with Sir James Clark, he fully recognises the beneficial effects of an appropriate change of climate in dyspepsia, nervous affections, rheumatism, and scrofula.

Art. XII.—*The Physiological Anatomy and Physiology of Man.*   By Robert Bentley Todd, M.D., F.R.S., F.R.C.P., &c. &c., and William Bowman, F.R.S., F.R.C.S., &c. &c.   Part IV., Section 1.—*London*, 1852.   8vo, pp. 208.   With 53 Wood-engravings.

However much we may regret the tardy appearance of the successive portions of this admirable work, we cannot feel that its authors are deserving of the blame which we consider to have been incurred by others, whose cases at first seem analogous; for they set out with the praiseworthy intention of examining all the more important departments of human anatomy and physiology for themselves, so as to give to their descriptions the value derived from original investigation; and they have been unwilling to lower its character by the mere reproduction of the untested statements and doctrines of others, notwithstanding that they soon found that the execution of their plan would require a far larger expenditure of time and labour than they originally contemplated.   It may be said that they ought to have formed a more accurate estimate, in the first instance, of the extent of their undertaking; but we are strongly inclined to believe, that if they had done so, they would not have entered upon it at all, and our much-valued "Todd and Bowman" (as the book is usually designated) would never have come into existence.   We are thankful, therefore, for what we can get; and would only express the hope, that if the increasing professional engagements of the authors should prevent them from themselves completing the work on the basis of their own personal researches, within a short interval, they will avail themselves of the assistance of some of those intelligent workers who have been trained up under their instruction, and will employ them (in the mode in which we know that Professor Kölliker is aided by his pupils) in carrying-out, under their superintendence, those enquiries which they are themselves unable to prosecute.

The present Part is entirely occupied with the organs of digestion, absorption, circulation (including the blood), and respiration.   It is almost superfluous to remark, that it maintains the high character of the preceding portions; but we feel it right to state, as one of the evil results attendant on this mode of publication, that the earlier sheets even of this fasciculus are prematurely old, having been in print before the appearance of important memoirs of at least two years' date, which are consequently not referred to.

Whenever the work shall be completed, we promise our authors a full *review*, in which its peculiar merits shall be carefully and discriminatingly set forth; at present we must be content with thus briefly noticing the appearance of a further instalment.

Art. XIII.—*The Hand; its Mechanism and Vital Endowments, as evincing Design.*   By Sir Charles Bell, K.G.H., F.R.S., L. & E., &c.   Fifth Edition, revised, with Woodcuts.—*London*, 1852.   Post 8vo, pp. 428.

We believe that this work has been among the most popular of the series of 'Bridgewater Treatises,' of which it originally formed one.   And we do not wonder at that popularity, whilst we confine our view to the happy mode in which the author brings down his argument to the most ordinary capacity, and illustrates it by the teachings of the most familiar experience. The truly scientific reader, however, can perceive many blemishes and many omissions; and whilst fully sympathizing with the writer in the pleasure with which he expounds his numerous and well-arrayed illustrations of special adaptation, cannot but feel regret that he should have dwelt so little upon the wonders of that general plan, of which these are only individual cases.   But Sir C. Bell wrote at a time when it was the fashion to consider these two sets of doctrines as antithetically opposed to one another, when "unity of type" was the bugbear of the theologist, and the idea of "design" was no less offensive to the so-called "philosophical anatomist." The two schemes of enquiry are now pursued in mutual harmony, according to the method so ably marked out by Professor Owen; and we have no longer any difficulty in imagining that a general plan may include every kind of subordinate modification, and that the hand of man does not less certainly display a designed adaptation to its varied offices, because the limb of which it forms part may be shown to be the "diverging appendage" of the occipital vertebra.

We cannot but regret, therefore, that this treatise should be republished in its original form; since it is not only behind the present state of anatomical science, but tends to keep up erroneous and prejudiced views in regard to it.   The editor, Mr. Alexander Shaw, seems to have entertained a sort of superstitious veneration for his uncle's work; for he has not done anything to amend or correct it; and the only difference between this and previous editions consists in the introduction of about eighty pages of additional matter, derived from the author's notes to Paley's 'Natural Theology,' and from his 'Animal Mechanics.'   The "respiratory system," of course, retains its place; and no hint is given of the more general scheme of "reflex action," its isolation from which, by Sir C. Bell, was one of his greatest errors, instead of being, as we believe he always

considered it, one of his most important discoveries. And the perpetuation of Cuvier's classification of the Animal Kingdom, which still maintains its place at the end of the volume, notwithstanding the glaring errors which the progress of research has shown to exist in it, is another mark of that indisposition to advance with the times, which seems to have descended from Sir C. Bell to his nephew and editor.   On all these accounts, therefore, we greatly regret the re-issue of the work in its present form; and trust that, should another edition be called for, it may be modified in the manner we have pointed out.

Art. XIV. — 1. *Homœopathy and the Homœopaths.*   By J. Stevenson Bushnan, M.D., F.R.C.P.E., &c.—*London*, 1852.   Fcap. 8vo, pp. 214.

2. *Homœopathy Unveiled : or, Observations on Hahnemann, his Doctrines, and Treatment of Disease.*   By William Perrin Brodribb, M.R.C.S. Second Edition.—*London*, 1852.   8vo, pp. 45.

These two publications have the same laudable purpose in view, that of showing the utter baselessness of the system of Homœopathy, and the extraordinary gullibility or dishonesty of those who practise it.   They are both of them intended rather for the professional than the general public; and are designed to satisfy the minds of those who may be tormented by the doubts suggested by the acknowledged uncertainties of legitimate medicine, or staggered by the confident assertions of success which are so liberally sent forth by homœopathic practitioners, and so eagerly caught up by the public; or, again, to furnish those whose own minds are in no danger of being thus unsettled, with the requisite data, whereby they may be enabled to give a ready answer to those who are teasing them with narratives of wonderful cures, and with challenges to refute the system.   Both are written in a calm, gentlemanly tone, and with an entire freedom from that arrogant assumption of superiority, and that manifestation of contempt for those benighted idiots who hold a different opinion, which disfigures the greater part of the productions of the homœopathic press.   But we would especially recommend Dr. Bushnan's little volume, as containing a much fuller and more searching examination of the subject, than the narrower limits of Mr. Brodribb's pamphlet admit.   And we deem it particularly valuable, as containing, in Hahnemann's own words, an exposition of his system, from which, notwithstanding its alleged perfection, many of his professed disciples have departed just as much as it suited their convenience or policy to do, thereby tacitly admitting its fallacies.   We can scarcely comprehend the state of mind, which, after a careful perusal of Dr. Bushnan's *exposé*, can continue to uphold Homœopathy on any other basis, than its admission of Nature to work out her cure without interference for evil or for good.—We have so recently brought the leading points at issue under the consideration of our readers, that there is no occasion for us now to go over the ground again.

PART THIRD.

𝔓eriscope.

ANATOMY, PHYSIOLOGY, AND ORGANIC CHEMISTRY.

On the Structure of the Liver. By C. HANDFIELD JONES, M.D., F.R.S.

DR. LEIDY and Professor Retzius, with Müller, Weber, and Kronenberg, maintain the existence of plexuses of ducts in the parenchyma of the liver containing the cells in their tubes. Some other anatomists, especially Gerlach, believe the ducts to be prolonged into the lobules of the parenchyma, under the form of mere intercellular passages without walls.

Injections of acetate of lead in saturated solution, thrown into the ductus communis choledochus, produce appearances which seem to confirm the latter view. The author, however, believes them to be fallacious, and that the ducts really terminate, as he has described them in his former paper, by closed extremities, either rounded and even, or somewhat irregular. Further details are given of the condition of the ultimate and penultimate ducts in the several vertebrate classes.

In the class of fishes, the minute ducts most commouly appear as solid cylinders of soft granulous substance, in which scarce anything but some oily molecules are to be discerned; but not very unfrequently two other conditions are observed, which seem to illustrate very well the active character of the function of the duct. In the first the granulous matter exists in much smaller quantity, and the nuclei imbedded in it are consequently seen much more distinctly; their presence is thus unequivocally determined; it is shown that there is no real difference between the ducts of the fish's and those of the mammalian liver, only that the granulous matter is usually accumulated in the former more abundantly than in the latter. The presence of free nuclei in granulous matter indicates an active change to be proceeding in the part. In the second condition sometimes observed, the granulous matter has imbedded in it a varying number of pellucid vesicles of great delicacy, but quite distinct; these testify that a process of active growth takes place in the minute ducts, and show, the author thinks conclusively, that these minute ducts are not mere efferent canals.

Sugar was detected on two or three occasions in the livers of Fishes; it seems to be absent when the organ is extremely fatty.

In the minute hepatic ducts of Reptiles, the condition of the epithelium is very similar to that in fishes; the nuclei sometimes appearing with great distinctness, sometimes being obscured by much granulous matter, sometimes developing themselves into pellucid vesicles. The livers of frogs and toads almost constantly contain dark yellow masses, which were formerly regarded by the author as biliary concretions, but are now considered to be only pigmentary deposits; they coexist sometimes with much diffused black matter.

The ultimate ducts have been traced recently very satisfactorily in Birds, Mammalia, and Man: and the description given of them in the paper accords with the author's former account.

The development of the liver and its apparatus of ducts has been traced out in fishes and reptiles, and the following results obtained in both classes:

(1.) The liver (i. e. the parenchyma of the organ) is formed as an independent mass, and does not proceed as an offset from the intestine.

(2.) The gall-bladder is developed separately as a transparent vesicle, containing a clear fluid.

(3.) The gall-bladder elongates itself at one end, tends towards the intestine, and at last opens into it, while from one part of its extent hepatic ducts are developed; in the frog the hepatic ducts seem, however, to be formed at the same time as the gall-bladder, and to be developed *pari passu* along with it. The cystic duct is lined by ciliary epithelium, which plays very actively.

The examination of the process of development in the chick has confirmed, so far as it was carried, the account given in the former paper.

In mammalia, the subject of enquiry has been chiefly the following—viz., to ascertain how far there was evidence that the secretion of bile actually is effected in and by the hepatic cells, or whether its presence in them is accidental, and the bile is really and necessarily secreted by the ultimate ducts.

It is remarked that the existence of a portal vein conveying blood from the intestinal surface is coeval, not with the formation of a bile-secreting structure (for many animals have organs which secrete abundance of biliary matter without any portal vein), but with the addition of a parenchymatous mass to the biliary organ, to which mass exclusively the portal vein is distributed. It is known that the parenchyma of the liver during, and for many hours after, digestion of food, forms, from the blood supplied to it, abundance of sugar, which thus appears to be its proper secretion; and it is not proved that the hepatic cells in a healthy state contain biliary matter, though they often do in various morbid conditions. Extracts of the hepatic parenchyma tested for bile by Pettenkoffer's method, give only very imperfect and doubtful traces of the presence of biliary matter, and, on the other hand, the sugar formed by the parenchyma, which is found so abundantly in the blood of the hepatic vein, is absent from the bile. The case of fatty liver, as occurring either pathologically or normally, seems also to require an explanation consonant with the view to which the above facts point, for otherwise it seems impossible to understand how perfectly formed dark-green bile could be contained in the efferent channels of a gland whose tissue is a mass of oil.

The structural condition of the ultimate biliary ducts is compared to that of the epithelium of the thyroidal cavities, and the nucleated granular tissue surrounding the lacteal in a villus; and it is shown to be probable that the terminal portions of the ducts, so far as they possess the peculiar characteristic structure, exert an active elaborating energy, by means of which bile is formed or generated out of oily, albuminous, or saccharine material, which surrounds and may be said to bathe them.—*Proceedings of the Royal Society,* Jan. 29, 1852.

[We have thought it right to place Dr. Handfield Jones's views before our readers, as those of a painstaking and conscientious observer; but we cannot help feeling great surprise that he should consider his physiological deductions to be required or even justified by the facts he adduces.]

On the Rhythmical Contractions of the Veins of the Bat's Wing.
By T. WHARTON JONES, F.R.S., &c.

THE author finds that the veins of the bat's wing contract and dilate rhythmically, and that they are provided with valves, some of which completely oppose regurgitation of blood, others only partially. The act of contraction of the vein is manifested by progressive constriction of its calibre and increasing thickness of its wall; the relaxation of the vessel, by a return to the former width of calibre and thickness of wall. The rhythmical contractions and dilatations of the veins are continually going on, and this, on an average, at the rate of ten contractions in the minute. The contractions *centrad* and *distad* of a valve appear to be simultaneous, as also the dilatations.

During contraction, the flow of blood in the vein is accelerated, and on the cessation of the contraction, the flow is checked, with a tendency to regurgitation,

which brings the valves into play. But this check to the onward flow of the blood is usually only momentary; already, even while the vein is in the act of again becoming dilated, the onward flow recommences and goes on, though with comparative slowness, until the vein contracts again. It is the heart's action which maintains the onward flow of blood during the dilatation of the vein, whilst it is the contraction of the vein, coming in aid of the heart's action, which causes the acceleration.

The contractility of the arteries the author finds to be altogether different from that of the veins, being *tonic*, not *rhythmical*. He has not been able to observe unequivocal evidences of *tonic* contractility of the veins, which they have been alleged to possess.

The author further finds that nowhere do the arteries and veins of the web of the bat's wing directly communicate, as has also been alleged; the only communication being the usual one through the medium of capillaries.—*Proceedings of the Royal Society*, Feb. 5, 1852.

On the Structure of the Papillæ of the Cutis.   By Professors Wagner and Kölliker.

In the Gottingen 'Gelehrte Augeige,' for Feb. 2, 1852, Professor Wagner published an account of recent investigations by Meissner and himself into the structure of the papillæ of the skin. The chief results of these investigations (of which we gave a brief statement from the 'Gazette Médicale' in our last number) were :—1. That the papillæ of the palm of the hand and of the fingers are of two kinds, *tactile papillæ* and *vascular papillæ :* the former contain no vessels, the latter contain no nerves.—2. Each *tactile papillæ* contains an oval body, the " corpusculum tactus," composed of a horizontally laminated substance, between the layers of which nuclei are scattered. The corpusculum is invested by a striated finely granular cellular tissue, and externally by the epidermis, which sometimes possesses a basement-membrane and sometimes does not.—3. The principal sensitive nerves divide, as those which are distributed to muscles and electric organs are known to do. They are usually dichotomous or trichotomous, dividing down to the axis cylinder, and then enlarging again so as to have double contours. Each nerve-fibril runs in the subcutaneous cellular tissue, and gives off its branches perpendicularly upwards into the papillæ; these then apply themselves to the corpuscula tactus ; their ends curl round, and the axis cylinder passes out and splits up into branches. The nerves of the papillæ form no loops.—4. In conclusion, Wagner regards the " corpuscula tactus" as peculiar sensory organs, as elastic cushions, for the purpose of transmitting and intensifying tactile impressions to the nerves of touch.

Incited to a re-examination of the subject by the statements of Wagner and Meissner, Professor Kölliker has just published (' Zeitschrift für Wissenschaften Zoologie,' Band iv. heft 1, 1852) the conclusions derived from an elaborate and apparently careful re-investigation of the structure of the papillæ of the cutis in all parts of the body. His results differ widely from those of Professor Wagner. In the first place, he denies that the corpuscula tactus, or *axile bodies* of the papillæ, as he prefers to call them, can be by any means so sharply distinguished morphologically, as Wagner asserts. They are not laminated, but are composed of a mass of homogeneous areolar tissue, with an external layer of imperfectly developed elastic tissue, consisting of ramified cells more or less elongated into fine fibres, with elongated nuclei. The elastic tissue winds transversely round the mass, which sometimes contains nuclei in its interior. In fact, these axile bodies are essentially similar to the bundles of fibrous tissue encircled by elastic fibres, which are to be found in the cutis. In papillæ which were investigated without the application of any re-agent, the axile bodies did not appear more sharply defined than Kölliker had already figured them in his ' Mikroskopische Anatomie' (fig. 4). Nor did a dilute solution of caustic soda, the re-agent which he chiefly employed in examining the

course of the nerves, render them much more marked. On the other hand, acetic acid, which was employed by Wagner and Meissner, usually, though not always, gives these bodies a more defined cylindrical or oval contour.

The axile bodies are to be found only in the palm of the hand, the red edges of the lips, and the point of the tongue; a few traces of them may be detected in the sole of the foot, and upon the back of the hand; but none were to be discovered in the skin of the toes, thorax, back, glans penis, or nymphæ. In the hand, they existed chiefly in the compound papillæ. In the lips, they were discovered in two persons, and not in a third. In the tongue, they were found in the papillæ fungiformes of one person out of three examined.

In direct opposition to Wagner, Kölliker describes and figures capillary loops in papillæ containing axile bodies and nerves. He asserts, on the other hand, that vascular papillæ containing no axile bodies, do, at times, possess ordinary nerve-fibrils; and suggests that even where no such fibrils have been detected, the difficulty of the investigation, and the possibility of the existence of such pale fibres as those discovered by himself in the skin of the mouse, should detract from the importance of mere negative evidence.

As to the course of the nerves of the papillæ, Kölliker, while he does not deny the possibility of the existence of free terminations, describes and figures six instances of unquestionable loops which he has recently observed. In any case, the nerves never terminate in the axile bodies, but only wind round them.

With regard to the function of the axile bodies, Kölliker argues that their non-existence in many parts of the skin which possess every variety of tactile sensation, is evidence that they have no such close and necessary relation to the sense of touch as Wagner suggests; and that they are, in fact, by no means to be considered as special tactile organs. He admits, however, that they may, by their elasticity and resistance, help to intensify tactile impressions; performing a function similar to that of the nails, phalanges, &c., but being no more special organs of sense than these are.

[It is to be regretted that a little bitterness seems to have entered into the controversy between Professors Wagner and Kölliker. The former, not content with attacking his opponent in the Allgemeine Zeitung in good vernacular German, borrows a little English, and characterizes Kölliker's style as "not gentlemanlike." We do not profess to be judges of German etiquette; but as a mere question of philosophy we may be permitted to say, that so far as we are acquainted with Professor Kölliker's writings, the English phrase in question is by no means applicable to them.]

On the Innocuousness of the Introduction of Virus and Virulent Matters into the Digestive Canal of Man and Animals. By M. RENAULT.

VARIOUS facts are known, accrediting the opinion that men and animals may feed with impunity on meats, the produce of animals that have died of diseases reputed malignant. M. Renault, director of the veterinary school at Alfort, has recently presented an interesting paper on this subject to the Académie des Sciences, containing many conclusive proofs in favour of this view. From his numerous series of experiments, he draws the following conclusions:—1. The dog and pig may eat with impunity all the products of secretion, and all the cadaveric remains, as blood and flesh, cooked or not, proceeding from animals which have suffered from the various contagious diseases,—glanders, farcy, charbon, rabies, typhus, the peripneumonic epizootic of the ox tribe, and the contagious epizootic of the gallinaceæ.—2. Fowls enjoy the same immunity, except, perhaps, with respect to this last affection.—3. The virulent matters of glanders and farcy, which completely lose their contagious property during the action of the digestive organs of the carnivora, preserve them, though diminished, in the intestinal canal of the horse.—4. The virulent matter of the blood of the spleen, which the dog, pig, and fowl can digest without inconvenience, often gives

rise to symptoms of charbon when swallowed by the sheep, goat, or horse.
5. This immunity, which the carnivora and omnivora enjoy, as compared to the
herbivora, seems to depend upon the fact, that these viruses, which in their origin
are of essentially an animal nature, undergo complete modification in digestive
organs fitted for animal matters, by means of which they lose their injurious
properties; but which they do not, in the digestive organs of animals fitted to digest
vegetable substances only.—6. It is proved that there is no danger for man
to derive his nourishment from the flesh or other products of pigs or fowls that
have been fed, for a longer or shorter time, with greater or less quantities of the
remains of animals that have died of contagious diseases.—7. There is no sanitary
reason why pigs and poultry should be prevented from feeding on such remains.
8. The cooking of meats and the boiling of the fluids which proceed from
animals suffering from contagious diseases, have the effect of destroying their
virulent properties, and this to such an extent, that the matter of farcy and
glanders may now be swallowed with impunity by the horse, sheep, and goat;
and the remains of birds which have died of epizootic disease, by poultry. So,
too, these matters, which in their fresh state are so powerfully and certainly
contagious, become quite inert, even by inoculation, after undergoing these
processes.—9. Hence it results, that however repugnant it may be for men
to feed upon the flesh or milk of animals dying of contagious disease, there is in
reality no danger in their doing so, when the meat has been cooked or the milk
boiled.—*Bull. de Thérap.* tom. 41, p. 525.

A Remarkable Case of Change of Complexion, with Loss of the Sense of Smell, in a Negro. By Joseph C. Hutchison, M.D.

The subject of this notice, a slave (Jo), the property of Mr. B., of this neighbour-
hood, æt. 45, was born in Ky., of black parents, and was himself perfectly black
until twelve years of age. At that time a portion of skin an inch wide, encircling
the cranium just within the edge of the hair, gradually changed to white; also the
hair occupying that locality.

A white spot next appeared near the inner canthus of the left eye, and from this
the white colour gradually extended over the face, trunk, and extremities, until
it covered the entire surface. The complete change from black to white occupied
about ten years, and but for his hair, which was crisped or woolly, no one would
have supposed, at this time, that his progenitors had offered any of the charac-
teristics of the negro, his skin presenting the healthy vascular appearance of a
fair-complexioned European. When about twenty-two, dark copper-coloured or
brown spots began to appear on the face and hands, being limited, as they are
at present, to those portions of the surface exposed to light. He had rubeola and
pertussis when black, and again after having changed to white: eyes black:
cannot smell, having lost this sense about the time his sable hue began to dis-
appear: taste unaffected.

With the exception of chronic rheumatism, pertussis, rubeola, and scarlatina,
which latter appeared after the change of colour commenced, he has enjoyed
uninterrupted good health.

The history of the above case was obtained from the patient himself, and
Mrs. B., a respectable and intelligent lady, in whose family he was born and raised.
It offers two interesting physiological anomalies — the disappearance of the
pigmentum nigrum, and the affection of the olfactory nerve, with total loss of
smell. The occurrence of measles and hooping-cough a second time (after having
changed his colour), although a singular coincidence, is not *very unusual*, similar
cases having occurred to almost every practitioner.

The integrity of the branches of the fifth pair, being necessary to the proper
performance of the functions of all the facial senses, to which they are distri-
buted, was carefully examined, and found to be complete. The secretion from the

Schneiderian membrane was not obviously increased or diminished; action of the dilators and compressors unimpaired; common sensibility of the nose perfect, internally and externally. Ether, ammonia, &c., produce a pungent, burning sensation, but excite no odour; the one being distinguished from the other only by its greater degree of pungency. Rancid olive oil, castor oil, alcohol, tinct. camphor, &c., produce no sensation whatever, and cannot be distinguished from water. No obstruction of the nerves. Snuff irritates the Schneiderian membrane so as to excite sneezing.

Notwithstanding this patient was deprived of the sense of smell, we were unable to detect any imperfection in the sense of taste; for he could readily distinguish the taste of substances of not widely different flavours, as well as of those that possess no odour, and which tasted very much alike; and although the tongue is endowed with a very delicate and accurate sense of touch, we do not think he could, by this alone, distinguish between substances of the same temperature, consistence, and form, possessing no irritating or pungent properties, as, for instance, butter and lard, molasses and honey.—*Amer. Journ. of Med. Sci.*, Jan. 1852.

On the Action of Ozone on Miasmata. By M. Schönbein.

M. Schönbein's additional researches have still further developed the analogy of this substance to chlorine, and leave no doubt of the injurious effects it may exert on the respiratory organs when in excess. Mice soon perish in an atmosphere containing $\frac{1}{6000}$. The quantity which prevails in the atmosphere is very variable, being proportionate to the amount of electricity, and therefore at its maximum in winter, and its minimum in summer. It is, however, highly probable that, when existing only in minute quantities, it exerts a purifying effect on the atmosphere by destroying various deleterious miasmata. There are a great number of inorganic gaseous bodies, which, when diffused in scarcely appreciable quantities, yet render the air irrespirable—such as the gaseous compounds of hydrogen with selenium, sulphur, phosphorus, and arsenic, gases characterized by the facility with which they become oxidized. Ozone belongs to a class of bodies which, as agents of oxidation, rapidly destroy these hydrogenous compounds. An incessant source of miasmata exists in the variety of gaseous compounds which are incessantly liberated by the decomposition of the innumerable masses of organic beings which perish on the surface of our globe. Although the composition of most of these is unknown, it is to be supposed that their accumulation would render the air unfit for respiration. Nature has, however, provided the means of destroying such deleterious compounds as fast as they are generated, for M. Schönbein regards ozone, which is so constantly generated under electrical influence, and is so powerful an agent of oxidation, even at ordinary temperature, as specially destined to that end. His experiments prove that air containing $\frac{1}{8000}$ of ozone can disinfect 540 times its volume of air produced from highly putrid meat; or that air containing $\frac{1}{3240000}$ of ozone can disinfect an equal volume of air so corrupted. Such experiments show how little appreciable by weight miasmata may be, which are yet sensible to the smell, and how small is the proportion of ozone necessary for the destruction of all the miasmata produced by the putrefaction of organic matter, and diffused in the atmosphere.

We may admit that the electrical discharges which occur incessantly in different parts of the atmosphere, and determine there the formation of ozone, purify the air by ridding it of oxidizable miasmata. At the same time that these are destroyed by ozone, the organic miasmata cause its own disappearance, and prevent dangerous accumulation of it. The opinion that storms purify the air may not be without foundation, as a large quantity of ozone is then produced. In the author's experiments, he has always found a large proportion of ozone in the vicinity of the stormy clouds on the Jura; and air ozonized by phosphorus for experiment, gives forth a similar smell to that perceived amidst storms in mountainous regions. It is very

probable, that in certain localities, the balance between the ozone and the miasmata does not prevail, and disease may be the consequence. As a general rule, however, numerous experiments have shown that the air contains free ozone (though in very different proportions), so that no free oxidizable miasmata can there exist. M. Schönbein recommends that the atmosphere should be tested for ozone, in localities and at periods where fevers and other forms of disease prevail, so that the results of accumulated observations may be obtained.—*Arch. des Sciences (Genève)*, tom. xviii. p. 194, and *Henle and Pfeufer*, N. S., Band i. p. 385.

A Case of Phosphorescence of the Urine and Semen. By Dr. Landerer, of Athens.

A young Armenian merchant, who had become prematurely aged by debauchery, had been treated for impotence by the most distinguished practitioners of Constantinople. Besides tonics and nervines, a mixture containing phosphorus was exhibited internally, and as it seemed to benefit him the patient continued it for some months, rubbing into the vicinity of the genitals also a liniment composed of phosphorized oil, laudanum, and tincture of amber. Dr. Landerer found him at Smyrna, much astonished that his urine and semen (for he had spermatorrhœa) exhibited phosphorescence in the dark, and continued to do so for several minutes, in spite of the addition of water. Attempts to blow out the light only added to its intensity.—*Büchner's Repert.*, viii. 332.

On the Presence of Arsenic in Plants. By M. Stein.

Ten years since M. Stein observed that charcoal which he employed emitted an arsenical odour when burnt; and the researches of Chatin on the great diffusion of iodine, and of Malaguti, Durocher, and Sarzeau upon that of silver and copper, have induced him to re-investigate the subject. He has found arsenic distinctly in the ashes of various hard woods, and in those of the straw of rye, though not in the rye itself. Cabbages from which the external leaves were removed, turnips, and the tubers of the potato, furnished an ash yielding arsenic—circumstances in fact nowise surprising, since Walchner and others have shown that arable land ordinarily contains arsenic.—*Jour. de Chim. Méd.*, 1851, p. 281.

On the Fat of different Regions of the Body. By M. Lassaigne.

From a variety of examinations that he has made, M. Lassaigne concludes—1. That the fat of the same animal does not present the same composition in the different regions of the body in which it is deposited. 2. That the proportions of stearine and elaine differ in these fats. 3. That the point of fusion of fatty bodies extracted from animals is in general high, in proportion as the fat has been taken from around deep-seated organs. 4. That the quantity of stearine separable by the action of solvents is in an inverse proportion to the fusibility of such fats.—*Jour. de Chim. Méd.* 1851, p. 266.

On the Gastric Juice of the Jackal. By Dr. Landerer.

A large jackal having been killed near the Hymettus, Dr. Landerer took the opportunity of examining the condition of the gastric juice. He found in the stomach a small quantity of a very acid fluid, which turned the edge of the teeth, and effervesced with carbonates. As the chief constituents of this fluid, he found free muriatic acid, acid phosphate of lime, muriates, sulphates, an albuminous matter, colouring matter of the bile, and a fatty substance extractable by ether. As the digestive powers of this animal are so powerful, he experimented upon an artificial

digestive fluid, formed by digesting the mucous membranes of the stomach in water, to which a few drops of muriatic acid had been added. The fluid thus obtained exerted a remarkably powerful effect on coagulated albumen, meat, and other articles of diet. Some of it administered to one of his pupils who suffered from indigestion and flatulence after taking the lightest articles of food, enabled him to manage those of very indigestible quality with ease.—*Büchner's Repert.*, viii. 342.

On a Special Acid of the Lungs. By MM. DUMAS & VERDEIL.

M. DUMAS recently presented a paper to the Academy, giving an account of his and M. Verdeil's researches on a special acid secreted by the pulmonary parenchyma in most animals; and which may be found free, but is usually combined with a salt of soda. Obtained in the crystalline form, it is a brilliant body, strongly refracting light. It does not lose its water of crystallization at a temperature of 100° Cent.; but when heated still more, it decrepitates, melts, and is decomposed, giving rise to empyreumatic products. Much coal remains, which disappears without leaving any traces of ash. It is soluble in water and boiling alcohol; but not in cold alcohol or ether. Its ultimate analysis exhibits definite proportions of carbon, hydrogen, oxygen, nitrogen, and sulphur. It forms crystallized salts with bases, and expels carbonic acid from the carbonates.

The existence of this substance is of high physiological interest; for the acid thus secreted by the parenchyma comes in contact with the carbonate of soda of the blood transported by the capillaries, and decomposes it, uniting with the soda and setting free the carbonic acid which is exhaled. The presence of a portion of this acid in the free state, in the parenchyma, indicates that it is really there that it is formed, and not in the blood, which is an alkaline fluid. By uniting with the soda of the blood, the acid does not change the re-action of that fluid, since it merely takes the place of the carbonic acid which is expelled during expiration.—*Journ. de Chimie Médicale,* 1852, No. 3.

PATHOLOGY AND PRACTICE OF MEDICINE.

On the influence exerted by Chronic Diseases upon the Composition of the Blood. By MM. BECQUEREL and RODIER.

THE following are the conclusions of a paper recently read at the *Académie des Sciences,* detailing the results of MM. Becquerel and Rodier's latest hæmatological researches.—1. The majority of chronic diseases and various anti-hygienic circumstances induce an increase or diminution in the three principal elements of the blood—the globules, the fibrine, and the albumen, and this either separately or simultaneously.—2. The globules undergo diminution in the course of most chronic diseases of long duration, and especially in organic diseases of the heart, the chronic form of Bright's disease, chlorosis, marsh cachexia, hæmorrhages, hæmorrhoidal flux, excessive bloodletting, the last stages of tubercular disease, and the cancerous diathesis. The same result is observed in those whose food is not sufficient in quantity or reparative power, or who are exposed to insufficient aëration, humidity, darkness, &c.—3. The albumen of the serum of the blood is diminished in quantity in the third stage of heart-disease, great symptomatic anæmia, the cancerous diathesis, and insufficient alimentation.—4. The fibrine is maintained at its normal proportion, and sometimes increased, in acute scorbutus. It is diminished in chronic scorbutus, as also in the scorbutic condition symptomatic of certain chronic diseases, which is most often and most markedly observed in organic diseases of the heart.—5. In all the above-mentioned circumstances, the quantity of water contained in the blood becomes very considerably increased.—6. A diminution of the proportion of globules is especially accompanied by the following phenomena: a

colourless state of the skin, palpitations, dyspnœa, a *bruit de soufflet* heard at the base of the heart during its first sound, an intermittent *bruit de soufflet* in the carotids, and a continuous *bruit* in the jugulars.—7. The diminution of the proportion of albumen, even though not very considerable, when it takes place in an acute manner, rapidly gives rise to the production of dropsy, but it requires to be much more considerable when not appearing in the acute form.   Considered in a general manner, dropsy is the symptomatic characteristic of a diminished proportion of the albumen of the blood.—8. A diminished proportion of fibrine is manifested by the production of cutaneous or mucous hæmorrhages.—9. In anæmia symptomatic of considerable hæmorrhage or insufficient alimentation, the change in the blood is characterized by a diminution of its density, an increase of the water, diminution of globules, a maintenance of the normal proportion or sometimes a slight diminution of the albumen, and a normal proportion of fibrine.—10. In chlorosis, which is an entirely distinct affection from anæmia, there may be no changes in the blood whatever.   When such are present, they consist in a diminution of the proportion of globules, an increase of that of the water, and the normal quantity or an increase of the fibrine.—11. In the acute form of Bright's disease the fibrine continues normal, and the albumen is diminished.   In the chronic form there is a diminution of globules and albumen, and sometimes of fibrine.—12. Most of the dropsies regarded as essential depend upon a diminution of the proportion of albumen ; and usually originate in a material cause, consisting in a degeneration of the solid or fluid parts of the economy.—13. In diseases of the heart the blood becomes more and more changed, as they approach the fatal termination.   The changes consist in the simultaneous diminution of globules, fibrine, and albumen, and an increase of water.—14. In acute scorbutus, the principles of the blood do not undergo any appreciable modification.   In the chronic form the fibrine is notably diminished, while the globules are sometimes considerably increased.   In both forms, the increase of the proportion of the soda of the blood explains all the circumstances ; but it has not yet been demonstrated.—15. The above modifications should influence our therapeutical management of these different morbid conditions, as each element of the blood is susceptible of special modification.   Thus, when the proportion of albumen is diminished, we prescribe cinchona, and a tonic strengthening diet.   A diminution of fibrine and an increase of the soda of the blood are to be met by good diet, vegetable acids, and appropriate hygiène ; and by hygienic measures and the exhibition of iron, we combat the diminution of globules.—*L'Union Médicale*, No. 66.

Case of Absence of the Sternum.   By Dr. Benjamin.

This was observed in a girl (aged 11) yet living.   The true ribs are connected together on each side by a strong, narrow, cartilaginous band, the two bands being joined below by a commissure.   Between them a space about as broad as the normal sternum exists, filled only with soft parts, and rising and falling with the systole and diastole of the heart.   The thorax, deeply depressed where the sternum should be, is elsewhere much vaulted, and appears to be still more so than it really is, from a deep depression in the abdominal coverings, immediately under the commissure.   At this spot there is a firm ligamentous mass connecting the commissure and the false ribs together. The interclavicular ligaments and the anterior ligaments of the cartilages of the ribs are wanting ; but the two cartilaginous bands impart to the thorax sufficient firmness, and allow of its mobility, although no movement of the anterior ends of the ribs on their axes can take place.   The respiration, is, however, but little impeded from this.   The muscles of the thorax which have attachments to the sternum, are, as far as can be ascertained, attached to the cartilaginous bands or their commissure.   The heart is pushed towards the right side, and its apex beats between the fourth and fifth ribs.   It is directly in contact with

the skin in the space between the cartilaginous bands, and its pulsations, though not abnormal, are there visible. During deep inspirations, the skin in the sternal region is projected strongly forwards. The liver is felt high up under the thoracic parietes. During the first years of life the girl suffered from a difficulty of breathing, which has now ceased, except when she ascends stairs hastily. She is in good health, but not strong ; and none of her four sisters have any organic defect.

Dr. Vaughan describes (in the ' New York Med. Repository,' 1801) the dissection of a child, aged 13, who from its birth had suffered from difficulty of breathing. The sternum was found absent, its place being supplied by a small connecting cartilage. The heart was very large and the pericardium full of fluid. The liver extended into the mediastinum. Another case is related by Dr. Saske, in the ' Oesterr. Med. Wochenschrift,' 1844, which refers to a man, aged 22, who had always enjoyed good health, and followed his occupation as a musician, notwithstanding the absence of the sternum. The ribs were connected by a cartilaginous border, a rudiment of the xiphoid cartilage alone existing. He did not apply on account of disease ; but to be examined for exemption from military duties. Sömmering also met with a living man destitute of sternum. Wiedemann relates another example in his Programma upon the subject.—*Oppenheim's Zeitschrift,* Band lxiv. p. 91.

On a form of Sanguineous Tumour of the Cranium. By MM. Stromeyer and Dufour.

Professor Hecker described, in 1845, a hitherto unobserved affection, under the name of *varix circumscriptus venæ diploicæ frontalis,* consisting in a sanguineous tumour of the cranium, having connexion with the veins of the diploe, and through these with the sinus of the dura mater—an incomplete formation of the outer cranial wall being present. To this M. Stromeyer proposes to give the name of *Sinus Pericranii,* and furnishes notes of two examples he has met with. One of these occurred in a child six years old, who, four years since, fell from a considerable height, producing an indentation of the parietal bone. A swelling formed here, which when full projected to three lines. It filled on occasions which caused congestion of the brain ; but otherwise no fluid could be felt in the depressed part. A deficiency of the outer table existed.— 2. A soldier, aged 20, had had from his birth a large swelling over his left eye, which, on occasions when the brain became congested, projected an inch above the level of the forehead. On emptying the swelling, a depression was felt in the os frontis, as if from a loss of substance. The blood re-entered the tumour half a minute after being pressed out. The condition of the skin was normal.—*Schmidt's Jahrb.* vol. lxx. p. 146.

A case recently related by M. Dufour seems to be of the same nature as the above ; and is the more interesting from a *post-mortem* examination having been made. The subject of it was an old officer, who in 1799 had received, while mounting a breach, a blow on the forehead. In 1847, M. Hutin carefully examined his condition. As there had been no breach of surface, there was no cicatrix ; but a very sensible depression of the frontal bone was present, supposed to be due to absorption of the diploe, and approximation of the other two tables. A tumour, covered with very thin skin, and of a livid colour, formed to the size of half an egg when he held his head downwards towards the ground, but disappeared again as soon as he altered his position. No pulsation existed in it, nor did the respiratory actions influence it, during the short period it could be examined—the position necessary for its production causing vertigo. The veteran enjoyed good health, could read without glasses, had a distinct articulation, and slept a good deal. He died of bronchitis (aged 81) in 1851 ; and after death, inclining the head downwards failed to fill the tumour as it did during life. The brain was found to be healthy. At three centimetres distance from the falx on the right

side, the two parietes of the arachnoid and the dura mater were adherent, as was this last to the skull. On detaching these adhesions, several red points, orifices of open vessels, were observed, and opposite to these, small openings in the table of the skull. Water dropped into this space soon filtered through, rendering the skin tumid; and by injecting fluids and passing bristles, the communication between these openings in the bone and the longitudinal sinus was proved to exist. The calibre of the sinus was somewhat enlarged, and it contained a coagulum. On dividing the scalp externally, slender fibrous prolongations were observed to extend from the fibro-muscular layer to be united with the periosteum, at the edge of the depression, which was five centimetres broad and two and a half deep. It was lined by a very delicate periosteum, continuous with these filaments. The edges of the depression consisted of thickened, compact tissue, while the centre was composed of very thin, rarefied bone. The portion of the bone corresponding to the thinning of the external skin (about two centimetres in diameter) was destitute of diploe, and perforated with holes. No internal projection corresponded with the external depression.

Although all tumour was now absent, the mouths of the vessels corresponded with the apertures in the bone; and the affection might not improperly be termed an *osteo-vascular fistula,* or, according to M. Bouchut, a *fistula of the longitudinal sinus.* The pathological openings were, however, situated about an inch from the sinus, and the reductibility of the tumour during life is to be borne in mind. M. Dufour thinks the case can be best characterized as one of "sanguineous hernia of the cranial vault, by communication of the meningeal vessels with the external skin, through apertures in the bone." His personal inquiries and literary researches have failed to discover a similar case.—*Gazette Médicale,* 1851, No. 49.

On a Case of Human Intestinal Calculus. By Dr. BÜCHNER.

ON examining the body of a man who had been liable to gall-stones, and who died after an attack of peritonitis, Dr. Büchner found that this last arose from the presence of a large calculus firmly impacted in one of the convolutions of the jejunum, and entirely filling up the volume of the intestine. Above this, the canal was much distended as far as the stomach, and was filled with fluid fæcal matter, which, however, was, to some extent, enabled to pass the obstruction. Some fingers' breadth below the calculus, a strong contraction, with mortification and perforation, was observed. The calculus weighed rather above an ounce, and its upper end was very pointed and smooth, probably from the peristaltic action. It measured 3 inches in length, and 4-5ths of an inch in diameter. It was found broken into two pieces, the broken surfaces presenting light and dark coloured concentric rings, like the section of the trunk of a tree. The microscope exhibited, amidst the amorphous mass of which it was composed, large and beautiful crystals of cholesterine; and its chemical examination furnished the constituents of the bile. There was no nucleus found, on a section being made. In the gall-bladder were two calculi of medium size. Dr. Büchner believes that this case of human intestinal calculus found so high up, is unique.—*Henle's Zeitschrift,* Band x. p. 191.

On the Reciprocal Influence of Acute Diseases and Menstruation.
By M. HERARD.

M. HERARD terminates a recent memoir with the following conclusions :—1. All acute diseases exert a pretty similar effect on menstruation.—2. This influence varies accordingly as the disease becomes developed during a menstrual epoch, or during an interval.—3. In the first of these cases the menses are usually suppressed completely or incompletely, when they may reappear after some hours or days,

though usually in diminished quantity. The patients regard the suppression as being the cause of the febrile disease, although the contrary is the fact : and even in the case of acute febrile disease becoming manifested after suppression, we must regard it as a consequence of the chill that has produced this.—4. When an acute febrile disease is developed in the interval, if the next epoch is near at hand, so that the fever continues to it, the menstruation is favoured by the increased hæmorrhagic congestion of the uterus and ovaries.—5. The menses are usually absent or notably diminished in quantity, at the periods which occur during the decline of a disease, or in convalescence. This secondary amenorrhœa, though sometimes persistent, usually only continues for from one to three months.—6. The menstrual eruption in nowise predisposes to disease.—7. Menstruation exerts no appreciable influence on the issue of acute febrile affections. The progress and termination of these are the same, whether the discharge appears or not, whether it is increased or diminished in quantity, is earlier or later in appearance, or whether this takes place at the beginning or end of the affection.—8. In treating acute febrile affections, it is the condition of the disease that must engage our attention ; for it is rare that any special therapeutical indication is derivable from the state of the menses ; and we must act absolutely in the same way if the menses are on the point of appearing, or are expected, as if they were not so.—9. Bloodletting does not, in general, prevent their appearance or continuance.—10. The sudden suppression of the menses by the development of an acute febrile disease, or amenorrhœa consecutive to such disease, does not, in general, call for any special treatment.— *L' Union Médicale*, 1851, No. 149.

On the Influence of Pregnancy and the Puerperal State on the Progress of Phthisis. By MM. GRISOLLE & DUBREUILH.

M. GRISOLLE, in reporting to the Academy of Medicine upon a memoir presented by M. Debreuilh, observes, that the views he formerly expressed[*] have only obtained additional confirmation. In none of the 13 cases related by M. Dubreuilh, or in the 35 now collected by M. Grisolle, has the power formerly vaguely attributed to pregnancy of staying the progress of phthisis, been observed. In some cases, indeed, it seems to have played the part of determining cause, and in others to have aggravated the condition. According to M. Grisolle's observation, cases in which the first symptoms of phthisis are developed at an early period of pregnancy, and amidst a state of health otherwise satisfactory, are more common than those in which the pregnancy is consecutive to the early appearance of the organic disease. Both observers are, indeed, of opinion that phthisical women conceive with difficulty ; and M. Delafond assured the reporter that cows, even at an early period of the disease, usually remained sterile, even though they continued fully alive to the attentions of the bull. He added, also, that in such as did conceive, abortion was common about the fifth or sixth month ; while in such as went their full time, the progress of the disease was in nowise modified. In M. Grisolle's former papers he stated that pregnancy, in his cases, so far from retarding, hastened the progress of phthisis ; and although the rate was found to be somewhat slow in M. Dubreuilh's cases, this probably arose from their having occurred in private practice, while M. Grisolle's were all hospital patients. Both sets of cases, however, amply disprove the suspending power of pregnancy ; and M. P. Dubois' experience has long since led him to a similar conclusion. Phthisis which has appeared at an early period of pregnancy pursues a constantly onward course ; and if improvement is to take place at all, it never does so until after delivery. It is rare for phthisis thus complicated to present those intermissions or sudden suspensions of progress sometimes met with in ordinary phthisis. The children brought forth by phthisical mothers, though usually small, are plump and well-looking to an extent that would not, *à priori*, be expected from persons suffering from so exhausting a disease.

M. Dubreuilh expresses a theoretical opinion in favour of the prevalent belief that the progress of phthisis is hastened by *delivery*, but his facts are against him ; and so complete is the suspension of the disease sometimes, that delusive hopes of cure are entertained.

In regard to the influence of *phthisis on pregnancy*, both observers are agreed that such patients *ordinarily* go their full time ; which must be regarded as a remarkable fact, when it is considered that more than one-half the pregnant women attacked by pneumonia, abort. Both also find that these women usually have very easy labours—a fact due to the smaller size of the child and the relaxed state of the tissues. Both, too, consider that the attempt to suckle exerts the most disastrous influence upon both mother and child.—*Bulletin de l'Acad.*, tom. xvii. pp. 14—25 ; *Rev. Méd.* 1851, tom. ii. p. 649.

On Hereditary Transmission of Phthisis. By M. GUILLOT.

SINCE 1825, M. Guillot has been tracing out the history of certain cases of phthisis, in order to illustrate the laws which regulate the hereditary transmission of this disease. He follows the history of the family-line, in order to ascertain whether this does not, by successive degradation, become exhausted and extinguished. He refers to the case of a man who died of phthisis, aged 66. Before the age of 48, all his four children died of the same disease ; all had children, but the third generation did not survive the period of the first dentition, all being carried off either by pneumonia supervening on tubercle, or by tubercular meningitis. In another example, a grandfather died of phthisis. One of his daughters also died of it at 30. The other daughter is still living, but three of her children have died either of tubercular pneumonia or meningitis. The general conclusion is, that in proportion as phthisis descends in the genealogical scale, its manifestation takes place at an earlier period of life. A child will therefore run greater chance of falling a victim to the consequences of the numerous accidents of a tubercular affection, in proportion as the phthisical parents who have given birth to it have not attained advanced age. In a diagnostical point of view, then, the existence of tubercular disease in the offspring while yet young, offers a very strong presumption of phthisis. The practical importance of this is especially evident in pneumonia, so common is it to find tubercles of the lungs in the bronchial glands, masked by the signs of this affection.—*L' Union Médicale*, No. 5.

On Measles as observed in Idiot Children. By M. DELASIAUVE.

THE remark has been frequently made, that in certain classes of the insane, incidental diseases exhibit a severity which is not usually observed in persons in the possession of their faculties. Exactly the contrary to this has been, it is true, maintained by some, and a supposed immunity asserted. Georget and Esquirol, however, have shown that insanity disposes the subjects of it to be more severely affected than are others by ordinary diseases ; and Ferrus especially points out dementia and idiocy as unfavourable conditions in this point of view. M. Thore, also, in a special essay on the subject, adopts the same view. M. Delasiauve deduces the same conclusion from the opportunties he has had of observing epidemics of *measles* at the Bicetre. The children of the *employés* of the establishment were recently attacked in great numbers, and from these the disease was communicated to the idiot and epileptic children. While among the former the eruption pursued a normal and favourable course, anomalous conditions complicated it among the latter, and very often rendered it fatal. In different epidemics, there has been observed a predominance of some one of these, such as engorgement of the lungs, of the brain, or the parotid, œdema, &c. Violent diarrhœa was the especial characteristic of the present one. Besides this, however, in six out of eight cases,

occurring in one section, asphyxia from bronchitis occurred, endangering the lives of the whole, and terminating fatally in two.—*Annales Méd. Psychol.* N. S. vol. iii. p. 343.

Epidemic of Lead Colic in Paris from Sophistication of Cider with the Acetate.

MUCH attention has recently been called to the very numerous cases of colic occurring in Paris among persons who drank cider; and on investigation being made, it was discovered, that as this year great difficulty in clarifying the fluid had been experienced, some of the brewers had resorted to the use of acetate of lead for this purpose, the base of which had only become partially precipitated with the dregs. The chemists found nearly a grain of lead per litre of the fluid. No sooner did the Board of Health receive an intimation of the matter, than the most laudable energy was employed in examining the remaining stock of cider in the capital; so that within three days all fear of a repetition of the accidents was rendered groundless.

The first cases that were seen, somewhat puzzled those called upon to treat them; but their great similarity to ordinary lead colic led to the appropriate treatment being soon decided upon. The seat of pain differed, however, somewhat from its ordinary locality, occupying the epigastrium and the right hypochondrium in place of the umbilicus; and stools were producible by much milder purgatives than are usually required in lead colic. In all the patients in whom it was sought for, the blue mark of the gums was discoverable. This valuable pathognomonic sign should indeed be always sought for, whenever obscure abdominal pains, local paralysis, or even cerebral symptoms of doubtful origin, are present. A remarkable example of this occurred to M. Martin-Solon. During the events of June, 1848, on a hurried transference of some of the patients to other wards, accounts of their cases were lost. One of these patients came under M. Martin-Solon's care, seemingly suffering from marked coma due to cerebral effusion. No account of the case could be obtained; but after an attentive exploration of the whole body, this sagacious observer detected the pathognomonic condition of the gums; and believing the patient to be suffering from saturnine encephalopathy, he ordered purgatives, and soon rescued him from an apparently hopeless condition.—*Bull. de Thérap.*, tom. xlii. p. 122.

SURGERY.

On the Operation for Strabismus. By Professor PANCOAST.

PROFESSOR PANCOAST believes that it arises from the ill consequences which have resulted from the non-observance of the principles which should guide the surgeon in performing this operation, that by many persons it has been considered as abandoned. His own conviction, founded on the result of about 1000 operations performed since 1839, is, that no other operation in surgery yields more gratifying results than this; and in very few would he feel so disposed to promise a perfect cure, on condition of obedience to rules of after treatment.

First, the nature of the defect to be remedied must be understood. It consists in a spastic or unrelaxed contraction of one of the straight muscles of the eye. Although it usually commences in one eye, the other, sooner or later, more or less regularly assumes the same condition. The *causes* of the contraction may be various, such as imitation, the pressure of a tumour, &c.; but the most frequent by far is the disturbance of the centric extremities of the nerves of the straight muscles by congestion or inflammation of the brain or its meninges, which so often occur in measles, scarlatina, pertussis, or cerebral disease.

The *divergent* form appears occasionally to be produced by congestion of the cavernous sinus, through which the nerves to the eyeball run. When seen recently, such cases have always been relieved by repeated alternate cupping and blistering the nape and the temples, thorough purging, and the cold douche. If not seen early, the external rectus has to be divided.

The common *internal* or *convergent strabismus* should be submitted to the following preliminary investigation. We have first to determine whether the distortion is double, and whether it is equal in both eyes. To do this accurately is difficult, as the patient can voluntarily direct either eye straight towards an object, and the whole obliquity of both seems accumulated in one eye, " I place myself in front of the patient, and direct him to roll the eyes as far as possible, alternately to the right and left. Now a person of middle age, with healthy eyes, can hide the white completely at the outer canthus, and slightly bury the edge of the cornea at the inner. A young subject, in which the tissues are always more yielding, can do more than this. If I find, after repeated trials of this sort, that the movements of one eye are normal, and that in the other the cornea is buried too deeply in the inner canthus, and that a strip of the adnata, one-sixth or one-eighth of an inch, is left uncovered at the outer, then, I say, the internal rectus of this eye is alone at fault, and its tendon must be divided. We will also frequently observe in the faulty muscle, an involuntary and rapid twitching when the eye is turned strongly in the opposite direction. If, at the same time, I find that the eye at fault pretty generally rolls upwards and inwards, or downwards and inwards, I say that a portion of the upper or lower rectus muscle is likewise concerned in the deformity, and may, after the operation on the internal rectus, require to have its tendon cut, and I locate my incision through the conjunctiva to suit. I next observe carefully, when the patient is making no unusual effort with the eyes, whether the lids open equally wide ; or, in other words, if there is not a larger disk covered at the upper part of the cornea in one eye than in the other. If so, I am sure that the eye that has the corner most covered, is the one which has the shortened muscle and the squint ; for the effect of the strabismic contraction is to sink the ball and leave the lids less widely separated."

When the case is a clear one of single squint, and the internal rectus has been divided, if the eye do not become straight, and remains a little sunken, we may afterwards divide a portion of the superior or inferior rectus. But if, without becoming straight, the eye is equally or rather more prominent than the other, the division must be abstained from, or we shall weaken the stays of the ball, and the eye, though straight, will become too prominent and protruding. This result has too frequently been produced by an attempt to cure a double squint, under the mistaken belief that only one eye was at fault. A squint exists in the other eye, but it cannot be considerable, or it would have been discovered during the preliminary trials. The corresponding tendon of this eye has, therefore, to be divided completely. The intermuscular fascia should not, however, be divided, nor the sustaining power of the conjunctiva diminished more than is unavoidable—the object being to straighten the direction of the ball without causing protrusion ; and Guerin's subcutaneous operation accomplishes this the best.

We frequently have a double squint so decided, that the single tendon may be divided (in the adult) on each eye at a sitting. We then apply our tests to discover which eye requires, in order to bring both parallel and equally prominent, the intermuscular fascia to be divided upwards or downwards, or one of the adjoining straight muscles partially, or in extreme cases, entirely cut.

The operation for *external* or *divergent strabismus*, cases of which occur in the proportion of four to five per cent., is easier, as the tendon is uncovered for a larger space, and we have no *plica semilunaris* to avoid. Still the cure of this is neither so easily nor so promptly accomplished as is that of the internal—a circumstance in some cases due to a paralytic condition of the internal rectus.

When treating the *young* (and operations under 10 years of age should be exceptional) the extent of our sections must be more guarded. Here the over-

stretched muscles have much recuperative energy, a gradual increase of their power seeming to extend over several days. The evil to be feared is, that the antagonistic muscle will pull the ball so far over, as to let the divided tendon become reattached too far back on the globe. In few cases, even of double squint, then, should both eyes be operated on at a sitting, under puberty. The tendon on the worst side should be divided, and then we may wait several days, or even in some cases months, to observe the effect. In children we must always allow for this recuperative power of the muscle, and not seek to make the eyes immediately straight. "The proper degree of allowance to make, is one of the difficulties of this operation, which has to be guided by the judgment of the operator, according to the age of the patient and the degree of the deformity. It is true, in case an external squint should follow an operation for one of an opposite kind, you have it in your power to correct it by a subsequent division of the external tendons; yet it is a most unpleasant result, which you ought, and may most usually, avoid. One mode to which, when I particularly fear such a result, I have recourse, is, to stop considerably short of making the eyes straight, to reintroduce subsequently my blunt hook, so as to loose the reattaching tendon, and to nick the fascia above or below very cautiously with the scissars without enlarging the wound of the conjunctiva, until the balls begin again to assume their proper position."

The division of the *oblique* muscles is in all ordinary cases unnecessary, as they are never directly concerned in the production of strabismus. In his earlier operations, Professor Pancoast several times divided them, without ever inducing the slightest change in the ordinary movements of the ball.—*Phil. Med. Exam.* vol. vii. p. 509.

On the Treatment of Paronychia. By Professor Pancoast.

It is important to know the *modus agendi* by which a trifling puncture may give rise to the alarming, or even fatal symptoms sometimes met with. If the puncture be not deep, there is merely inflammation beneath the skin, giving rise, however, to great suffering, from the free nervous distribution to the finger. The cuticle does not give way readily, and a poultice by softening it may afford relief. If this is not obtained, the inflammation sooner or later extends beneath the vaginal ligament, involving the sheaths of the tendons. The ligaments not yielding to the increased bulk of the inflamed tissue, strangulation is produced, and sloughing of the tendinous sheath and periosteal lining, and subsequent death of the phalanges, may occur, producing what is called in America a "felon." The disease, compressed laterally, extends readily upwards towards the palm, following along the sheath of one finger, into the sero-cellular covering of all the tendons of the hand and wrist. It may now extend itself down the thecal bursæ of the other fingers, or along the tendons of the flexor muscles, high up in the arm. The first indication is to relieve the strangulation. A sharp-pointed, curved bistoury is passed vertically through the skin and vaginal ligaments, to the flexor tendons, and brought down with its back towards the palm, so that the point may slide under the ligament, in the direction of the finger, and so pass out. The incision should extend the length of one phalanx only; if this is not enough, it is better to make a separate incision over another phalanx, than to divide the whole length of the finger, when the tendons would start from their sheath. The palm is not to be laid open as we do the fingers, although this would relieve the strangulation; but a deforming and troublesome cicatrix would be left, and a troublesome hæmorrhage might arise. When the middle palmar fascia becomes involved at one of its sides, so as to let a soft fluctuating tumour form in the palm, this may be opened, as the preliminary inflammation will have blocked up the vessels. When the palmar fascia has not yielded, Dr. Pancoast has several times afforded complete relief, without any ill-consequences following, by passing a somewhat blunt-pointed, curved, and rather dull bistoury, under the opened sheath of the tendon, from the first phalanx into the

centre of the palm, and sweeping it gently round, so as to break down the inflamed synovial and cellular tissues, and furnish an outlet for the fluids with which they are distended. Little force is given to the sweep, so that the tendons need not be cut. No hæmorrhage follows, for the swollen state of the parts separates the two palmar arches—the radial arch being fixed by its perforating vessels near the face of the metacarpal bones, and the ulnar being pushed off with the palmar fascia, lying as it does in front of the superficial flexor tendons.—*Philad. Med. Exam.*, vii. p. 505.

[In the 'American Journal of Medical Sciences,' Jan. 1852, Dr. Morgan states that paronychia, during last summer, prevailed in an *epidemic* form at Washington. The constitutional symptoms were in some cases very severe.]

On the Consequences of Congenital Phimosis. By M. Fleury.

M. Fleury terminates his paper upon this subject with the following conclusions : —1. Congenital phimosis possesses a pathogenetic importance that has been almost entirely overlooked.—2. This is exhibited in three orders of phenomena to which it gives rise. These are (*a.*) Accidents relating to the genital organs. The penis and testes are frequently small, and the mucous membrane of the glands delicate, red, and morbidly sensitive. Coition is painful and ejaculation incomplete, difficult, and often attended with severe perineal pain. Exhausting erections and nocturnal pollutions are of frequent occurrence, as are urethral discharges even after the purest connexion. The genital sense is sometimes so excited as to give rise to almost continual erection, immoderate venereal desire, masturbation, and involuntary emission ; while in other cases it is, so to say, extinct, a more or less complete anaphrodisia prevailing. (*b.*) Phenomena referrible to the urinary organs. These are, frequent desire to pass urine, pain at the orifice of the urethra, and other symptoms, which are usually attributed to a vesical neuralgia, disease of the prostate, stone, or some other disease of these organs. (*c.*) Various disturbances of the nervous system, presenting a great analogy to those observed in women suffering from uterine disease, or displacement of the uterus, such as gastralgia, hypochondriasis, and hysteriform attacks—the true cause of which symptoms is always overlooked.—3. The excision of the prepuce is the only remedial means.— In 23 out of 27 cases in which this was resorted to, success was complete, the 4 others being lost sight of soon after the operation.—4. Whatever form of treatment may be had recourse to, these cases cannot be relieved until the vicious conformation has been remedied. After the operation, tonics, anti-spasmodics, and especially hydro-therapeutic means, are often of great service.—5. Ricord's operation, aided by the *serre-fines* of Vidal, constitutes the best surgical procedure.— *Bull. de l'Acad.*, tom. xvii. p. 79.

On the Treatment of Hospital Gangrene. By Professor Porta.

In treating this disease, two principal indications are to be observed ; the removal of the causes, and the arrest of the gangrenous process. By fulfilling the first of these we may often cut short an epidemic, and cure the individual case without resorting to special means for accomplishing the second; the gangrenous process, no longer fed by the influences which induced it, ceasing, the eschar becoming detached, and the wound returning to its simple condition. The success obtained in such cases has seduced many observers into the erroneous belief that the cure of hospital gangrene consisted entirely in removing the causes, without the necessity of attending to the local process at all. It is true that at first this will suffice ; but when the gangrene has struck deep roots into the part, it can maintain itself there in spite of the removal of the primary causes.

From whatever causes it may proceed, hospital gangrene is, at least at first, of an inflammatory character, and calls for general and local antiphlogistics. But

frequently these become useless in consequence of the fugacious or illusory character of the inflammation; and then the stimulant, tonic, and antiseptic remedies, formerly so much abused, are of avail. Notwithstanding, however, the eulogia passed on caustics and the cautery, Dr. Porta was accustomed to find them usually disappoint expectation in his own and other hands, the gangrene continuing to advance, or reappearing, and the patient dying or undergoing amputation. Examining these cases after death or amputation, he found that the gangrenous substance below the eschar was untouched, and that at the periphery of the ulcer it sent prolongations below the integument into the areolar membrane and fleshy substance. The supposed relapse was in fact but an extension of the disease. As even repeated cauterizations failed to reach the ultimate roots of the gangrene, the author believed that the knife might advantageously precede or replace them. At a distance of some lines from the gangrenous margin, he carried an incision quite round this, through the skin, adipose substance, and aponeuroses, opening a passage into healthy parts. When the bleeding had ceased he applied some caustic substance, or oftener the cautery, freely along the track of the incision, so as to surround the gangrenous mass by an artificial eschar. In other cases, in place of following the incision by caustic, he removed by means of the knife all the corrupted mass which covered the part, leaving a clean wound behind. This practice has proved highly successful; for although in some exceptional cases, under the influence of the causes which first produced it, the gangrene has appeared in the recent wound, in the majority of cases the complete removal proved final. In some cases the effect has seemed almost marvellous; the patient, who was exhibiting all the symptoms of typhus, recovering as if by enchantment, after the removal of this source of sanious resorption and reflected irritation.—*Annali Omedei*, vol. cxxxv. pp. 549-57.

Statistical Account of the Fractures occurring in the New York Hospital. By Dr. LENTE.

IN this paper Dr. Lente furnishes a statistical account of the 1722 cases of fracture which were treated in the New York Hospital during twelve years (1839-51); and he compares some of the results with those furnished by the cases reported by Malgaigne, from the *Hôtel Dieu* (1939), by Lonsdale, from the Middlesex (1392), and by Norris, from Pennsylvania (840). The following table exhibits the percentage of the principal fractures:—

	Lente.	Malgaigne.	Lonsdale.	Norris.
Thigh	16·26	15·88	13·00	15·95
Leg	33·62	32·59	20·76	34·88
Arm	9·35	15·98	8·49	27·02
Forearm	15·62	15·83	27·73	
Clavicle	9·11	11·60	26·79	10·00
Jaw	3·19	1·39	2·29	2·26
Patella	1·74	0·31	2·73	1·90
Cranium	7·43	2·73	3·45	5·47

In respect to *age*, Dr. Lente observes that the greatest number of fractures of the thigh, arm, forearm, clavicle, and lower jaw, occurred between 20 and 30; of the leg and patella, between 30 and 40. Of fracture of the leg, 65·80 per cent. occurred between 20 and 40; while of the thigh, only 38·04 per cent. occurred at this age. In the earlier years of life, fracture of the thigh is more common than that of the leg—viz., prior to 10 years of age, as 17·75 per cent. compared to 1·71, and between 10 and 20 as 20·65 to 8·11. No case of fracture of the *cervix femoris*, either within or without the capsule, occurred prior to 23. Above the age of 50 the per-centage of fractures of the os brachii (11·25) is greater than that of any other bone, that of the forearm being only 3·12. Only 1 case of fractured clavicle under 10 years of age, and 21 cases under 20 years, occurred in

a total of 158 ! In regard to the *sex* of the patients, the females varied from 10 to 11 per cent. of the males, except for the clavicle, when they were 17 per cent.

Dr. Lente regards the question of *shortening* as an unnecessary refinement in respect to any other bone than the *femur;* inasmuch as an inch or so of shortening in the upper extremity does not interfere with its uses. In the leg, if there be no considerable deformity from loss of bone, there will be no appreciable shortening; and even when the deformity is great, the shortening is usually inconsiderable. In a great number of cases that Dr. Lente has measured, he has never found any shortening that could give rise to a limp, where there was no loss of bone, even when the fracture was comminuted. For several days after the accident, there is considerable shortening; but the fractured surfaces gradually adjust themselves so as to render this imperceptible, even on measurement. It is this same self-adjusting power which, in fracture of the femur of young children, corrects the obstinate early bowing-out at the seat of fracture. In fracture of the *femur* it is of great consequence to prevent shortening, as this will produce lameness; and this is the only fracture in which, for that reason, extension is required. Dr. Lente agrees with Malgaigne that a certain amount of shortening *must* take place in almost all cases; and he believes that surgeons who declare they can treat cases without *any* occurring, are either disingenuous, or are incompetent to the somewhat difficult task of comparing the limbs by measure. In nine cases out of ten there will be no limp, though a shortening of $\frac{1}{2}$ or $\frac{3}{4}$ of an inch occur; and a shortening of $1\frac{1}{4}$ inch may be overlooked in the ordinary mode of measuring by placing the limbs side by side. To measure properly, requires tact and practice; and a limb should be examined several times before coming to a conclusion. At the New York Hospital, the average shortening amounts to $\frac{3}{4}$ of an inch; and sometimes, in spite of every care, it reaches an inch or more.—*New York Journal of Medicine*, vol. vii. p. 154.

Pseudo-membranous Thickening of the Tunica Vaginalis, in Hydrocele and Hæmatocele. By M. GOSSELIN.

IN this essay M. Gosselin endeavours to demonstrate that the condition described by authors as *thickening of the tunica vaginalis*, is really due to the production of a firm, thick, imperfectly organized pseudo-membrane, which lines the tunica vaginalis, and produces the thickness and inflexibility observed in these cases. It can be completely detached from the tunica, except near the testis, leaving it in its normal state of suppleness and flexibility, though slightly thickened. To the naked eye it looks like dense fibrous tissue, and has been often described as *fibro-cartilaginous transformation*, but its minute structure resembles no normal tissue. Short, narrow fibres are observed, lost amidst amorphous matter, nucleated fibres being found here and there in small numbers. Sometimes calcareous matter is deposited amidst the stratified layers, which has been supposed to arise from incipient ossification; but the microscope detects no vascular canaliculi, or bone-corpuscles. The membrane contains vessels which are continuous with those of the tunica. The fluid in such a hydrocele is usually of a chocolate-brown or deeper colour, from the mingling of blood, and it contains a notable quantity of cholesterine. The inner surface of the pseudo-membrane is rugous, and sometimes covered with plastic exudations. The *testis*, usually placed as in ordinary hydrocele, is some-times situated in front, and sometimes it remains below the tumour, which then simulates encysted hydrocele of the cord. It is often pressed back and flattened, so as not to be felt from the interior of the sac; but the author has never known it atrophied. The body of the *epididymis* is sometimes separated two to five centimetres from the testis, becoming elongated and flattened in proportion to the degree of separation, and assuming a curved form, with an external superior convexity. It is now even difficult of recognition, and has been often divided or cut away in operations. This change in the position and form of the epidi-

dymis, is, however, also a result of the distention of the sac, and may, as shown
by Curling, be present in ordinary hydrocele. M. Gosselin has met with sperma-
tozoa only in one case, but this may arise from the suspension rather than from
the destruction of the function of *secretion;* and he has as yet seen no such
disorganization of the seminiferous substance, as would justify the belief that secre-
tion could not be reproduced. *Excretion* may in some cases be prevented by the
obliterations which occur at the *globus minor,* and which may be a consequence of all
chronic inflammations of the testis and its coverings. In some of the cases, injec-
tion proved that no such obstruction was present.

In their mode of formation, these pseudo-membranes resemble those more deli-
cate and completely organized ones, which are found uniting the two surfaces of the
serous membrane after the cure of hydrocele; and which, indeed, are seldom found
absent (to a partial extent) in the adult and aged. From these the present differ,
by not establishing adhesions between the two surfaces (almost exclusively belong-
ing to the parietal one), and by becoming very much more thick and inflexible.
Their cause is found in whatever may produce a chronic and often unperceived
vaginitis, this arising sometimes spontaneously, sometimes from traumatic causes,
and at others following orchitis or epididymitis.

The diagnosis of the affection is often obscure, and especially in its early stages.
It may be suspected when the transparency of the hydrocele is imperfect, when its
formation has been attended with pain, or when injections have already once or twice
proved unavailing. At a later period it may be confounded with sarcocele, from
which it is distinguished by its less weight, and by the fluctuation, though indis-
tinct. The greatest difficulty is presented when calcareous deposits have been
formed. An exploratory puncture is usually required to assure the diagnosis, and to
ascertain the degree of thickness of the envelopes, and the power of contracting upon
themselves they may possess. Left to itself, the diseased mass may reach the size
of a double fist, but rarely gets larger. A sudden increase sometimes supervenes
on external injury; and there is a great proneness to suppurative action, as a con-
sequence of even slight wounds or injuries. The general symptoms attendant
upon this traumatic inflammation, are often very serious; so that the exploratory
puncture should not be made until we are prepared to follow it by the definitive
operation it may show to be necessary. The prognosis of this pseudo-membranous
hydrocele is much more serious than that of the common one, in consequence of
the more severe operations required for its removal, and the greater liability to
consecutive dangerous accidents. These last arise from the difficulty with which
the adhesive inflammation is set up, and the tardiness of the formation of granula-
tions after suppuration—tedious fistulæ being common, while the false membrane
may be eliminated through gangrenous inflammation, giving rise to great con-
stitutional irritation.

In describing the *treatment* suitable for this affection, M. Gosselin passes in
review the different operations that have been recommended, observing that the
French surgeons have rejected that of *incision,* still performed in England, as highly
dangerous, and prefer to it that of *excision,* or even, on account of the dangers
attendant upon both of these, *castration.* He now proposes for adoption, one
which he terms *decortication* or *ablation of the pseudo-membrane*—an operation
not more dangerous than castration, and one which leaves the testis uninjured.
He thus describes it. Having left-in some fluid after the exploratory puncture,
and ascertained that the testis is situated posteriorly, he carries a vertical incision
along the whole length of the anterior side of the tumour, dividing the tissues
layer by layer. When little more of these remain, he plunges the bistoury into the
lower part, and enlarges the incision upwards. The detachment of the membrane
is effected by the handle of the scalpel or the fingers, just as one would peel an
orange, drawing it inwards at the same time by means of a forceps held in the
other hand. If there is much resistance, a few strokes of the bistoury or scissors
may be required. When the lowest portion near the testis is reached, he stops,
and proceeds to separate the other half in the same manner. When both portions

are thus brought down, they may be divided by the scalpel or scissors; or one side may be excised before proceeding to the other. The bottom of the wound being now, after the removal of this abnormal membrane, composed of healthy tissue, takes on a good form of inflammation, and easily fills up with granulations.

In the only case in which he has tried this procedure, the author has met with complete success. This is related, as are several others illustrative of the different points adverted to in the memoir.—*Arch. Générales,* xxvii. § 5, 295 and 385.

Statistics of Hernia. By M. Hutin.

At a recent examination of the pensioners at the Hôtel des Invalides, M. Hutin found that among the entire population of 3177 pensioners, there were 670 who had hernia. These were distributed as follow:

> 631 Inguinal (213 double, 418 single).
>   6 Femoral (5 left, 1 right).
> 18 Umbilical.
> 11 Superumbilical.
>   2 Subumbilical.
>   2 Near spine of ilium.

670.—*Rev. Méd. Chir.* vol. xi. p. 182.

Cases of Atresia Ani in the Adult, with Preternatural Anus. By Dr. Deutsch.

Dr. Deutsch has met with two of these rare cases. The first occurred in an unmarried woman, æt. 29, whose corporeal development seemed, however, rather that of a girl of 15. She, however, enjoyed good health until seized with a nervous fever. While she was in an unconscious state, it was discovered, on attempting to give an enema, that there was no anal aperture. At its usual seat a cartilage-like substance, the size of a pea, could be felt; and at the entrance of the vagina, immediately below the hymen, which was complete, a pretty regular round opening, half an inch in diameter, was observed, surrounded by a firm, almost cartilaginous edge. The finger, passed through this into the rectum, caused fæcal matter to flow out. Around the cartilaginous border, radiated folds of membrane were observed, resembling those surrounding the orifice of the urethra. The opening contracted slightly upon the finger, but could not be completely closed. The patient, on recovering her senses, was surprised to learn that she was the subject of an abnormal formation. She said she had only inclination for stool once or twice in a fortnight, the fæces then passing either in a fluid state or in small hard pieces. She refused to submit to any attempt to remedy an inconvenience from which she suffered so little.

On examining the other case, occurring in a well-developed girl of 16, a completely flat surface, with neither depression nor elevation, was observed at the seat of the anus. About the middle of the perinæum, and rather towards the right side, an aperture existed about the size of a pea, surrounded by almost cartilaginous edges, and capable of completely opening and shutting. Towards the interior it was lined by a circular fold of mucous membrane; and the completeness of the power of expansion and contraction which existed, left little doubt of the existence of a muscular apparatus.

Doepp states, in the 'Transactions of the Petersburgh Physicians,' that among five cases of atresia ani in the adult, two occurred in girls. The hymen extended to a membrane closing the anus, and an aperture existed in the vagina. In both cases he sought, by an incision in the interval between the hymen and rectum, to enlarge the aperture of the artificial anus; but both cases died in consequence of his interference.—*Neue Zeitsch. für Gebürts,* Band xxx. pp. 281—285.

On the Cause and Diagnostic Value of Muscæ Volitantes.   By M. Tavignot.

M. Tavignot assigns as the cause of this phenomenon, the passage of the luminous rays through a very circumscribed spot of the semi-transparent tissue of the iris, which has become deprived of its pigmentary matter—a fissure in the uvea.   This theory explains : 1st. Why the muscæ are placed near the visual axis, but always on one side of it ; 2nd. It explains the fact of their disappearance in obscure light, and their especial distinctness in a bright one, which induces the contraction of the pupil, and the enlargement of the aperture in the uvea ; 3rd. Also their varied form, according to the different action of light upon the eye, and the effect of this upon the size of the fissure ; 4th. It explains their appearance after sudden movement of the eyes upwards, which is always accompanied by a contractile oscillation of the iris, as also their diminution or disappearance as the pupil enlarges.

If this theory be sound, the muscæ ought to disappear when the pupil is dilated by belladonna ; and M. Tavignot declares that his experiments have convinced him that they do disappear in proportion as artificial mydriasis is thus produced, and that they return again with the returning motions of the iris.   It is to be borne in mind that these remarks are referrible only to *essential* muscæ volitantes ; M. Tavignot intending to show hereafter, that in the *sympathetic* form (as in glaucoma) an altered condition of the texture of the iris explains the appearance, and adds confirmation to the above view.

Artificial dilatation of the pupil enables us to decide whether we have to do with muscæ volitantes, properly so called, or with the spots known as *scotomata,* which are found in partial opacities of the cornea, and in incipient cataract ; for while the muscæ volitantes disappear on the production of the mydriasis, the scotomata persist, and even become more distinct.—*Gaz. des Hôp.,* 1851, No. 127.

On the Combination of Lithotomy and Lithotrity in the case of large Calculus.
By M. Petrequin.

Since the introduction of lithotrity, lithotomy has become a far less common operation, and presents a much smaller proportion of successful cases, inasmuch as the most favourable ones are selected for the new operation.   A surgeon will in future have some difficulty in bringing forward such a statistical account as that of M. Viricel, of Lyons, who, during 1806-12, operated upon 109 patients, and lost only 9, having 53 consecutive recoveries.

Among the dangers of lithotomy, the *voluminous size of the stone* stands foremost, so that when this has proved excessive, various authors have recommended the operation to be left unfinished.   The older surgeons attacked these calculi by various forms of crushing forceps, all of these being of a most gigantic size.   The moderns have more turned their attention to the improvement of the operative procedures ; but in spite of any direction or extent that may be given to the incision, there are calculi too large to be removed by the perinæum with impunity, notwithstanding the dilatability of the prostate and the elasticity of the neck.   Too large incisions also favour the infiltration of urine.

To meet such cases, M. Petrequin adopts a combination of lithotomy with lithotrity.   He does not mean that cystotomy should be resorted to after lithotrity has failed, for fruitless attempts to crush the stone only add to the danger of subsequent cutting.   Simple lithotomy should be confined to cases in which it is best calculated to succeed—viz., calculi capable of removal by a moderate perineal incision ; and when the calculus is too voluminous we should call in the aid of lithotrity to reduce its size. Two cases are related in which this combination was attended by a happy issue, notwithstanding the presence of numerous serious complications.—*Bull. de Thérap.,* tom. xli. p. 442.

MIDWIFERY, &c.

On the Treatment of Syphilis in Pregnant Women. By M. DEVILLIERS.

THE following are the conclusions with which M. Devilliers terminates a memoir upon this subject:—1. A pregnant woman usually supports mercurial treatment pretty well during the first half of pregnancy, and even from the first week.— 2. Any injurious effects that may occur during this period, seem principally to depend upon a want of tolerance in the digestive organs, and consequent nervous irritability.—3. The fœtus is more sensitive to the effects of syphilis, and to the action of specific remedies, in proportion as it approaches the perfection necessary for extra-uterine life.—4. In the application of treatment, the following circumstances in relation to the progress of syphilitic disease in pregnant women should be borne in mind—viz. (*a.*) The condition of conception may excite the external manifestation of symptoms which, for a greater or less period, had remained dormant. (*b.*) The symptoms frequently exhibit oscillations during pregnancy, and have an especial tendency to re-appear about the sixth, seventh, or eighth month. (*c.*) They generally disappear spontaneously, and rather quickly after delivery.—5. Palliatives for primary symptoms in the early months are useless, and a radical treatment should at once be resorted to. It is still more urgent to treat without delay any secondary or tertiary symptoms that may be present.—6. Active treatment undertaken or recommenced towards the latter part of pregnancy—i. e., the period when abortion from syphilis is most likely to occur—requires greater precautions to be observed than during the early period.—7. When the treatment during the first half of pregnancy has not been completely interrupted, or has been so only for a short time, its resumption on the re-appearance of symptoms during the latter period exposes both mother and child to less chance of accidents.— 8. Treatment should not be discontinued too quickly after the disappearance of the symptoms, but persevered with in very small doses as long as possible.—9. The treatment seems to be well borne both by mother and child at all periods of gestation, in proportion to the complicated and aggravated condition of the syphilitic accidents.—10. The syphilitic symptoms, whether primary or secondary, which are manifested during the latter weeks of pregnancy, require general as well as local treatment. The child is then more amenable to treatment, if this be required after birth.—11. We must not wait too long after delivery, to commence or resume treatment, deceived by the decrease of the symptoms so common at that period. If the child, suckled by the mother, exhibits any mark of syphilis, we must not wait later than the eighth or tenth day.—12. In the early period of pregnancy, internal mercurial treatment is often ill borne. This is less frequently the case in the middle and later periods. In the former, inunction should be resorted to.

M. Gibert, commenting upon the above essay, likewise says that the ill effects which several practitioners have observed to result from the administration of mercury in syphilitic women, have arisen from their giving it internally. It frequently excites vomiting and colics in the fourth and fifth months; and mercurial frictions are infinitely preferable. Abortion from syphilis ordinarily occurs after the fourth or fifth month; whence, if treatment is to be preventive, it must be commenced early.—*Bull. de l'Acad.*, tom. xvii. p. 77; *Bull. de Thérap.* tom. xli. p. 439.

On the Reproduction of Lactation after a lengthened Intermission.
By M. GUBLER and Dr. BALLOU.

TWO very interesting communications have appeared at the same time from different quarters, showing that when suckling has been suspended, even for months, it may, in the great majority of cases, be resumed by a little perseverance on the part of the mother. The first of these is from M. Gubler, who gives an account of M. Trousseau's experience in this matter. That able practitioner is in the

habit of advising that children whom disease or accident have kept away from the breast, and who are suffering in consequence, should be reapplied at periods when this is usually deemed useless. Cases are referred to in which the breast had not been taken for two, four, or even nine months, and wherein the resumption of suckling was usually attended with little difficulty, or at all events only temporary difficulty, and followed by the best effects.—*L' Union Médicale,* 1852, No. 7.

The other communication is a paper read by Dr. Ballou at the Rhode Island Medical Society, in which he mentions, that he was led by physiological considerations to expect that the secretion of milk might be re-excited without much difficulty, even after a lengthened suspension of it; this being chiefly dependent upon "the stimulus which the child exerts upon the mammæ in its efforts to obtain nourishment from them, connected perhaps with the mental desires of the mother to foster and nourish her offspring. Accordingly he states that in several cases where a temporary malady had checked the secretion of milk, and a suspension of the function of lactation had necessarily ensued, in cases where there was no return of the secretion after recovery, he ordered the child to be applied to the breasts, two, three, or more times daily, with the uniform result of a renewed secretion of milk. In other cases, where the mother was suffering from debility, and it was evident that the system could not sustain itself under so great a tax upon its powers, Dr. Ballou has directed the removal of the child for a season, and the drying-up (to use the common phrase) of the milk, until the restorative powers of the system, with the assistance of appropriate means, should produce a return of health and vigour. After the recovery of health, he has advised the re-application of the child to the breasts; when it has in a few days uniformly obtained the usual supply of nutriment, in some instances to its own great benefit. In no case, Dr. Ballou assures us, have his expectations been disappointed.—*American Jour. of Med. Sci.,* Jan. 1852.

On the Jaundice of Infants. By M. Duclos.

ALTHOUGH this frequent affection of early infancy does not, in the great majority of instances, present any danger, it occasionally gives rise to important occurrences, and indeed, when complicated with other affections, may sometimes prove fatal.

Besides the yellow colour, the icterus of infants may be attended with fever, somnolence, tension of the belly, and colic, with constipation or diarrhœa. Its causes may be ranged under five different heads, which it is of importance to distinguish. 1. *Retention of the meconium* is the most frequent of all. If it be not evacuated within twenty-six hours, colicky pains are set up, and the skin becomes yellow. The *colostrum* is in this case the best purgative. When the child cannot or will not suck, a teaspoonful or two of the syrup of rhubarb, chicory, and peach-flowers, equal parts, may be given. When, after the meconium has been passed, a considerable degree of tympanitis remains, together with what is called "windy colic," preventing sleep, M. Duclos administers small doses of rhubarb and calcined magnesia.—2. The next in frequency is *spasm of the digestive organs.* The child suffers from cardialgia and colic, is in a state of fever, is constantly trying to suck, and has few or greenish stools. Sometimes convulsions occur. Purging and vomiting aggravate in place of relieving the condition. As retained meconium is usually the origin and cause of the symptoms, that must first be obviated, and then recourse had to emollient baths, mild anti-spasmodics, linseed poultices, friction with camphorated oil, and mild *lavements.* If the milk is too old, the nurse should be changed; and when an anodyne is required to relieve the violent colic, a little lettuce-water should be added to some sugared water. This description of medicine, however, requires care, and opiates in any form are inadmissible. Narcotism, which induced death in one child and was nearly fatal in another, was brought on by a clyster containing ten drops of laudanum.—3. *Engorgement of the liver* is another cause, and one especially acting after compression of that organ by the uterine contraction in foot

and breech presentations.   When this condition is present, purging the child is not sufficient.   It must be kept warm, and its skin rubbed with hot flannel ; with gentleness, however, lest erysipelas be induced.   When the skin is rough and hot, emollient tepid baths are useful adjuvants.—4. *Bad nourishment* is a frequent cause of icterus, the milk disagreeing with the child, or improper food being given to it when brought up by hand.—5. *Cold and humidity :* young infants are very susceptible to changes of temperature—too great heat or cold being alike injurious to them ; but as regards the present affection, cold is especially mischievous.—*Revue Med. Chir.*, tom. x. p. 350.

On the Increase of Weight observed in Infants consequent upon Suckling. By M. GUILLOT.

M. GUILLOT observes, that although many attempts have been made to determine the quality of the milk furnished to infants, no information exists as to the quantity they really take or require; and he proceeds to give an account of some experiments he has made upon the subject, by *weighing* the child every morning prior to and after suckling, and multiplying the weight so obtained by the number of times that suckling is repeated in the 24 hours.   At present he only publishes some of the rough results, to call attention to the subject and induce co-operation in the investigation.

[We may observe here, that it seems to us by no means safe to infer that the child sucks the same amount of milk each time that it is applied; while the number of times M. Guillot says that the children are applied in the 24 hours (from 25 to 30) seems truly enormous.]

In the examples given, the quantity of milk estimated to be taken by children from 2 days to 40 days old, in the 24 hours, varied from less than 1 kilogramme (nearly 32 oz. Apoth. weight) to nearly 4 kilogrammes, the quantity usually fluctuating between 1 and 2.   The rapidity with which even slight ailments were attended with a diminution of weight was remarkable.   One child gained 270 grammes in 17 days ; and a loss of weight of 90 grammes, consequent on slight erythema of the nates, was repaired, and 260 grammes added to the original weight, six days after the erythema had ceased.   This oscillation of weight, consequent upon disease apparently slight, is one of the most remarkable circumstances ascertained.   A child weighing 3904 grammes on admission, at the age of 30 days, increased to 4258 grammes in 12 days ; and its nurse was calculated to have furnished it during a month with milk equal in weight to that of her own body.   The examples upon which these statements have been founded, were hospital children ; and M. Guillot believes that the more robust infants of private life will be found to require at the end of the first month more than 2 kilogrammes of milk daily, their weight increasing more than 50 grammes per diem.

When the child from illness is unable to swallow or digest, its weight rapidly diminishes ; and death may always be expected when this diminution continues progressive.   Examples are given in which the child lost 500 grammes in 5 days, and decreased to 1700 or even 1500 grammes in weight.   This diminution is observed in infants that are brought in with sclerema, and cannot suck ; but the most rapid diminution of weight takes place under the influence of diarrhœa.   The weight of a child in good health, and with a good nurse, should regularly increase day by day.   It decreases, on the contrary, sometimes several days prior to the outbreak of diseases (as pneumonia or measles), and during their continuance.   It again resumes its regularity of increase when these have disappeared.   On the other hand, a remarkable *increase* of weight sometimes precedes diarrhœa, being due to accumulation of matters in the intestines.   M. Guillot's general conclusion is, that an infant must be supplied with more than 1000 grammes of milk a day.—*L' Union Médicale,* No. 16.

MATERIA MEDICA AND THERAPEUTICS.

On the Employment of Manganese. By MM. Petrequin and Burin.

In this paper M. Petrequin brings forward additional evidence of the greatly-increased efficacy which the combination of manganese with iron confers upon the latter metal. M. Burin has recently had a memoir which he presented to the Academy, upon the condition in which manganese exists in the blood, very favourably reported upon by M. Lecanu. According to M. Burin's analyses, 1000 grammes of blood are thus constituted as regards this substance:

	Weight of Globules.	Oxide of Iron.	Oxide of Manganese.
Plethoric man . . .	143·500	1·360	0·071
Normal blood . . .	128·200	1·220	0·060
Chlorotic girl . . .	63·980	0·500	0·025

M. Burin has furnished several formulæ for *ferro-manganese* salts, which are now obtainable in Paris, and are as easily administered as is iron alone—their only drawback being their high price.

This metal being as much a normal ingredient in the blood as is the iron, its administration is indicated in chlorosis and other cases, in which both substances are in deficient quantity; and M. Petrequin believes that the resistance which many of these cases offer to iron, and their frequent relapse, may be obviated by this addition, by the use of which also a smaller quantity of iron is made to suffice. He objects to the administration of this substance unaccompanied by the iron; and he regards the statement that sometimes the manganese, sometimes the iron, is deficient in quantity in the globules, as purely conjectural—the analysis showing thus far, that both substances are alike wanting. M. Petrequin has found these combined medicinal bodies especially useful in blood-diseases, such as the chloro-anæmia after hæmorrhage, operations, metrorrhagia, &c. They are of remarkable efficacy in the chlorosis which appears about puberty, a malady which is far more common even among male adolescents than is supposed. It is also met with at the critical period of women, especially when passive hæmorrhage prevails. Its efficacy is also remarkable in the anæmic cachexia following prolonged intermittent fever, as also attendant upon prolonged suppuration, and on strumous and cancerous disease. In the disordered states of the circulation met with in chloro-anæmia, which are sometimes mistaken for organic disease, the ferro-manganese preparations, combined with digitalis or other substances, are of great utility. The various neuroses of the digestive organs, which complicate the chlorotic state, are frequently relieved; and in the disorders of the nervous system, from various descriptions of excesses, they are found highly useful.—*Bull. de Thérap.*, tom. xlii. pp. 193—206.

Turpentine Embrocation in Ague.

The following embrocation is recommended by M. Debout as being highly efficacious as a substitute for quinine, and especially for the poorer classes:—Essential oil of turpentine, 100; chloroform, 10; or laudanum, 4. It is to be rubbed along the spine twice a day, during the apyrexia, taking care that one of the frictions should take place an hour or two prior to the expected paroxysm.—*Bull. de Thérap.*, tom. xlii. p. 154.

Employment of Oxygenated Water in Asphyxia. By M. Ruspini.

In a recent series of experiments upon the effect exerted by various gaseous bodies upon the contractility of the heart of the frog, MM. Polli and Broglia have especially drawn the attention of practitioners to the stimulating effect of oxygen gas in asphyxia; and to the feasibility of employing injections of oxygenated water

when the respiratory organs can no longer perform their functions.   M. Ruspini, of Bergamo, recently in part carried out these views in treating a case of asphyxia from charcoal vapour, in which a slight degree of animal heat constituted the only remaining sign of life.   He caused some oxygen to be disengaged from a flask of oxygenated water, and this having been respired, and a little of the oxygenated water poured down the throat, animation became speedily restored.—*Jour. de Chim. Méd.*, No. 4, p. 276.

On Matico in Diarrhœa.   By M. Modonini.

M. Modonini states that he has employed this substance in about 120 cases, and usually with good and speedy effect, in various species of intestinal discharges, and especially in atonic diarrhœa.   It is given at the Bologna Hospital in doses of from 18 grs. to ʒj. per diem, no inconvenience save a little nausea or diarrhœa ever being caused, and this being capable of removal, either by diminishing the dose, or temporarily suspending the remedy.   In the most favourable cases, an impression is made upon the disease by three or four doses ; and it is quite relieved in from three to six days.   The medicine should, however, be continued awhile longer, in order to prevent relapse.   In those diarrhœas in which, owing to the existence of visceral changes, or the presence of a general dyscrasis, the return of the diarrhœa is inevitable, the matico is still an invaluable remedy, owing to the rapidity of its astringent action, which enables us to suspend the exhausting discharges, and obtain time for the employment of any other means which the nature of the case may indicate.—*Bulletino delle Scienze Med.*, vol. xx. p. 63.

On the Mode of Administration of Iodine.   By M. Devergie.

M. Devergie administers this substance as an "antilymphatic," in the following manner :—Porphyrized unoxidized iron-filings, 40 centigrammes ; iodine, 1 gramme, 65 to 80 Centig.; water, 8 grammes ; simple syrup, 500 grammes.   The iron and iodine are triturated in a mortar, the water being added drop by drop, and then incorporated with the syrup.   When it is desired to add the iodide of potassium, 6 or 8 grammes are previously dissolved in the smallest possible quantity of water, and incorporated with the iron and iodine before the syrup is added.   A tablespoonful of this syrup is given in a bitter tisane (that of hops, prepared according to the Paris Codex, being the best), night and morning.—*Gaz. des Hôpitaux*, No. 22.

On the Treatment of Chilblains.   By M. Trousseau.

M. Trousseau washes all the parts affected with chilblains, three times a-day, with the following lotion :—Borax, 50 parts ; water, 500.   Four tablespoonfuls are added to a quart of water.   He also prescribes, both for the prevention and removal of chilblains, the following lotion, to be used night and morning :—Sal ammoniac, 20 parts ; water, 40 ; proof spirit, 10.   When ulceration has occurred, he prescribes one of the following formulæ :—Tannin, 10 parts ; water, 500.   Or, Ext. Rhatany, 10 parts ; quince mucilage, q. s.   This is mixed up as a soft electuary, with which the parts are smeared ; and the application is also an excellent one for the cracked lips which occur in the winter.—*L'Union Med.*, No. 35.

On the Preventive Power of Belladonna in Scarlatina.   By Dr. Porcher.

In this paper Dr. Porcher presents his readers with an analysis of the various opinions which have been published upon this subject since they were summed up in the 2nd volume of Bayle's *Bibliothèque de Thérapeutique.*   This survey has necessitated the examination of some hundreds of volumes, and Dr. Porcher

believes that a dispassionate consideration of the subject necessarily leads to a conclusion decidedly in favour of the prophylactic power of this substance. "However some may consider the evidence of a negative character, and therefore unworthy of confidence, yet from its cumulation, from the careful way in which some observers conducted their inquiries, and from the possibility of failure being owing to the use of an inferior or badly-prepared drug, we cannot but conceive that to discard it as utterly indecisive would be indulging a spirit of irrational incredulity, leading to the rejection of any amount of merely presumptive proof."— *Charleston Medical Journal,* vol. vi. p. 453.

On Disguising the Taste of Quinine. By M. PIORRY.

A PIECE of chocolate should be half-masticated, and retained between the cheeks and the teeth. The quinine draught is to be rapidly swallowed; and then the mastication of the chocolate is to be completed, so that it may be swallowed also. The taste of the quinine is thus hardly perceived.— *Bull. de l'Académie,* tom. xvii. p. 329.

On the Effects of Camphor on the Teeth. By M. STANISLAUS MARTIN.

SOME time since, a warm discussion was carried on by our weekly contemporaries as to the effect exerted by camphor dentifrices. In France, for some time past, camphor, under the patronage of M. Raspail, has become the popular specific for all bodily ills. There are numerous persons who are in the habit of daily administering to themselves a camphor lavement, rubbing-in camphor ointments, washing the mouth with the spirit, snuffing the powder, inhaling the fumes, or chewing the substance. Its effect is supposed to be preservative from colds in winter, and from miasmata and epidemic influences in summer. One of the results of this folly is the frequent production of pains of the jaw, and a carious state of the teeth. M. Martin has for several months kept teeth in contact with camphor in powder, and finds that the essential oil completely penetrates them, and diminishes their hardness. If we mix phosphate of lime and powdered camphor, the water which is added, is loaded, in the course of some months, with calcareous matter, which is certainly due to a chemical action between these two bodies.— *Bull. de Thér.* tom. xl. p. 456.

On the Rapid Cure of Itch. By MM. HARDY and DEVERGIE.

M. HARDY states, that by the modification which he has introduced into the mode of treating the itch at St. Louis, he is now enabled to definitively cure a patient in *two hours,* so that it is in contemplation to treat all patients suffering under this disease as out-patients. The entire body is first thoroughly rubbed for half an hour with soft soap, which has the effect of breaking up some of the furrows in which the acari are lodged. A tepid bath is next employed for one hour, in order to soften the epidermis, the patient continuing to wash himself well while in it. Finally, the patients rub each other thoroughly for half an hour over the entire surface with the sulphur ointment (lard 8 parts, sulphur 2, subc. potass 1), and the itch is cured. The various secondary eruptions, formerly confounded with the itch, may require several days for their dispersion, by means of simple baths. In 4 only out of 400 cases so treated, have relapses occurred, and 144 cases out of 145 occurring in June were so cured.

In this way the disease spreads by contagion much less than heretofore, when the patients had to wait until they could obtain admission into the hospital.—In a recent paper, however, M. Devergie expresses an opinion that this rapid cure of a disease which has often been long persistent, is a practice not to be followed, as being dangerous to present or future health. In certain forms of itch the secretion

is abundant, and when the disease has been mistaken, this may increase, so as to form a kind of purulent emunctory not to be at once destroyed with impunity. It is probable, that some of the internal abscesses, which have been observed in cases of this disease, have arisen from its repercussion; and certainly in no other form of pustular eruption would the practitioner thus act. The itching or pruritus which is produced is not entirely an indifferent matter, and the habit of nocturnal scratching must not be suddenly suppressed. Even when pedicular disease or prurigo is suddenly arrested, dangerous pulmonary congestion sometimes occurs, requiring active rubefacients, although here little or no secretion has been suppressed. A single gentle friction suffices to destroy the contagious property, and it is best to complete the cure by repeating such for five or six days, than to run the risk, by too violent friction with very strong ointments, of producing the too sudden repression, or of exciting various forms of eczematous or lichenoid eruptions, which are sometimes more difficult to cure than the original disease, especially among the working-classes, who are so apt to neglect the condition of their skin.—*Union Méd.* 1851, No. 95; *Bulletin de Thérapeutique,* tom. xli. p. 385.

On the Treatment of Intermittent Fever by Salt.

THE employment of so cheap a substance as salt, as a succedaneum for quinine, has of late attracted much attention in France, several practitioners having stated that they had derived great advantage from its use. M. Piorry has instituted several trials at his hospital, and has delivered a very favourable report to the Academie de Médecine, basing his approval, however, chiefly on the alleged fact, that the salt produces an immediate diminution in the size of the enlarged spleen, which he considers as the essential cause of the febrile paroxysm. He believes it of use even in some cases where quinine fails, and in others in combination with quinine. A great obstacle to its employment is the utter repugnance the patients manifest to it in the large doses (ten or fifteen grammes) in which it is required, and the difficulty of retaining it in the stomach. His recommendation that its use should be adopted in charitable establishments, and in the army, was warmly opposed in the Académie, by MM. Grisolle and Levy. The former declared, the trials upon which this had been based were most unsatisfactory, as the diagnosis had been made from the condition of the spleen, not the symptoms of the disease. The cases, too, had been treated as soon as admitted; whereas Chomel long since showed, that the altered hygienic condition of patients admitted, alone sufficed to cure a large proportion without any medicine at all. M. Levy declared that M. Piorry's doctrine of the dependence of intermittent fever on enlargement of the spleen, met with no favour among the medical officers of the army familiar with the disease in Algeria, Corsica, and Rome, where it prevailed endemically. They were well aware that the organ always became engorged and diseased, as a consequence of the affection. In these localities, too, where the engorged and diseased state of the viscera and the cachexia consequent on repeated attacks and relapses had to be combated, medicines which proved successful amongst the simple intermittents of Paris had far less success; and a much smaller proportion of cases were cured by quinine itself than in the capital. For treating these more complicated cases, he considers the *cinchona* itself, and especially the *vinum cinchonæ,* to be far preferable to quinine—it being at once a febrifuge and a tonic, while the quinine and its substitutes act as mere anti-periodics.—*Bulletin de l'Acad.,* tom. xvii. pp. 315 & 390; *Gaz. Méd.* 1852, No. 8.

On the Medicinal Uses of Urate of Ammonia. By Dr. BAUER.

FIVE years' observation has convinced Dr. Bauer that this is a most valuable medicine in chronic cutaneous diseases and in tubercular diseases of the lung. An ointment, containing one scruple to the ounce, is applied by a pencil to the erup-

tion night and morning, the cure being effected in from one to three weeks.   In tubercular disease the ointment is rubbed-in alternately night and morning on the back and front of the chest.   No inflammatory complication should be present during its employment.

In reference to this substance it is interesting to observe, that in Colombia, South America, where lepra prevails so extensively, the benefit derived from the external and internal use of *guano* has been placed beyond all doubt; and Dr. Lallemand has found it of great utility in the treatment of morphæa in the Brazils.   The urate of ammonia, which is found in this substance in great abundance, is probably the chief medicinal agent.—*Buchner's Repert.*, No. 19, p. 86.

FORENSIC MEDICINE AND MEDICAL STATISTICS.

On Poisoning by Sulphate of Iron.   By M. Orfila.

M. Orfila observes, that prior to Smith's and his own experiments in 1815, the salts of iron were supposed to be almost innocuous; and when these experiments proved their poisonous effects on dogs, the point was still regarded by many as undecided as respects man, although M. Orfila has fully demonstrated, that all substances which prove poisonous to the dog, do so also to man.   Several trials before the French tribunals have of late years decided the actual occurrence of such poisoning. In the most recent one, M. Orfila has been consulted in consequence of the incompetency of the provincial *experts ;* and he has drawn up an interesting report of the case, embracing some incidental questions.   The present case was that of a child aged fifteen months, who died after purging and vomiting a black fluid.   On opening the body ten days after burial, the stomach was filled with a greenish fluid, and the vessels of the lungs and brain were gorged with black blood.   M. Orfila detected sulphate of iron in notable quantities in the portions of the abdominal contents forwarded to him.   He does not think that signs of inflammation of the alimentary canal were not present because the *experts* did not find them.   They are so in most cases of this poisoning; but the thick coat of greenish varnish has to be cut through before the state of the membrane can be ascertained.   Absence of inflammation is, however, no proof that poisoning may not have occurred, as the sulphate of iron acts on the economy by producing disorders in it which are the results of absorption, rather than by causing local inflammatory action.   In commenting upon the defective procedures of the country *experts*, who at first were unable to detect the iron at all, and then employed a process which confounded the accidental with the normal iron of the economy, M. Orfila observes, that this last is always to be carefully avoided in judicial investigations.   When copper, lead, or ferruginous salts exist in the alimentary canal as a consequence of poisoning, we have only to treat the canal by means of very dilute muriatic or acetic acid, at a moderate heat— these acids dissolving the metallic substances sought for, without attacking any portion of those metals that form part of the organization.   To obtain these last, we must treat the viscera by more energetic agents, or incineration.

M. Orfila took this occasion to represent to the court the reasons why *experts* could not reply to the question so often put to them, as to whether *a sufficient quantity of poison to cause death had been administered ;* and the danger, in reference to the suppression of crime, the insisting upon such a question gave rise to. The chemist may only be able to detect a thousandth or the twenty-thousandth part that has been administered, when the poison has been evacuated or excreted, and the discharges have not been preserved.   If all the poison has been thus expelled, he may not be able to detect even a trace; and yet although, in the one case, what he has detected has been insufficient to cause death, and in the other he has found none at all, so that the jury may pronounce that no poisoning has occurred, yet has the person died of such poison.   To ascertain the whole amount of poison

that remains in the body, the entire frame would have to be submitted to analysis, which is clearly impracticable; while calculations of the quantity existing in the whole body from that which has been obtained from a part, would give rise to the greatest errors, inasmuch as the poison is not equally distributed over the frame, some portions of this absorbing and retaining much more of it than others. Different processes, also, employed by the same hand, obtain very different quantities; as does the same process wielded by chemists possessed of different degrees of expertness. The French law, too, does not require any decision on this point, as it punishes the *attempt* to poison by any substance that may cause death—this applying, not to the proportion employed, but to the substance used.—*Annales d'Hygiène*, tom. xlvi. pp. 337—382.

On Poisoning by Tartaric Acid. By MM. Devergie & Orfila.

Employed as an *expert* in a case of poisoning by this substance, M. Devergie furnishes in this paper a minute account of the analyses and experiments he undertook for the elucidation of the subject. He comes to the following conclusions as to the action of this substance on the animal economy :—1. Tartaric acid is a poison capable of producing death in a short time.—2. It acts energetically in an inverse proportion to the quantity of water in which it is dissolved.—3. It induces death rather by asphyxia, than by the local lesions it causes.—4. The asphyxia is produced by the absorption and passage into the blood of the poisonous substance. —5. It exerts a special influence on the lungs, in which it gives rise to partial congestions, approaching to hepatizations, which are disseminated amidst the healthy tissue.—6. It exerts a special influence on the blood, seeming to augment its fluidity, while it modifies its nature; so that the blood assumes, when exposed to the air, a bright red-currant colour, which it communicates to the different organs, in proportion as it is freely distributed to them. It remains fluid for an extremely long period.—7. This poison is one, therefore, which chiefly acts by absorption. —8. It nevertheless exerts a corrosive action on the tissues; but this would seem to be only a secondary cause of death.—*Annales d'Hygiène*, tom. xlvi. p. 443.

In the succeeding number of the *Annales,* M. Orfila severely criticizes the foregoing, declaring that the chemical processes employed were faulty, and that the *post-mortem* appearances observed are uncharacteristic. Phosphoric acid, cream of tartar, neutral tartrate of potass, tartrate of soda and potass, or even an excess of wine, will furnish the same re-actions as those which M. Devergie so obtained; while Orfila's experiments on animals show, that poisoning by tartaric acid is unattended by any special symptoms, or peculiar *post-mortem* appearances,—the ecchymosed appearance in the lungs, and currant-red colour of the blood, being also found in animals that had died from the influence of other poisons. From his own researches, M. Orfila concludes—1. That tartaric acid is absorbed, since he has detected it in the blood and liver of dogs poisoned by it.—2. That no conclusion as to poisoning having taken place from free tartaric acid, can be drawn, unless this substance has been obtained from the fluids of the stomach, the blood, or the liver, by alcoholic and not aqueous treatment (that employed by Devergie): water being able to dissolve the tartrates, which act upon acetate of lead and sulphuretted hydrogen just as tartaric acid does. Pure alcohol does not sensibly dissolve these tartrates.

On the Deprivation of the Noxious Power of Poisonous Mushrooms. By M. Gérard.

M. Gérard has recently exhibited before a committee of the Paris Council of Health the complete innocuousness of the most poisonous species of mushroom, after being subjected to a very simple mode of preparation. The experiment was exhibited in his own person, after both he and all the members of his family had made similar trials with the like result. Two of the most poisonous forms were

chosen: the *amanita muscaria* and *venenosa*, of Pearson; and the trial was pronounced quite satisfactory. The preparation, principally consisting in suitable maceration, has indeed been long practised to some extent by the country people. The researches of Letellier have also shown that the principle, which he calls *amanitine*, is very deliquescent, and is remarkably and almost exclusively soluble in water. Alcohol only takes it up by reason of the small quantity of water which it contains; and when *amanitine* renders sulphuric ether yellow, this is owing to imperfect rectification. M. Gérard directs that to every 500 grammes of mushrooms cut up into a medium size, a litre of water, slightly acidulated by two or three spoonfuls of vinegar (or, if nothing else is at hand, gray salt), should be added. If water alone can be obtained, this must be renewed once or twice. In this fluid the fungi are to be macerated for two entire hours, after which they are to be washed in abundance of water. Next they are to be put into cold water and boiled for half an hour, after which they may be taken out, washed, dried, and used as food.—*L'Union Médicale*, 1851, No. 148.

On Spontaneous Human Combustion. By M. DEVERGIE.

MM. BISCHOFF and LIEBIG, employed as *experts* in the recent celebrated case of the Countess of Görlitz, not only declared that *her* case presented an example of *post-mortem* burning, which proved to be true, but took the occasion absolutely to deny the trustworthiness of any of the cases of spontaneous human combustion on record. This position M. Devergie combats, founding his argument upon the consideration of a case which occurred to himself, and of the various accounts of other examples that have been recorded by trustworthy persons. Although the term *spontaneous* is not a strictly correct one, inasmuch as there has always been an immediate cause of the combustion, he retains it for want of a better; and he considers the leading characteristic of these cases to be the *absence of harmony between the mass of the parts burned and the feebleness of the agent of combustion.* He enumerates the following peculiarities, as exemplified by most of the facts on record:—1. The extent and depth of the burns, as compared with the feeble proportion of combustible matter employed in their production.—2. Indulgence in spirituous liquors by the victims.—3. The far greater frequency of the occurrence in women, and especially in old women.—4. The presence of an accidental determining cause.—5. So complete is the combustion in some cases that nothing but the ashes remain, and these are always of the same fatty soot.—6. The combustion while acting on a mass of flesh and fat has usually spared highly inflammable bodies in the vicinity.—7. The flame when seen has always been described as of a bluish colour, and as inextinguishable.

M. Devergie points out how these circumstances differ from those observed in the countess's case, and in death from ordinary combustion. When this extends from the clothes to the person, very large superficial burns are produced, which from their very size prove fatal; but there is no instance of bodies becoming completely carbonized or reduced to the condition in which they are found in these cases. It is true that when the amount of combustible body exists in due proportion to the body to be burned, we may see such effects produced; but the *absence of this relation* is the prime characteristic of these cases. A mere lamp or a hot cinder suffices; while in the experiments made upon the countess's body, 125 lbs. of wood had to be used. The other capital point is, the *isolation of the combustion amidst combustible bodies,* the most inflammable substances remaining uninjured. In the countess's case the floor and chairs, even at a distance, were burned. In M. Devergie's case, complete combustion of the body had taken place in a little wooden room five or six feet broad by eight or nine feet long, and yet two muslin curtains at the window were uninjured. In all the cases, too, *abuse of alcohol* is mentioned; and although Bischoff laughs at this as a mere invention of the persons of the vicinity, for the purpose of pointing a moral, it is too particularly specified in all the cases to admit of doubt. And it is to this abuse of alcohol, that M. Devergie

is disposed to attribute the production of the phenomenon. The quantity excreted by the urine and sweat is probably not in due relation to that imbibed; and a vital modification is impressed upon the tissues, by reason of which they become endowed with a greater combustibility, either mechanically, or by the transformation of the absorbed alcohol combined with the tissues into a new substance.— *Annales d'Hygiène*, tom. xlvi. pp. 383—431.

Muscular Power of the Insane. By M. MOREL.

A GENERAL popular error prevails, that the insane are endowed with inordinate muscular power; and this explains why so many persons are brought to the Mareville Asylum fearfully tied and corded. When M. Morel was first appointed to this, he found numerous patients bound up, reputed dangerous, and especially so because of their vociferations. He set them at liberty, without any ill-effect, and attributes much of the violence that had previously occurred to the ill-conduct of the attendants. He agrees with Jacobi, that, as a general rule, the insane exhibit no inordinate muscular power; and some of the patients of almost colossal stature are easily managed by one person. Indeed, the insane, when engaged in manual labour, soon tire, and require frequent repose. If some of them, by exception, work with a feverish activity, and display great strength, the majority are dejected and languid. The persons in whom he has met with the greatest development of muscular power, belong to the following categories:—1. Persons of small stature, delicate complexion, and nervous temperament; and especially females who appear exhausted by their cries and agitation. Among such miserable-looking beings, a power of resistance is developed under certain circumstances, which defies the united energies of several attendants; 2. Insane epileptics; 3. Monomaniacs, who are not yet exhausted by the disease or irrational treatment. When their passion is opposed, these persons sometimes manifest a resistance only to be overcome by several attendants.—*Annal. Med. Psych.*, tom. iii. p. 560.

On the Duration of Life among the Staff-Officers of the Prussian Army. By Dr. CASPER.

As the result of an examination of the dates of the birth and death of 667 staff-officers of the Prussian army, Dr. Casper found that no less a number than 277 (or 41·5 per cent.) attained more that 70 years of age; while this happened to only 2487 (or 22 per cent.) in 10,000 of the picked lives of the London Equitable Assurance Society. Natural causes of mortality are only taken account of, officers dying in battle or from wounds received therein being excluded. In reference to this point it may be remarked, that while, during the wars of Frederick the Great, 51 generals fell in battle (37 in the Seven years' war alone) only 1 general and 2 brigadier-generals fell during the bloody strife of 1813-15, in which the Prussian armies took so large a part.

On adding together the entire number of years lived, and dividing by the 667, a quotient of 69 years is furnished—i. e., persons of this condition have an equal probability of living and dying at this age. The date of the appointments of 413 of these officers was ascertained, and this was found upon an average to have taken place exactly at the 40¼th year of age. As the average age of death was 69, these individuals had lived 28¾ years after their nomination, the probability of life at the same age being for the male population of Berlin only 20¾ years. Again, the nomination as major-general took place on the average of 588 instances at the age of 48½. The probability of life was then 20½ years, while for Berlin generally it was only 16 years. Dr. Casper explains these favourable results by the absence of excessive exertion of mind or body, while a sufficient alternation of activity and rest for the maintenance of health is secured, and by the blunting the sting of inordinate ambition. When these conditions become somewhat altered by the too sudden cessation of the habits of an active career, results of an opposite character

are produced. Thus the average age of retirement of 236 officers was $61\frac{1}{2}$; so that they lived but $7\frac{1}{2}$ years after, although the expectation of life at that age in Berlin is $10\frac{1}{2}$ years.—*Casper's Wochenschrift*, 1851. No. 36.

[It is with regret we find that Dr. Casper's valuable periodical is to be discontinued. Fortunately this does not arise from a lack of subscribers; but from his intention of establishing in Prussia a quarterly publication, analogous to the French *Annales d'Hygiène*, which will engross all the time he can spare for literary avocations.]

On the General Improvement in the Condition of the Insane.
By M. MOREL.

M. MOREL observes, that since the reform of the French asylums commenced by Pinel and Esquirol, and continued by Ferrus, the physiological condition of their inhabitants has become quite changed; and remarks on their condition, which would have been just, twenty or thirty years since, are so no longer. Among the 800 patients of Mareville, one may seek in vain for the frightful types of degradation seen in different asylums but ten or fifteen years ago. The insane have a more civilized appearance, and are cleaner. The number of dirty patients is considerably diminished; and excrement-eaters are now only found quite exceptionally. For the purpose of clinical instruction, all the idiots and imbeciles of the asylum have been brought together, so that their special characteristics might be studied; and the graphic picture of their condition drawn by Esquirol is not now found of general application. Their condition has become ameliorated by the agency of work, exercise, gymnastics, good diet, and especially by their being transferred to healthy, well-ventilated, and well-lighted localities; for they are no longer confined within walls which prevent their view of the horizon. We must never deceive ourselves by the supposition that patients apparently in the most degraded state are insensible to the charms of nature. Surrounded by these, their features expand, and their physiognomy loses somewhat of its stupid appearance.—*Annales Med. Psych.*, tom. iii. p. 563.

Statistics of the French Hospitals and Hospices.   By M. DE WATTEVILLE.

M. DE WATTEVILLE, Inspector of Charitable Establishments in France, has recently presented a Report upon their present condition, from which we extract various interesting particulars.

There are in France 1133 " Hospital Administrations," which have under their control 337 *hospitals* (in which the indigent sick are received), 199 *hospices* (in which are maintained aged persons, those suffering from incurable diseases, foundlings, and orphans), and 734 *hospital-hospices*, constituting a combination of the two classes of establishments. The number of hospitals in towns are far too few; and the reporter regrets that the great bulk of legacies and donations that have occurred of late (122,514,890 francs during 1800-45) has not been employed in founding new establishments rather than in enlarging old ones. These 1270 hospitals and hospices possessed, in 1847, a *revenue* of 54,116,660 francs. This was derived from three sources:—1. The landed and funded property of the establishments (24,453,654). 2. Contributions of the communes, amounting to 16,164,117 francs, of which the city of Paris contributed 3,133,174. And (3.) the repayment of expenses incurred, to the amount of 13,498,888. Of this sum 1,817,967 francs were received from patients who were admitted for the payment of small sums, which raised them from the rank of paupers. The large sum of 2,772,524 francs was paid for soldiers in the communes where there were no military hospitals; and even this does not repay the actual cost. The *expenditure*, in 1847, was 51,900,415 francs, or more than 2 millions less than the receipts.

In the 1270 hospitals and hospices, there are made up 126,142 beds. Of this number, 46,538 are destined for the sick, and 55,052 for the aged and children in

the hospices.   There are 7853 beds devoted to the insane.   These 126,142 beds were occupied, in 1847, by 575,223 persons.   Of this number there were treated gratuitously in the *hospitals* 380,840 patients; 206,201 men, 139,616 women, and 35,023 children—the small number of these last being accounted for by the fact that many hospitals refuse to receive very young children.   There were 87,500 soldiers admitted, who, added to the 63,000 admitted into the military hospitals, constitute the enormous number of 150,000 sick in a force of 300,000 men, or 1 in 2.   There were 24,176 orphans in the hospices, at the expense of 200 francs per head.   The *mean duration of residence* in the hospitals, for all France, was 40 days for men, 67 for women, 70 for children, and 260 for the insane.   In some of the rural districts, where there is no demand for the beds, patients remain in hospital sometimes for 5 or 6 months; but in the urban districts the duration of residence is below the average.   Thus, at Paris it is 24 days for men, 25 for women, and 21 for children; at Lyons 26 for men, 28 for women, and 25 for children; and at Marseilles 20 for men, 32 for women, and 24 for children.   The *mean daily cost* of a patient in the hospitals of France is little more than a franc.   The *mean mortality* (which, however, varies extremely, even in adjoining departments) in the hospitals is 1 in 15 of the men, 1 in 12 of the women, and 1 in 16 of the children.

Cost of Administration, &c.—For the administration of the 1270 hospitals and hospices, providing 126,142 beds, there are not less than 31,488 official, medical and menial *employés*—being, in fact, 1 to every 4 inmates!   Of this number, however, the 5927 administrators serve gratuitously.   Connected with the general management of the institutions, their possessions, finances, &c., there are 4439 persons.   The physicians, surgeons, pharmaciens, and internes, amount to 2874; and the midwives and female pupils to 376.   The *religieuses*, teachers, and immediate attendants on the sick, number 12,058; the other *employés* and servants, 5814.   The expenses of this enormous staff absorb a fifth of the entire revenues. In some of the smaller hospices, to take care of from 10 to 20 patients, there are from 5 to 10 *religieuses*, besides 2 or 3 servants.   The 2167 physicians and surgeons receive 817,497 francs.

M. de Watteville concludes his report with a comparison of the former and present condition of the charitable establishments, at least as far as the imperfect statistical statements of Necker and Tenon allow it to be drawn.   1. While in the year 1780 there were 870 hospitals and hospices in France, there are now 1270.— 2. These establishments then possessed a revenue of 20,000,000 francs, and now possess one of 54,000,000; or 40,000,000, excluding the expense of foundlings, the insane, and the military, which is repaid.—3. In 1780, 110,000 persons could be taken charge of, while now 126,500 can be so, or, including foundlings brought up at the cost of the public, 225,000.—4. The mortality is nearly the same for the two epochs, in spite of the immense amelioration which the internal management of charitable establishments has undergone.—*Gaz. des Hôpitaux*, 1851, Nos. 59, 91, 96, 99, 102.

[This last statement, were it not based upon statistical documents, examined by n competent observer, would be incredible.   How, then, have the hygienic ameliorations of the last half century, in which France has so largely participated, failed to produce an impression on a class of establishments so especially open to their influence?]

MEDICAL INTELLIGENCE.

It will be seen from our Advertisement Sheet, that a Prize of 50*l.* is offered for the best Essay on the Nature and Treatment of Hydrophobia; this sum having, we understand, been placed at the disposal of Professor Miller, by a gentleman who is desirous that the subject may be elucidated.—The Prize, it will be noticed, is liberally thrown open to all competitors.

BOOKS RECEIVED FOR REVIEW.

The Physiological Anatomy and Physiology of Man. Part IV., Sect. 1. By Robert B. Todd, M.D., F.R.S., and William Bowman, F.R.S. With 53 wood-engravings. London, 1852. 8vo, pp. 208.

Lectures on Histology, delivered at the Royal College of Surgeons of England, in the Session 1850-51. By John Quekett, Assistant Conservator of the Museum. Illustrated by 159 woodcuts. London, 1852. 8vo, pp. 215.

On Animal Electricity : being an Abstract of the Discoveries of Emil du Bois-Reymond. Edited by H. Bence Jones, M.D., A.M., F.R.S., &c. London, 1852. Fcap. 8vo, pp. 214.

Magic, Witchcraft, Animal Magnetism, Hypnotism, and Electro-Biology. By James Braid, M.R.C.S.E., &c. Third Edition, greatly enlarged. London, 1852. Fcap. 8vo, pp. 122.

An Essay on Unhealthy Inflammation. By M. Broke Gallwey, Surgeon, Royal Artillery. London, 1852. 8vo, pp. 90.

On Diseases of the Liver. By George Budd, M.D., F.R.S., &c. Second Edition. London, 1852. 8vo, pp. 486. With Four Coloured Plates.

Second Report of the General Board of Health on Quarantine. Yellow Fever. With Appendices. London, 1852. 8vo, pp. 414.

Statistical Report of the Epidemic Cholera in Jamaica. By John Parkin, M.D. London, 1852. 8vo, pp. 61.

The Physical Diagnosis of Diseases of the Abdomen. By Edward Ballard, M.D. Lond., &c., &c. London, 1852. 12mo, pp. 25.

Homœopathy Unveiled ; or, Observations on Hahnemann, his Doctrines, and Treatment of Disease. By William Perrin Brodribb, M.R.C.S. Second Edition. London, 1852. 8vo, pp. 45.

Homœopathy and the Homœopaths. By J. Stevenson Bushnan, M.D., F.R.C.P.E., &c. London, 1852. Fcap. 8vo, pp. 214.

The Spirometer, the Stethoscope, and Scale-Balance ; their use in discriminating Diseases of the Chest, and their value in Life-Offices. By John Hutchinson, M.D. London, 1852. pp. 79.

Observations upon the Importance of Establishing Public Hospitals for the Insane of the Middle and Higher Classes. By Thomas Dickson, L.R.C.S.E. London, 1852. 8vo, pp. 62.

Insanity, its Causes, Prevention, and Cure ; including Apoplexy, Epilepsy, and Congestion of the Brain. By Joseph Williams, M.D. London, 1852. 12mo, pp. 317.

Blennorrhagia and Syphilis, their Nature and Treatment : being an Analysis of the Letters of Ricord. By M. H. Stapleton, M.D., M.B., &c., &c. Dublin, 1852. 8vo, pp. 61.

On the Anatomy and Physiology of the Male Urethra, and on the Pathology of Strictures of that Canal. By Henry Hancock, F.R.C.S., &c. London, 1852. 8vo, pp. 86.

Die Lehre vom Hornhaut Staphylom. Nach dem gegenwärtigen Standpunkt der Wissenschaft zusammengefasst von W. Roser, Prof. der Chirurgie in Marburg. Marburg, 1851. 4to, pp. 44.

Lectures on the Principles and Practice of Midwifery. By Edward William Murphy, A.M., M.D., &c. London, 1852. 8vo, pp. 616. Illustrated by lithographic plates and woodcuts.

The Symptoms and Treatment of the Diseases of Pregnancy. By William J. Anderson, F.R.C.S. London, 1852. 12mo, pp. 119.

The London Dispensatory : a Practical Synopsis of Materia Medica, Pharmacy, and Therapeutics. By the late Anthony Todd Thomson, M.D., F.L.S., &c. Eleventh Edition. Edited by Alfred Baring Garrod, M.D., &c. London, 1852. 8vo, pp. 1230.

A Handbook of Organic Chemistry for the use of Students. By William Gregory, M.D., F.R.S.E., &c. Third Edition, corrected and much extended. London, 1852. 12mo, pp. 532.

Chemical Manipulation and Analysis, Qualitative and Quantitative, &c., &c. By Henry M. Noad, Ph.D. A New Edition, considerably enlarged. London, 1852. 8vo, pp. 431.

The Sanitary Condition of New Orleans, as illustrated by its Mortuary Statistics. By J. C. Simonds, M.D. Charleston (South Carolina), 1851. 8vo, pp. 71.

Southern Medical Reports ; consisting of General and Special Reports on the Medical Topography, Meteorology, and Prevalent Diseases, of the Southern States. Vol. 11., 1850. New Orleans, 1851.

The Vital Statistics and Sanitary Condition of Memphis (Tennessee). By George R. Grant, M.D. New Orleans, 1852. 8vo, pp. 19.

Climate of Italy in Relation to Pulmonary Consumption ; with Remarks on the Influence of Foreign Climates upon Invalids. By T. H. Burgess, M.D., &c. London, 1852. 12mo, pp. 206.

The Natural History of Animals. By Thomas Rymer Jones, F.R.S., &c. Second Volume. With 104 wood-engravings. London, 1852. 12mo, pp. 395.

Class Book of Botany ; being an Introduction to the Study of the Vegetable Kingdom. By J. H. Balfour, M.D., F.R.S.E., &c., Edinb. 1852. With upwards of 1000 Illustrations. 8vo, pp. 357.

The Affghans, the Ten Tribes, and the Kings of the East ; the Moabites, the Druses. By the Right Hon. Sir George Rose. London, 1852. 8vo, pp. 162.

The Oration delivered March 8, 1852, before the Medical Society of London. By Edward Canton, F.R.C.S., &c. London, 1852. 8vo, pp. 31.

Life of Dr. John Reid. By George Wilson, M.D. Edinburgh, 1852. Fcap. 8vo, pp. 316.

Dictionary of Domestic Medicine and Household Surgery. By Spencer Thomson, M.D. Parts I.—VI. London, 1852. 8vo.

Annals of Pharmacy and Practical Chemistry. Edited by William Bastick and William Dickenson. Nos. I.—VI. London, 1852. 8vo.

The Upper Canada Journal of Medical, Surgical, and Physical Science.

Canada Medical Journal, and Monthly Record of Medical and Surgical Science.

PART FIRST.

Analytical and Critical Reviews.

Art. I.

1. *Second Report on Quarantine—Yellow Fever; with Appendices, by the General Board of Health; presented to both Houses of Parliament, by command of Her Majesty.—London, 1852.* pp. 414.

2. *Observations on that portion of the Second Report on Quarantine by the General Board of Health, which relates to the Yellow Fever Epidemy on board H.M.S. Eclair, and at Boà Vista, in the Cape de Verde Islands.* By J. O. M'WILLIAM, M.D., F.R.S., Medical Inspector H. M. Customs.

THE First Report on Quarantine by the Board of Health was a general treatise on Epidemic Diseases; the Second deals specifically with Yellow Fever; and the Third, which looms dimly in the darkness of 1853, is to be occupied with the subject of Oriental Plague. Our readers will recollect that we were compelled to record our dissent from the principles of the First Report; we now find ourselves necessitated to pursue the same course with regard to the Second; and without consulting a clairvoyante, we have little doubt that a similar manner of dealing with the Third will be required at our hands.

We may state with perfect truth, that it gives us pain to criticise and to condemn the productions of a Board, by whose exertions we at one time hoped the entire nation would have profited. The Board of Health is the impersonation of the principle, that the health of the people is worthy of the attention of its government. It was the result of an extensive agitation on the part of those who knew what the sanitary condition of our great towns is, and what it might become. Great results were expected from this first systematic attempt to put into practice wise principles, and thereby to arrest great and increasing evils.

That the Board of Health have not done all that was expected, and not unfairly expected, from them, must be admitted. They can plead in excuse that they have been hampered by absurd legislation, and cramped by the

distrust and ignorance of those with whom they had to deal. Whether this plea can justify all their short-comings, we shall not stop now to ask. At the present moment it concerns us more to inquire what they have written, than what they have done; what they wish the legislature to do, than what positive results can be traced back to their own official existence of four years.

Our pages can testify that we have been anxious to support the Board of Health, whenever we felt it possible to do so. In spite of the anomaly of what should be a medical body, having only one medical member, and being directed by lawyers and engineers, the principle involved in its existence was so important that we were at first anxious only to discover good, and not to probe too severely what there might be of feebleness and untruth in its proceedings. The Board of Health, however, have now left us no alternative but to oppose them, and to use our utmost endeavours to prevent the legislature from adopting their opinions. The medical press of this country is in fact the only antagonistic element which the Board of Health have to meet. The general press are contented to adopt and echo their opinions; the legislature which called them into action can scarcely refuse assent to the conclusions which they profess to have arrived at by an impartial investigation of the subject; parliament, when called upon to deliberate on their measures, is not likely to bestow much study on a distasteful subject, but if the government have a working majority, will probably at once confirm their propositions. Hence there is really a great chance that principles, which we must designate as utterly false in themselves, and as eminently dangerous in practice, will be acted upon throughout the vast extent of the British colonies, and in all the dependencies over which the British crown has influence.

The question of Quarantine is no petty subject which the medical profession can afford to throw aside without consideration. It is more than a national, it is an universal subject. The practitioners of medicine in all countries are naturally the persons to whom the public will look for information and for guidance. We ought to be prepared both with our opinions and with our reasons for them. We ought to know how far our real knowledge reaches; and at what point observation fails to penetrate farther, and where uncertainty and perplexity begin. To fix this point, to present to the legislature all the ascertained facts which bear upon the subject, and then, passing onwards, to recommend provisionally what common sense and wise precaution indicate, would be the duty of a commission appointed to inquire into the question of quarantine. Such a commission would find that the whole administration of quarantines in Europe, and perhaps in all parts of the world, is founded on no uniform and settled principles; that the rules vary without reason in neighbouring localities; that many of the customs are not based on any appreciable scientific ground, but arise from obsolete dogmas, or are grounded on hypothetical reasoning for which no proof could be advanced. Quarantines, in fact, would be found to be little else than a chaos of arbitrary and useless rules, needlessly oppressive to individuals, and vexatious to trade, and yet not sufficiently protective against the evils to oppose which they were formed.

It would be observed, also, that in all the countries in which quarantines are employed in their most vigorous and extended form, the precautions

which common sense directs us to employ against pestilences, are neglected in as great a degree as the quarantine regulations are enforced. Severity of quarantine, and laxity of sanitary rules, appear to go together. A traveller landing after many days' detention in some of the towns of Italy or Sicily, must be amused at the contrast between the care with which he has been purified, and the utter neglect of the inhabitants to purify themselves. It would also be found that the more obscure and recondite conditions which appear to be necessary for the spread of great pestilences, had been altogether overlooked by the framers of quarantine laws; the inquiry into the propagation of epidemics having been looked at from a single point, and having ended in the adoption of a single precaution.

Yet, in spite of blunders and omissions, the commission would find that at the bottom of quarantines there is a real and unquestionable principle— a principle which has had force enough to prevent the practice based upon it from being swept away as utterly useless and unnecessary. This principle is, that in the case of many diseases, the sick man for a certain time may communicate a similar disease to those around him. The first great labour, then, is to discover *what* diseases are thus communicated; and the second is to know in what manner and under what conditions the communication takes place.

I. To the first question Science does not even now return a complete answer. That in certain cases, diseases pass from one human system to another, is undoubted. We can carry the potential cause of the disease on the point of a lancet, and place it where we please. In other cases, the virus cannot be thus bodily transferred; yet that in some way or other it *is* transferred, that it is thrown off by one body and is received by another, is a fact as certain as that light streams from the sun and sinks into the earth. From time to time, it is true, men may arise, who have minds so singularly constituted as not to acknowledge this fact, and who, from some peculiar mental defect, cannot receive this doctrine of contagion. To the objections of such people, however, Science would make no more reply than to those who in the nineteenth century deny that water can be partitioned into elements, and is compounded of dissimilar gases.

But if this doctrine of contagion is certain when applied to small-pox, to measles, to scarlet-fever, to typhus, or to plague, it is not so certain in the case of some other diseases; among which, yellow fever, till lately, held a prominent place. The weight of proof was not so great here as in some other diseases; the evidence was more divided and was more uncertain. On either side of the proposition, whether or not yellow fever can be transferred from one person to another, great names are ranged. The older evidence on either side, if alone submitted, appears, like a clever advocate's speech, to be overwhelming. It is only when we hear the other side, that we become aware how much the partizan has left unsaid. As contagionists and non-contagionists unfold their budget of facts, the mind sways like a pendulum beyond the point of stable equilibrium, at which it appears destined never to arrest itself, into the vague and unmeasured regions of uncertainty and doubt.

In order to avoid these extraordinary discrepancies of evidence, we adopted, in our former articles on Yellow Fever,* the following line of

* See vol. i. pp. 49 & 369.

argument. We inquired if there was on record a single decided instance of yellow fever being introduced among a community by contagion. We felt that if a single undoubted case could be made out, light would be thrown on every other epidemic of yellow fever which had occurred. On a careful examination of the old epidemics of yellow fever, we found it to be impossible to affirm of any one, with as much certainty as we thought desirable, that it *had* been introduced by contagion. A subject surrounded by intrinsic difficulty had been still more complicated by the passions of men; witnesses and judges had alike become advocates and partizans; and a question of pure science had been discussed with the fierceness of irreconcilable hostility.

Previously to 1845, the contagion of yellow fever, if probable, was not proved with as much certainty as appeared necessary. One case, it is true, existed, in which the evidence seemed complete; in every other instance, possible error might have crept in; in every epidemic one could put one's finger on a point, and say, here a link is lost. In 1845, however, a case occurred, so remarkable, so wonderful indeed in all its features, that, not to speak it profanely, it seemed as if Providence had desired to unravel the web that the perverse ingenuity of man, and the complication of events, had produced.

Let any one picture to himself how an experiment on the contagion of yellow fever could be best conducted, so that all fallacies might be avoided. What might a non-contagionist demand, in order that no more room for doubt should remain, and that he should avow his conversion?

" To prove that yellow fever can be imported into a place," he would say, " I should take an island in the middle of the ocean; I would surround it with other islands possessing the same characters of climate and soil, and peopled by the same race. I would have this island healthy for at least fifty years. Then I would have arrive at it a ship scourged with yellow fever; this ship should implore succour, and it should be granted; yet, as a wise precaution, some kind of quarantine should be established. Of all the inhabitants of the island, some few only should be brought into the immediate presence of the sick. Within the period fixed by experience as the stage of incubation of this disease, one or two of these few, and these only, should begin to suffer from the same disease; then from these sufferers the disease should gradually spread as from a centre, until the whole island was infected. While, however, this one island was thus ravaged, the adjoining islands which the infected vessel had not visited should remain perfectly free, in order to prove that there was no (so styled) ' epidemic constitution.' Then, if all these conditions were fulfilled, I would confess that yellow fever *may* be contagious, and *can* be imported."

Unlikely as it might seem, that in the case of any contagious disease, an experiment like this could be tried, or a similar series of events traced out, it is the fact, that in the history of yellow fever there are no less than two cases in which the evidence is as stringent and as convincing. The first case was that of the introduction of yellow fever into Ascension by the *Bann;* the second was that of its introduction into Boa Vista by the *Eclair.* The first was sufficient to convince an ardent non-contagionist, Sir William Burnett, of the facts of contagion and importation; the second has convinced every one, who, with a candid and unprejudiced mind, has looked at the

evidence in order to arrive at truth, and not for the purpose of picking out materials to support a foregone conclusion.

The history of the epidemic of Boa Vista is, indeed, the cardinal point in the discussion on the contagion of yellow fever. In nearly all other cases (except in that of the *Bann*) it is almost idle to discuss the evidence. Hard swearing on one side is met by hard swearing on another; and we merely attribute to others the impressions produced on ourselves, when we say that whoever will go over and attempt to put into order the evidence adduced on the epidemics of the West Indies and of Philadelphia at the latter end of the last century, or on the Spanish epidemics of 1803, 1811, 1819, or 1828, will be utterly disgusted and disheartened at the way in which a scientific question has been bedaubed with the violence of party feeling, and has been disfigured with the personal animosities of those who have pretended to investigate it. If anything like certainty is to be reached, we must sweep away as useless lumber all this conflicting, irreconcilable, and, in many cases, evidently false testimony; and must make a *tabula rasa*, on which we may delineate more cautious and truer lines.

In the midst of these difficulties, the case of the *Eclair* stands out like a mathematical demonstration, which leads irresistibly to a conclusion. All the elements of the experiment we might have devised were given us. While yet the facts were fresh and vivid, an observer eminently calculated for the task was sent out to investigate them. If there be truth in human testimony, Dr. M'William's evidence on this point must be received. Take the facts recorded in his Report; arrange them how you like; try them, as we have tried them, by every test; collate them from end to end; and one only conviction can remain—namely, that the *Eclair* introduced yellow fever into Boa Vista. So satisfactory was the evidence, that parliament acted on it without hesitation; and compensated, as far as it could, the inhabitants of Boa Vista with a grant from the imperial treasury.

But, as if to leave the case without the shadow of a doubt, after Dr. M'William had left the island, another officer, known to be highly unfavourable to the doctrine of contagion, was sent out to Boa Vista to re-examine the evidence. In not one single point did this officer, Dr. King, succeed in shaking the evidence collected by Dr. M'William; nay, although he still adhered to his own previous conclusions, it is a most remarkable fact, that from this Report, intended to disprove contagion, the actual contagion and importation of the Boa Vista fever can be clearly made out.[*]

We do not intend now to go again fully into this evidence. We have already done so at great length; and to do so again would be both tedious and unnecessary. We refer to our first volume (p. 49) for the full details of the case, and in our subsequent remarks must suppose our readers to be acquainted with our former articles.

The Report of the Board of Health is a kind of summary of some of the epidemics of yellow fever; the summary is, however, very carelessly done, and in no case is the full evidence presented. To do so, would indeed have required a dozen volumes, instead of a slender Report of 140 pages. If,

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[*] Vol. ii. p. 163. We cannot avoid referring, in this place, to the published opinion of Dr. Graves (' Dublin Quarterly Journal,' 1849), that in our article on Dr. King we actually succeeded in proving, with his own premises, a conclusion contrary to that affirmed by Dr. King.

however, the old epidemics are to be gone into at all, we protest against a system of *culling* evidence, taking what suits, and leaving what does not suit, and then coming to a grave conclusion, as if the whole subject had been sifted.   For ourselves, we willingly leave these epidemics, and shall not attempt to supply the deficiencies of the Board.   The long disquisitions of Dr. Gilkrest and Mr. Howell have no kind of interest for us ; we regard it as a waste of time to dig down into these old mines, and to pump the water out of shafts from which the ore has been already drawn.   If we refer to them at all, it would be merely to throw on them the light which the exacter evidence of recent investigations has kindled, and thus to disentangle, if it may be, their complicated and conflicting histories.

The Board of Health, however, in their zeal for the abolition of quarantine, have denied the contagion and importation of the Boa Vista fever, and have stated their objections to Dr. M'William's Report.   This has called out a reply from Dr. M'William ; and we feel bound thus far to re-open the question.

It is of great importance to know what weight these objections have. Not only is the case by far the most important which has ever occurred, but it is the one on which the debates in parliament will chiefly turn.   Let us begin by repeating what we have formerly said, that here, as everywhere, truth is everything, and individual opinion nothing.   It is of no consequence to us individually, whether yellow fever be contagious or not.   All we want to know, is, whether it *is* or *is not*, since the decision must influence our conduct.   If, on certain evidence, we come to one conclusion, and if, on additional evidence, we see fit to alter our opinion, we should not hesitate to do so.   What we want is truth, and not triumph.   If in the Board's Report we find anything which shakes our faith in Dr. M'William's Report, we shall avow it.

The objections taken in the Report to the generally received opinion of the importation of yellow fever into Boa Vista, are contained in the body of the Report, and in two of the appendices.   To the Report there are affixed no less than four appendices—namely, one from Dr. Gilkrest, one from Mr. Howell, formerly judge at Gibraltar, and one from Dr. Browne ; the fourth appendix contains a notice of the last Inquiry into the Nature of Yellow Fever by the Army Board, with a special statement of his own opinions from Dr. Burrell, a member of that Board, for the use of the Privy Council.   Dr. Gilkrest and Mr. Howell occupy themselves chiefly with the epidemics of Gibraltar, and do not allude to Dr. M'William ; Dr. Browne has a special note, intended to invalidate Dr. M'William's evidence ; and Dr. Burrell has a single paragraph on the same point.

We have therefore to consider, in reference to this subject, (*a*) the statement of the Board ; (*b*) the note of Dr. Browne ; (*c*) the paragraph of Dr. Burrell.   We shall be as brief as is consistent with the importance of the subject.

(*a*) The first objection taken by the Board of Health is a preliminary one.   They deny the evidence.   They consider that it was collected by Dr. M'William in the loosest possible manner, and was in itself exaggerated, highly coloured, and altogether incorrect.

To this we might have replied, by alleging that Dr. M'William went out

with a full knowledge of the gravity of the inquiry, and that he adopted the only course open to him, namely, that of examining *every* person, and putting down their names;—and by remarking that the internal evidence which may be collected from the Report itself, proves that it was collected with great care;—had not the Board at once abandoned their own position, and after stigmatizing Dr. M'William's facts as altogether inaccurate, proceeded to argue upon them as if they were quite certain ?   This appears to us a mistake both in law and in medicine. If the facts are unworthy, reject them; if not, receive them; do not reject and receive them in the same breath.   However, the Board receive the facts, and then try to overturn the conclusions. Their first attempt is singularly unfortunate.   On reference to our first article it will be observed, that two Portuguese soldiers, who had been thirteen months at Boa Vista, formed part of the guard which watched the sick of the *Eclair*.   These two men died of a disease similar to that of the seamen of the *Eclair*.   On this the Board remarks :

"Two European soldiers, lately arrived in the colony,* and therefore peculiarly predisposed to an attack of endemic fever,† go from Boa Vista, which at that time was healthy, to a confined, unventilated, overcrowded, and filthy spot, on another island, where fever was raging to such a degree, that within the space of three weeks there had occurred no less than sixty attacks and thirty-three deaths in a crew consisting on the arrival of the ship of one hundred and seventeen officers and men.   We submit that there is in this no evidence of the propagation of disease by a specific contagion ; on the contrary, it is the ordinary production of disease by its ordinary cause—namely, exposure to a polluted atmosphere, the pollution being in this case excessive from overcrowding, from accumulation of filth, from foul and offensive privies, from the impossibility of the admission of fresh air, owing to the construction of the building, and from the intense and oppressive heat, the ther-mometer ranging from 80° to 86° Fahr.   The seizure of two men with fever under such circumstances is precisely analogous to the case to which we have so often directed attention—the attack of persons previously healthy with typhus, who take up their abode in the crowded and filthy courts and alleys of English towns." (Report, pp. 101—2.)

The firmest believer in contagion could require no other expression of the facts than this.   Unwittingly the Board have admitted everything. According to their own showing, the " polluted atmosphere" about the sick gave two men previously healthy, the disease under which the sick laboured.   And, as if to clench the argument, the Board refer to the case, " precisely analogous," of a man catching typhus in a crowded London court.   This is, in fact, as strong a contagionist statement as could be made.   The Board do not indeed say, that in that " polluted atmosphere" the *specific cause* of yellow fever hovered; but they could be pushed to this admission, and could not indeed escape from it, unless by adopting that untrue and unphilosophical view which makes no distinction between diseases and their causes, but confounds in one chaos English and Tropical fevers, and regards, in fact, all epidemic diseases as but so many mani-festations of one changeable affection.   This is a view which the Board have more than once approached, but have never announced without qua-

* Thirteen months.—Rev.

† We have not time to notice this assumption, but refer to our review of Dr. King (vol. ii. p. 167). We may only remark, that slight remittents and intermittents were the only endemic fevers known for fifty years at Boa Vista; and that the endemic fever which could kill in three days, with black vomit, was altogether unknown there.—Rev.

lifications amounting to a surrender of it. It is, however, the view they must now take, unless, adopting the usual reading of their own words, they admit the contagion of yellow fever. The Board, in fact, must suppose that yellow fever and that typhus arise simply from overcrowding, and own no specific cause; which is simply to affirm, that the *same* causes give rise in different places to *different* diseases—diseases than which none in the nosological chart are more distinctly separated by absolute incongruity of symptoms. To those who are trained in the philosophy of medicine, who know that divers effects own divers causes, and that constant effects point to constant causes, the admission of the Board that two men died of yellow fever from being in an atmosphere "polluted" by yellow-fever patients, is conclusive.

After these two men died, a third soldier, a negro, was taken ill. The Board say that the explanation they have formerly given will suffice to explain also this man's attack—they have, however, omitted one material circumstance. The Europeans, say they, had lately arrived in the colony, and were therefore "peculiarly predisposed." Now this negro had *not* lately arrived in the colony, and from race and residence was not "peculiarly predisposed." It does not do to insist upon a condition in one case as important, and to abandon it in the next as immaterial.

This negro was removed to another place, and was attended by a woman named Anna Gallinha, who cooked for him, nursed him, and, according to Dr. M'William, performed for him and for a comrade, services less simple and innocent;*—this woman was attacked with yellow fever and died. How do the Board account for this? They adopt Dr. King's suggestion, that there were local causes near the house to which this man was taken, and that Anna Gallinha's illness was owing to these. Would it not be an almost incredible coincidence, that, for the first time for fifty years at least, certain local causes of moderate intensity, and whose very existence is denied by some, but which if they existed must have always existed, should have produced in this woman a fatal yellow fever, at the same time that a steamer, having on board the same disease, had appeared at the island, and at the same time that the woman had to nurse a soldier who, it is admitted, had caught the disease "in the polluted atmosphere" which surrounded the sick of the steamer? But if we made such an assumption, it would be merely one among a dozen such which we should have to make.†

The Board, then, like Dr. King, show themselves ready to adopt any but the most obvious explanation of these four admitted cases of yellow fever. In the cases of the two Portuguese soldiers, it is predisposition and exposure to an atmosphere polluted by sickness and overcrowding; in the case of the negro, the predisposition is dropped, and the "polluted atmosphere" is omnipotent; in the case of the woman, both predisposition and atmosphere are discarded, and a pool of stagnant water, and a "locality to which persons resort when obeying the calls of nature," are supposed to give off emanations which produce an identical disease

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* It is a curious circumstance, which has now been frequently noticed in the Southern States of the North-American Union, that the sexual desire very commonly undergoes an extraordinary augmentation in yellow-fever patients.

with the atmosphere polluted by the sick. But is this reasoning? Is it sense? Can anything be more pitiful than such subterfuges? Why does not the Board at once deny the fact that Anna Gallinha died of yellow fever, and thus be at any rate logical and consistent?

The next statement of the Board is so extraordinary, that when we read it, we could scarcely believe it possible that an able physician and a lawyer are members of the Board, that a physician and a surgeon are attached to it, and that some of its junior officers, who are not medical men, have yet in their own opinion so mastered the subject, as to come forward in public to debate the question. With the facts, the names, the very dates before them, the Board say, that after the case of Anna Gallinha—"the presumed chain of evidence stops; there is no further link traceable; there is nothing really connecting the illness of Gallinha with the next cases." (p. 103)

Why, the very next case which occurred was *in the person of a woman named Joanna Texeira, who lived with and nursed Anna Gallinha*, and who was attacked on the 19th of October, seven days later than Anna Gallinha, and three days after her death. A man, named Manoel Affonso, was also said to have been often in her house, and was taken ill the day after she died; that Manoel Affonso often visited Anna Gallinha during her illness, appears from the evidence of John Jamieson. (Dr. M'William's Report, p. 26.) His widow, however, denied to Dr. M'William that her husband had visited Anna Gallinha. (Op. cit., p. 85.) The Board of Health quote the widow, but say nothing about Jamieson's counter-evidence. Whether Manoel Affonso did or did not visit Anna Gallinha may be uncertain; certain it is that he lived only twenty yards away, and therefore Jamieson's statement may be correct. But here we discovered that the Board, in order to arrive at the fact—that the widow of Affonso made this statement—must have read over the part of Dr. M'William's Report, in which the attacks of Joanna Texeira, of Gertrude Bent, of Antonio Perica and others, composing the early cases, and who visited Anna Gallinha or each other,* are recorded. Yet, with the very names staring them in the face, the Board coolly assert that after Anna Gallinha the chain of evidence stops.

So far from the chain of evidence stopping in reality (although it does stop in Dr. King's Report, from which the Board have perhaps taken this assertion), every link is most complete. Let us recapitulate the facts. At the place where Anna Gallinha lived, the row Breira in Pao de Varella, there are only four houses, and two or three at a few yards' distance. In our first volume (p. 56) we have given tables of the residents and of the cases of fever; and we found that " there is not mentioned in Dr. M'William's Report the name of a single person who lived in Pao de Varella, or who was in personal communication with the sick soldiers, who was not attacked with the fever at the time." So much for the chain of evidence being snapped.

Leaving this part of the Report, the Board then allude to the case of Pathi, the labourer of Rabil. For this case we refer to vol. i. p. 57, and vol. ii. p. 168. There is some discrepancy between the evidence of Drs. M'William and King with respect to dates, and in our former article

† See vol. i. p. 56.

we were inclined to adopt the statements of the former. We need not go over the ground again—the main facts are certain. The Board, however, commit here a great error; they state that "a child in another family at Rabil, having no communication with the family of Pathi, died about the same time as Pathi's first child." (p. 105.) This is certainly erroneous, as the child referred to lived next door to Pathi, and the mother, who had been a good deal in the house of Pathi during his illness, was attacked with the fever before the child. As Dr. M'William very justly remarks, "This alleged non-communication is most unwarrantably assumed, and is wholly unsupported by evidence of any kind."*

The Board then refer to the negative evidence derived from the fact that certain persons in communication with Captain Estcourt and his crew did not take the disease, and that the washerwoman did not suffer. This negative evidence is of little value, and we have formerly attached to it all the weight it deserves.

Before turning to Dr. Browne, we may remark that the Board state, after Dr. Stewart (Report in the Admiralty Correspondence), that yellow fever prevailed in the adjoining island at Porto Praya. This has already been fully contradicted in the evidence of Mr. Macaulay, late commissioner at Boa Vista, and of Mr. Miller, a visitant at Porta Praya at the very time.†

We may also observe, that the Board do not allude to the very strong and indeed incontestable evidence for contagion derived from the history of the attack in the *Eclair* herself, apart altogether from Boa Vista. And, finally, we may quote the following recapitulation of the Board, as a tissue of the most singular errors we have ever read. We have inserted in brackets notes of assent or dissent, and are prepared to prove their correctness.

"It is admitted," say the Board, "that the Cape de Verde Islands are within the yellow-fever zone [*true*], and are liable to frequent and severe outbreaks of epidemic fever." [*Quite false, as regards Boa Vista.*] "It is admitted, that the physical and social conditions are eminently those which are found by universal experience to localize epidemic diseases, whenever an epidemic influence is present." [*This may be true, but yet Boa Vista had been healthy for fifty years, and no epidemic has ever been recorded until this one.*] "It is admitted, that the *Eclair* arrived at Boa Vista, at the season of the year when endemic fevers usually prevail." [*Quite untrue; the slight intermittents and remittents appear during the rainy season in the autumn. The yellow fever appeared during beautiful weather.*] "It is admitted, that at the very time of her arrival yellow fever was actually prevailing at Porta Praya, in the island of St. Jago." [*Quite false, as proved on incontestable evidence.*] "It is admitted, that some time before the outbreak of the epidemic, the atmospheric and other conditions which usually precede and accompany the development of epidemic disease, were so manifest as to attract general attention." [*Untrue.*] "It is proved, that sporadic cases of the disease appeared, as is usual some time before the presence of the epidemic was declared in its distinct and recognised form." [*Sporadic cases had occurred in some of those who were in contact with the crew of the* Eclair, *but were not scattered about.*] "It is admitted, that the epidemic influence extended to animals as well as man, a mortal epizootic disease prevailing over the whole of the island at the same time." [*Untrue; the mortality among the cattle was very slight, and not more than occurs every year, from famine*

before the rains.*] " It is proved, that the epidemic did not break out until about a month or six weeks after the *Eclair* with all her crew, healthy and sick, had left the island." [*Entirely and utterly false.*] " It is admitted, that a similar epidemic appeared among men and animals the following year, not imported, but entirely of local origin." [*Untrue, as regards animals; so far true, as regards men, that there was a revival of the disease.*]

So much for the dicta of the Board—not one of their conclusions is true—a child might answer them. It pains us to speak in such terms of men whose private character we respect; but personal feelings must not blind us to the interests of truth.

(*b*) The " note" on the fever of the *Eclair* communicated by Dr. Browne to the Board of Health (p. 306), is to our minds a document of very little weight. There is a great deal of petty criticism in his " note," which does not in the least bear on the real merits of the case. Dr. Browne thinks it an argument that neither the washerwoman nor Captain Estcourt introduced the disease, or Jamieson, or Dr. Henry, or Almeida, who visited the ship. These negative instances are of no value.† When we come to the main facts of the case, we find that Dr. Browne cannot deny the deaths of the two Portuguese soldiers, but places their death on the 20th or 21st of September, instead of the 17th and 18th. We have already given these latter dates; the alteration is of no value one way or the other.

Some remarks on the illness of the corporal of the first guard over the sick of the *Eclair* are curious. This man was attacked with some kind of illness, the nature of which cannot be known, but which does not seem like yellow fever, and was not considered to be so by Dr. M'William. Dr. Browne, however, admits that this man had yellow fever, and therefore so far strengthens by another case the presumed derivation from the *Eclair*. But why does he so admit it, and thus strengthen the hands of the contagionist? Simply to argue against contagion, because, although the patient was taken to the barracks, no one in the barracks had the disease at the time. This is indeed blowing hot and cold.

With respect to the two soldiers who were sent to Pao de Varella, and were nursed by Anna Gallinha, Dr. Browne appears to argue, that at this time the disease was in its incubative stage only, and that therefore they could not have communicated it. Now, it appears‡ that, to use the words of one of the soldiers, " he was still complaining when he went to the barracks;" that is, when he was removed from the vicinity of Anna Gallinha. If this does not mean that the man was ill when he was attended by Anna Gallinha, what does it mean?

After a careful study of the rest of Dr. Browne's note, we are unable to discover what is his precise meaning. He appears to object to the presumed introduction of the disease into Rabil by Louis Pathi, because no person was taken ill at Moradinha, at which place Louis Pathi was ill for eight days before being removed to his own house, and because a child of Manoel Fachina had died before either of Pathi's children. But, as we have

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* Further Observations, &c., p. 16.

† It appears, if the thing were at all material, that the clothes and bedding of all deceased persons were thrown overboard. The officers' clothes only were washed on shore, and two officers only had had fever.

‡ Dr. M'William's Report : evidence of Miguel Barbosa, p. 23.

before remarked, Manoel Fachina lived next door to Pathi, and his wife, who was ill before her child, was often in Pathi's house. The illness of the child of Manoel Fachina does not in the least weaken the case; it rather strengthens it. In fact, we placed in our list (vol. i. p. 58) the wife and child of Manoel Fachina as the two first cases after Pathi.

As a specimen of Dr. Browne's mode of arguing, we may quote the following. He says, at page 314, "The replies to 683 and 1253 would seem to render it by no means a '*positive* fact,' that even Louis Pathi was the first person attacked in Rabil."

We confess we were startled when we read this, and were afraid we had overwhelmed some important evidence. However, on turning to No. 683 and 1253, we found the *doubtful* (*!*) evidence as follows. Domingos, a mulatto, is asked:—683. "Who, to the best of your knowledge, was the first person who had the fever in Rabil?—I think it was Louis Pathi."

That it was Louis Pathi is certain, from other evidence; but because one witness says "he thinks," this expression is to invalidate his own almost certain, and the certain belief of all others.

The other reference is as follows:—

Manoel Fachina is asked:—1253. "Had your wife been to visit any sick person?—Yes; she had been a good deal in the house of Louis Pathi, who was sick among the first, if not the very first, at Rabil."

Is this expression sufficient to warrant such an assertion as that made above by Dr. Browne? If so convincing to his own mind, as to throw doubt on the other evidence proving that Pathi was first ill, it cannot be equally so to others. At any rate, Dr. Browne should have quoted the questions and answers.

(*c*) The paragraph by Dr. Burrell (p. 411) calls for only one remark. His main objection is as follows. "The two soldiers," he says, "who are supposed to have communicated the disease to Anna Gallinha, were not attacked for several days after intercourse had ceased between them and Anna Gallinha." Is it possible that Dr. Burrell has read the evidence given by Barbosa? (Dr. M'William's Report, p. 23.) "He was still complaining," he says, "when he left Pao de Varella, and was in bed in barracks on the *next day*."

Is it possible to arrive at any just conclusions, if men are thus permitted to misrepresent and mistake facts? No man is to be blamed for error of judgment; but he is to be blamed for culpable inattention, and for assertions which are falsified by the very evidence on which he relies.

Some may think that we have spoken harshly of the erroneous statements of the Board of Health, and of their witnesses. But we entreat those who think so, to glance for a moment at the gravity of the subject. This is no trifling debate, no theoretical point on which it would not matter what opinions were held. The lives of thousands, it may be, will depend on the legislative acts of this country in reference to quarantine, which will be prompted by the Board of Health. Is it not, then, our duty to point out, to the utmost of our power, that in the most remarkable case of imported yellow fever that has ever been known, the Board of Health have, with an inconceivable audacity, dared to falsify and misrepresent

evidence, and either from ignorance or incapacity, have striven to commit the English Parliament to a course of conduct that cannot but be perilous, and may be fatal ?

The Board of Health, on behalf of the government, sent a delegate to the Conference lately assembled at Paris, to discuss the subject of quarantine. The Report of this Conference has not yet reached us, but we most fervently trust that the case of the *Eclair* has not been represented in the light in which it appears in the document before us. If so, it must have misled the Conference, and may be followed by even more danger than the acts of the English Parliament. We shall wait with anxiety for the publication of the minutes of the Conference, and of its conclusions.

We have already said that we do not intend to discuss at length the other evidence brought forward by the Board of Health. We will merely enumerate the epidemics noticed in the Report. " In the following examination," say the Board, " we propose to include all the cases on which any material reliance is placed, not omitting one, as far as we know, which would in the present day be regarded as deserving consideration." (p. 7.)

The first case is that celebrated instance of the *Hankey*, on which whole libraries have been written. A few pages, taken almost entirely from Bancroft's work, dispose of it. Secondly comes the case of the *General Elliott*, described also by Chisholm, which receives twenty-seven lines of comment. Thirdly, the Cadiz fever, in 1800, receives two pages. Fourthly and fifthly, the Gibraltar fevers of 1810 and 1813 receive three and a half pages of comment. Sixthly, the Barcelona fever of 1821 is noticed. Seventhly, the *Bann* fever has seventeen lines devoted to it. Eighthly comes the *Eclair* controversy, which occupies no less than *thirty pages*. Ninthly, the Barbadoes fever of 1847 is alluded to; and, tenthly and lastly, the epidemic in Gibraltar in 1828 (on which Mr. Howell has written a Report, which appears in the Appendix), is discussed in eight pages. Thus, in fifty-eight pages, it is to be presumed by the government that they have an accurate summary of controversies, which have occupied several hundred volumes.

Besides this, the Board have entirely omitted all the American epidemics of the last or present century, and we do not think Dr. Rush's name occurs once in the Report; they have left unnoticed all the instances mentioned by Gillespie; they do not refer to the numerous treatises of Pariset; and, what is most singular, they choose to pass over all the epidemics of Sierra Leone, although, not three years ago, a full history of each was published by Dr. Bryson, of the Admiralty, and was reviewed in this[*] and in other journals. And yet they say no material case has been omitted !

But even in the cases they have noticed, the evidence is most meagre and most inaccurate. Thus, to take one example only, the introduction of yellow fever into Ascension by the *Bann*,[†] is thus disposed of.

" This case was examined by Sir William Burnett, who published an official report upon it, from which it appears that Sir Gilbert Blane was in error as to the state of the health of the crew of the *Bann* when she left Sierra Leone; that disease of precisely the same character as that alleged to have been imported by

the *Bann*, had existed and been prevalent in the island at former periods;* that after the most careful inquiry, it was found impossible to trace the fever in question directly from the *Bann* to any individual of the garrison of Ascension, and that the first person attacked was certainly not known to have been in the ship, or in contact with the sick." (p. 89.)

We will not go again into the evidence of this case, but observe only that any one would believe from this passage, that Sir William Burnett had *disproved* the importation into Ascension, whereas the evidence obliged him to admit, *against* his previous opinions, that " there is just reason to believe that the disease was introduced into the island by the *Bann*."†

Were we to go at length into the statements of the Board, we should fill a volume instead of an article. We have, we trust, said enough to deter any one from quoting any of the assertions of the Board, unless he feels a pleasure in being corrected.

The evidence of the occasional contagion of yellow fever is now so strong, that the Board of Health are almost singular in their opinions. Thus, as remarked by Dr. M'William, the Medical press, with only one or two exceptions, is against them. The great Corporations, when they have pronounced an opinion, are also decidedly against them. Thus, in 1850, the opinion of the College of Physicians was requested by the Secretary of State. The College had three questions laid before them.

" 1st. As to the Bulam fever being *sui generis*, and distinct from remittent or the marsh fever of warm climates?

Answer. " After a very careful consideration of all the facts and arguments adduced on both sides, the College are of opinion that sufficient grounds have not been laid for stating that 'yellow fever' is a disease *sui generis*.

" 2nd. As to its being an infectious disease; that is, communicable from person to person, and likewise capable of being imported?

Answer. " It appears to the College to be sufficiently proved, that this disease is under certain circumstances infectious, and consequently that it may be imported.

" The principal circumstances under which the infectiousness of this disease is likely to be developed, would seem to be a high temperature and moisture of the atmosphere, particularly in unhealthy seasons, and when the influence of these causes is aggravated by local insalubrity of site, and by the absence of free ventilation.

" That the disease has been in some cases imported, the history of the epidemic fever which occurred in H. M. S. *Eclair*, and at Boa Vista in 1845, affords conclusive evidence.

" 3rd. As to the non-liability of persons to a second attack of that disease?

" The third question proposed, does not admit of being settled in a decided manner."

The Army Medical Board, consisting of a president (Dr. Andrew Smith, now Director General), and of four members (Drs. Spence, Miller, Burrell, and Pilleau), made in 1849 and 1850 a most lengthened inquiry into yellow fever.

The main conclusions were as follow :—

The president and three members conceived the yellow or Bulam fever to be a distinct disease. One member, Dr. Burrell, believed it to be a form

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* This is erroneous : a remittent fever was known at Ascension during the turtle season, but was always much milder.

† Official Report on the Fever of the *Bann*, by William Burnett, M.D. London, 1824, p. 53.

of ordinary, continued, remittent, and intermittent fevers. The president
and two members conceived the fever to be sometimes contagious, but
sometimes not to be demonstrably so. Mr. Miller thought it always,
and Dr. Burrell never, contagious. The president and three members
believed the yellow fever to be capable of being imported, and Dr. Burrell
dissented from the conclusion.

We believe, then, if any evidence whatever can be trusted, that the
yellow fever *may be* contagious, and *may be* imported. We have formerly*
examined at great length into the question of its origin and of its occa-
sional or universal contagion. We need not return to these points. It is
sufficient to our present purpose to come to the above conclusion,—for
this conclusion leads us at once to our second heading.

II. In what manner and under what conditions does the communication
of yellow fever take place?

The answer to this question would render necessary a full inquiry into
all the circumstances connected with yellow fever. That this fever, like
all other diseases, is profoundly influenced by general conditions of atmo-
sphere, and by the local conditions surrounding it, is undoubted. Because
we admit yellow fever to be sometimes contagious and propagated from
person to person, we do not intend to overlook the fact that it may spread
in other ways, and that contagion *per se* will not account for all the
phenomena of its course. The Board of Health occupy some seventy pages
in a short account of some of the peculiarities of yellow fever; of its
frequent limitation to particular spots; of the advantage of removal from
such infected districts to healthier localities; of the influence of over-
crowding, of filth, of want of drains, and of swamps and marshes. What
they say is perfectly true, and might have been extended with advantage.
That such attendant circumstances influence yellow fever, as they would
small-pox or typhus, is undoubted. As we have often insisted, complete
preventive measures should aim at the removal of these accessory causes;
and the establishment of quarantines, without regard to these causes, is an
absurdity. It is to build up a wall of brass against enemies, and yet to do
one's best to create those very enemies within the guarded circle.

But do these circumstances render yellow fever contagious; and will
their removal remove its contagion, or their increase heighten it? This
point is surrounded with doubts, and we feel that it is dangerous to give a
positive answer. We are inclined, however, to reply in the affirmative,
and to believe that a poison which may spread in various ways, besides by
contagion, may under particular conditions also spread by contagion. The
particular conditions are enumerated by the College of Physicians; and we
think that it is some or all of these, which are thus capable of grafting a
contagious property on a specific poison, which under other circumstances
may not be contagious.

What answer, then, should be given to a government seeking to know
whether quarantines are to be established against yellow fever?

We should say that quarantines are necessary in all warm countries
where yellow fever can appear; that the period of seclusion should not be
long, since it is probable that about eight days is the usual incubative
period, and about double this time would be safe; that the period of

* Vols. i. ii. & iv.

seclusion should be calculated from the termination of the last case, and therefore, that if even a ship came from an infected port, yet if she were sixteen days at sea without a case, she might at once be admitted to pratique; that the quarantine station should be at least five hundred yards from other habitations, and should be most thoroughly ventilated and cooled; and that the power of clothes to carry the contagion is yet undecided.

We should also point out that quarantines are, after all, only partial, and so to speak, temporary measures; and that considering the immense influence of locality on the development of yellow fever, the establishment of quarantine against danger from without, and the neglect of prevention against danger from within, is like shutting the gate against a mad dog, and straightway proceeding to inoculate with hydrophobia all the pet spaniels who have the free run of the house.

In this direction the labours of the Board of Health would have been most useful; and if they had admitted the occasional contagion of yellow fever, and had sought to mitigate instead of to abolish quarantines, and to direct attention more strongly to the accessory causes, they would have made a Report of the greatest utility.

Before concluding this article, we must refer to one argument of the Board, which is very characteristic of them and of the kind of feeling they represent.

" The necessity of intercourse," writes the Board, " between all the members of the human family, is one of the final necessities of our race. The policy of encouraging and facilitating that intercourse is one of the favourable distinctions of our age. The great discoveries in science, the wonderful facilities which have resulted from some of them for personal and commercial transit and correspondence, all have the effect, not only of connecting the remotest cities and towns of individual nations, but of bringing the most distant countries into close neighbourhood and familiar intercommunication with each other. But if it be true that plague and pestilence are capable of being imported from country to country, bringing devastation in their course, and that this calamity may be prevented, and can only be prevented, by placing a rigid barrier between one nation and another so as effectually to obstruct their intercourse, then there is a contradiction between the necessities and obligations of the human family and the physical laws of their being; a contradiction in the highest degree improbable, since we see it in no other part of nature, and which, therefore, ought not to be admitted as a truth, and much less acted upon as a principle, without the clearest and most indubitable proof." (p. 69.)

This argument is one from final causes, and its full meaning will be best brought out by a syllogism:

> Intercourse is a necessity of the human race.
> Contagious diseases prevent intercourse.
> Therefore contagious diseases do not exist.

The philosophy of this will be best exemplified by a little anecdote:

The Queen's harriers were formerly kept near the metropolis; and as, at that time, in the streets of London, dogs were beasts of burden, and dragged carts like horses, hydrophobia was a very common disease. The Queen's harriers were therefore put into quarantine, and were allowed to have no intercourse with their canine brothers of the metropolis. For a long time the harriers remained contented and sane, and did not desire to exchange calls with the plebeians of the town. At length, with the progress of

enlightenment, the aristocratic hounds got tired of their privileges, and the result was a public meeting, to consider their relations with the city dogs. The principal reformer, a dog of a steady aspect, with a quiet confidence of manner, and a low and rather melodious bark, made a speech, in which he remarked, that the necessity of intercourse was one of the final necessities of the canine family, that the policy of facilitating that intercourse was one of the favourable distinctions of the age, &c., and that if madness could be prevented by barring out dogs affected with hydrophobia, it would be a contradiction between the obligations of the canine race and the physical laws of their being. After enlarging on this head, the orator remarked that it was in fact ridiculous to ascribe the rabies which so affected their brethren of London to any but the local circumstances which surrounded them. It could scarcely be credited that, in the nineteenth century, dogs could be found who dragged carts, who slept on door-steps, who never washed, and who licked bones. Such habits, of course, produced hydrophobia. But if these dogs were fed with chopped meat, dwelt in kennels like the harriers, took a little brisk exercise on a fine day, and had their feet washed when they came in from a run, madness would soon be unknown. Rabies was, then, produced by local causes; being thus caused, it could not of course spread beyond the sphere of those local causes; and to interrupt commerce and prevent intercourse on such an account, was an absurdity worthy only of the middle ages.

This speech was hailed with enthusiasm, and the dogs, showing their teeth, would soon have done away with all precautionary measures, had not an old hound, who bore the character of taking good care of number one, suggested that a little delay was advisable, that they were well to do in the world, that they did not know what madness was, and that from what he had heard of that unpleasant disease, he would rather keep it on the other side of the wall. He agreed, he said, with the philanthropic cur who had just lain down, that those local causes were of great importance. Still, he put it to the assembly, whether, in a question of this kind, it was not the duty of their London brethren to do away with these local causes, before they sought to be admitted to their society on equal terms. He had never spoken to a street dog, but he had heard him snarling, and he thought him both dirty and dangerous. It did not appear obvious to his friend's capacity, but yet, after all, he thought it possible, that if madness was created by dog-carts and over-driving, it might get into the blood and come out with the saliva. He admitted that the necessities and obligations of the canine race as to intercourse and brotherhood, and the physical laws of their being, would be in opposition, if it were certain that dog-carts and over-driving and want of water were among those physical laws. But he did not hold such to be the case; and not to occupy longer their precious time, he would conclude with this declaration of opinion, that if the London dogs would give up dragging carts, take to water, be cleanly in their eating, and reform their drainage, he should then, but not till then, be happy to abolish quarantine.

Every one knows that, perhaps in consequence of this advice, dog-carts were given up in London, hydrophobia disappeared, and quarantines became as obsolete as the disease against which they were formed. But if the process had been reversed, if dog-carts had been continued, and quarantine

discontinued, does any one doubt that hydrophobia, though (according to the hypothesis) produced by local causes, might not have been carried to every town in England?

Improvement may begin at a right or a wrong end; and in the case of all infectious and contagious diseases our object should be, to learn how to disarm the contagious power, and not to twist evidence, and cheat ourselves into a scepticism which daily experience should destroy. If it be true that " sanitary precautions and not quarantine lines are the safeguards of nations," it is not less true that till those sanitary precautions have been adopted, quarantines offer the only means in our power of keeping at bay some of the most terrible diseases which can afflict mankind; and those who destroy such a safeguard may have to answer for a slaughter more terrible than the bloodiest campaigns of Napoleon or Genghis Khan.

ART. II.

1. *On Syphilis, Constitutional and Hereditary; and on Syphilitic Eruptions.* By ERASMUS WILSON, F.R.S. 8vo, pp. 235.

2. *Blennorrhagia and Syphilis; their Nature and Treatment; being an Analysis of the Letters of M. Ricord.* By M. H. STAPLETON. (From the 'Dublin Quarterly Journal of Medical Science.') 8vo, pp. 61.

MR. ERASMUS WILSON, it is perhaps superfluous to say, is already favourably known as an author, especially by his excellent works on Diseases of the Skin. Mr. Wilson's present publication has grown out of his endeavours to elucidate the diagnosis of " eruptions, which proceeded from ordinary causes, from those originating in syphilis; and having before me" (he goes on to say) " an ample field of research, I determined to investigate the matter as it was presented to myself, and without reference to the opinions and labours of others in the same department." (p. vii.) This determination is, we think, to be regretted. It is unsafe, now-a-days, to investigate any subject, without full and careful reference to the information accumulated by preceding competent inquirers in the same department. Letting that pass, however, we have only to examine the contents of the volume before us; and previously to doing so in detail, it will be convenient to give a summary of the more prominent results which Mr. Wilson has deduced from his independent method of research. Mr. Wilson, however, does not clearly specify in a distinct and consecutive shape the several conclusions to which he has been led by his investigations; but we believe that everything in his views which he considers either peculiar, on the one hand, or specially important, on the other, is embodied in the following assertions, propositions, and claims of discovery, which we express, so far as is practicable, in the author's own words.

1st. One of Mr. Wilson's " first results was the discovery, that there existed but one syphilitic *eruption*, and that the apparent differences in the character of the cutaneous affection were," consequences of modifications, chiefly depending " on time, treatment, and on the temperament of the patient." (p. vii)

2nd. Our author's inquiries " tend to the conclusion," that " syphilis, in all its multitudinous and Protean shapes, originates in *one poison*"

(p. viii.); but it does not clearly appear whether Mr. Wilson claims this also as a discovery.

3rd. Mr. Wilson believes, that though the contact of the syphilitic virus with a mucous membrane commonly produces an ulcer, yet that it may merely cause " a common gonorrhœa," and further, that " in these cases, constitutional syphilis will follow with as much certainty as if it were preceded by a chancre." (p. viii.)

4th. " I trust, also," says Mr. Wilson, " to have cleared up the mystery which enveloped the induration of the true chancre of Hunter. That induration I consider to be the result of a constitutional action, and, consequently, an evidence of the contamination of the system." (p. viii.)

5th. The following series of conclusions, if all well founded, are some of them formidable enough. At p. ix. Mr. Wilson says, " the tenacity of the syphilitic poison to the human organism cannot but lead to the conclusion, that *once admitted* into the blood and tissues, *it remains there for life.*" [The italics here are our own.] Moreover, syphilis may lie completely masked throughout the whole duration of life, and yet an individual may be " so saturated with the virus, as to possess the property of communicating syphilis to a sound person by means of his secretions." (p. 3.) And not only may an apparently healthy, yet really contaminated person, transmit syphilis to his immediate offspring, but, adds Mr. Wilson, " I am firmly of opinion, that the powers of the poison may be manifested after the lapse of several generations." (p. ix.) Moreover, the malady may sustain various metamorphoses in the process of transmission, for " what is syphilis in the parent may be scrofula in the child;" and further, " lupus, kelis, lepra, and psoriasis may take their origin in hereditary syphilis." (p. x.)

6th. Finally, Mr. Wilson seems not far from admitting the spontaneous evolution of syphilis. " The syphilitic poison" (he says, p. 34) " originates in the human body; it is probably little more than a modification of the natural secretions."

The second book whose title we have above transcribed, is an epitomised translation of a series of letters lately published by M. Ricord in the *Union Médicale,* with occasional observations and comments by Mr. Stapleton, the translator. It is quite unnecessary to premise any summary of M. Ricord's doctrines, inasmuch as our habitual readers must be thoroughly familiar with them, even if they had no other source of information on the subject than this journal; and there is nothing new in the present exposition of M. Ricord's views, that calls for prefatory explanation. Indeed, these letters have disappointed us a good deal. M. Ricord complains that " his new doctrine of syphilis has met the same fate with all scientific discoveries." Despite his incessant labours for the last twenty years, the malevolent will not, and the dull cannot, understand him; and, consequently, he is subjected to the constant mortification of seeing his opinions misrepresented, and futile objections advanced to his doctrines. Fortunately, however, M. Ricord's philosophical tranquillity is proof against such provocation; he " is neither astonished nor indignant;" far from it; on the contrary, he " thanks his adversaries for having presented him with a new stimulus to exertion." (pp. 1, 2.) Now, if M. Ricord were thus persecuted, he would only share the fate of many

other great men; but we really think he has singularly little cause for complaint. His publications and opinions have received, to say the least, fully as much attention and respect as they are entitled to, and he has numerous unquestioning admirers, who adopt his dicta wholesale, without doubt or demur. True it is, that objections have been advanced to many of his opinions, by persons whose authority is nothing inferior in weight to his own; and M. Ricord may rest assured, that these objections will continue to be advanced, till he brings forward more convincing evidence in support of his disputed opinions than he has hitherto done. But M. Ricord's " adversaries" will scarcely be converted into believing disciples by his letters to the *Union Médicale;* they ask for proof respecting debated and debatable points, and he has replied by merely repeating his former dogmatic assertions.

Mr. Wilson's book presents some marked contrasts with M. Ricord's publications. M. Ricord is authoritative, peremptory, and magistral, but Mr. Wilson is very far from putting forward his opinions as demonstrated facts. With the most praiseworthy candour he says, almost at the outset of his work, " I have contented myself by merely *mentioning my belief,* and adducing some *slight evidence* in support of my opinion. The fruit may ripen in other minds, or time and observation may afford me an opportunity of gathering *stronger evidence,* and at some distant day of placing the results in the hands of the profession of medicine." (p. x.) In the meantime, we must rest satisfied with examining Mr. Wilson's opinions, and the admittedly slender proofs which he offers to sustain them.

The English and the French author also differ very widely in another respect; and here the advantage lies entirely with the latter. M. Ricord is consecutive and methodical in his arrangement, and clear and precise in his language; and consequently he is, at all events, always intelligible; but we must observe, by the way, that the qualities last noticed have been somewhat impaired by his epitomiser and commentator. Mr. Wilson, on the contrary, is vague, unmethodical, and confused; and we have to gather his views, and what he offers by way of arguments and proofs, bit by bit, as the fragments lie scattered here and there throughout the work. It is hence really impossible to examine Mr. Wilson's book in the order, not to say disorder, in which it is written; and we shall therefore, although it is our intention to discuss Mr. Wilson's views and statements rather than those of M. Ricord, rather follow the arrangement of the latter writer.

After some preliminary observations, intended to show that " an era of confusion," respecting all matters relating to venereal disorders, existed previously to his own investigations, M. Ricord proceeds, in his first letter, to deliver his oracular dicta concerning " Blennorrhagia." M. Ricord, our readers are doubtless aware, not only utterly rejects the doctrine that gonorrhœa and syphilis arise from the same cause or specific virus,—maintaining that a *concealed* chancre must exist or have existed in every case, where either the known symptoms of constitutional syphilis follow a urethral discharge, or where the inoculation of the matter of that discharge produces a characteristic chancre,—but he also denies that gonorrhœa, properly so called, is a contagious disease, or that it is produced by a peculiar and specific virus. To the former position we are certainly disposed to

assent; the latter is, we think, quite untenable. Here is a disease, charac-
terized by symptoms as definite and constant as are those of, perhaps, any
of the confessedly contagious and specific maladies, and capable of being
transmitted and of reproducing these symptoms an unlimited number of
times, to and through an indefinite number of persons.   If such an
affection is not a contagious and specific disease, we should like to know
what is the precise signification attached by M. Ricord to these phrases.
Indeed, the discussion, as managed by M. Ricord, merely throws us back
upon the definition of terms.   At one time, M. Ricord writes as though
syphilis were the only specific malady.   Thus, having put the question—
" Must blennorrhagia be ascribed in all cases to a specific cause ?" he
replies as follows:

" Hunter had proved that inoculation with the pus of a chancre produced a
chancre.   If, then, *blennorrhagia* must be ascribed to a *specific* cause, the muco-
pus which *it* secretes being inoculated, will *indubitably* produce phenomena similar
to those which the inoculation of *pus from a* chancre produces." (p. 5.)

The *non-sequitur* is here too palpably obvious, to need that we should
dwell upon it; if the argument were valid, it is plain that no morbid
secretion can be of a specific nature, unless it causes a chancre when
inoculated.

M. Ricord, a little further on, disputes both the incubation of gonorrhœa
and its specific nature, because " the period of incubation fixed by Hunter
and others was from a few hours to fifty days; a very *elastic* incubation
indeed,"—and then he goes on to say:

" In virulent diseases, where incubation is incontestable, the limits can be more
clearly determined, as in syphilis, in scarlatina, in measles, &c.   Even in plague,
owing to the successful labours of M. Aubert Roche, the period has been deter-
mined to be eight days." (p. 7.)

It is quite beside our purpose to inquire whether Hunter correctly fixed
the limits of time within which gonorrhœa may appear after impure con-
nexion; but even if the period of incubation were as *" elastic"* as Hunter
supposed it to be, that circumstance, *per se,* would do nothing towards
proving that gonorrhœa is not a virulent or specific malady.   If we admit
that syphilis, scarlatina, measles, and plague, have each, respectively, a
brief and well-defined period of incubation (a point we do not stop to
examine), it by no means thence follows that the same holds true of every
other specific disease and morbid poison.   In hydrophobia, for example, to
take but a single instance, the period of incubation varies, in the dog, from
a few days to certainly upwards of seven months, and probably much
longer; and it is equally, if not more undetermined in the human
subject.

In M. Ricord's observations respecting the seat of gonorrhœa, we find
the following passage :

" Observation teaches us that the parts of the mucous membrane *most exposed*
are those that are *most* easily affected.   Our author, however, admits that, as
regards sexual intercourse, the urethral mucous membrane in both sexes is the part
most frequently attacked." (p. 8.)

It is not for us to reconcile the conflicting dicta that the *most exposed*
mucous membranes are those most easily affected, but that, nevertheless,
the urethral mucous membrane, the *least exposed* of the mucous surfaces

concerned in sexual intercourse, is yet " the part most frequently attacked"
as a result of that intercourse. No explanation is offered of this little
difficulty; but M. Ricord and his commentator both consider it " incon-
testable" that " blennorrhagia of the urethra" is a malady contracted by
sexual intercourse with an individual suffering from the same disease. The
exudation of pus from the urethra, in fact, is to gonorrhœa what inoculation
is to chancre, an infallible diagnostic sign, which unerringly guides the
practitioner; and in doubtful cases in the female, Mr. Stapleton is

" In the habit of introducing a finger into the vagina, and pressing it against
the urethra, when if we find that pus is discharged from it, we pronounce the case
to be blennorrhagia occasioned by sexual intercourse." (p. 8.)

In fine, neither M. Ricord nor his commentator can discover any evidence
that gonorrhœa is a specific or contagious disease. All the seeming peculia-
rities in the phenomena of the malady, we are told, are completely explained

" By the sole and incontrovertible fact, that pus furnished by the urethra is
the most irritant of all varieties of pus, when applied to certain mucous mem-
branes." (p. 9.)

Where there is so much affectation of strict logical reasoning and rigorous
philosophical accuracy, it is amusing or provoking, according to one's tem-
perament, to meet with a passage like this. With the help of such
gratuitous and convenient assumptions, which it would be mere waste of
time to comment upon, any facts might be explained away, or any hypo-
thesis be apparently overthrown or established.

But it is one question whether gonorrhœa is a specific and contagious
malady, and another and a very different question, whether gonorrhœa can
cause secondary affections of the system, either closely resembling, or, as
some say, identical with, constitutional syphilis. Mr. Wilson, as we have
seen, admits the interchangeableness and entire identity of the two diseases;
but beyond the expression of his belief, he has left the question precisely
as he found it. The whole of what he says respecting gonorrhœa occupies
a few lines more than one page; and fully half of that limited space is taken
up with the notes of a case, which we shall presently refer to. Of the remain-
ing half, the following passage contains every syllable that bears upon the
point under consideration:

" The recognition of the contagion of constitutional syphilis, a fact too obvious
to admit of a moment's hesitation, will go far to explain a circumstance which must
have fallen under the observation of every unprejudiced investigator of the syphilitic
poison and its manifestations—namely, the occurrence of syphilitic eruptions and
other symptoms of constitutional syphilis after gonorrhœa. When we see a man
perfectly free from any primary symptoms of disease, communicating syphilis to his
newly-married wife, by his secretions alone, can we doubt the possibility of a similar
result accruing from a syphilitic secretion poured out by the mucous membrane, as
happens in gonorrhœa?" (pp. 19, 20.)

This is a fair average example of Mr. Wilson's manner of arguing. He
sets out by assuming that the contagion of constitutional syphilis is a fact
too obvious to admit of a moment's hesitation, and that the mere secretions
of a person labouring under secondary syphilis can communicate the malady;
and next he says, that this being the case, how can we doubt that the
syphilitic secretion of gonorrhœa can communicate it also?—here, again,
dragging in another assumption—viz., that gonorrhœa *is* syphilitic. Thus

he begs the very question that he is endeavouring to illustrate by an analogy, which is quite unfounded, as we believe, but which, at all events, is most certainly fallacious and inconclusive; for even if it were established that ordinary constitutional syphilis is contagious, that fact alone would not supply the slightest proof of the identity of gonorrhœa and syphilis. Mr. Wilson also cites a case, above referred to, to prove that constitutional syphilis may result from gonorrhœa. A medical man consulted Mr. Wilson for an eruption of syphilitic tubercles on the face. He had been affected with gonorrhœa twice, twenty-two and twelve years previously, and had two preceding attacks of symptoms of secondary syphilis. The patient "felt convinced that he could not have had a chancre in the urethra." (p. 21.) This is the substance of the whole history that is given of the case; it is not even said that an external chancre had never existed; and from these meagre and imperfect statements, Mr. Wilson jumps to the conclusion that gonorrhœa is capable of producing true secondary syphilis. Moreover, any imaginable weight that this case might be supposed to possess is completely annulled by Mr. Wilson's own doctrines; for, according to his views, as we shall presently see, this gentleman might have contracted his constitutional taint, by hereditary descent, from his father or his grandfather, or even from a remoter ancestor. Or again, his secondary symptoms might have been due to his connexion with a woman apparently enjoying the soundest health, but nevertheless tainted with concealed constitutional syphilis. In fact, Mr. Wilson admits of so many and such various inlets for syphilis, that, were his doctrines admitted, it would be almost hopeless, in any given case of secondary syphilis, to determine positively the particular avenue by which syphilis gained admission to the system. By force of extending the laws of the transmission of syphilis so as to embrace every apparent difficulty, Mr. Wilson would render it impossible to establish almost any one law beyond doubt.

It may be proper to state that Mr. Wilson does not consider every gonorrhœa syphilitic; on the contrary, he thinks that but few are so, " but those few," he says, " have as much the power of transmitting syphilis as an undoubted chancre." (p. 20.)

We have now given a faithful account of everything Mr. Wilson says respecting gonorrhœa; indeed, we have literally transcribed the greater part of what he writes on the subject, and our readers can now judge how exceedingly " slight" is the evidence brought forward by Mr. Wilson in support of his views. It is formally stated in the Preface (p. viii.), that the contact of the syphilitic poison with a mucous membrane can indifferently produce a chancre or gonorrhœa; but there is not a single fact or case so much as hinted at, to bear out that opinion.

M. Ricord, it is well known, entirely denies that gonorrhœa can cause consecutive general symptoms resembling those of secondary syphilis; and in that opinion we are inclined to coincide; but, at the same time, we do not pretend to maintain that this point of doctrine is absolutely and conclusively settled. M. Ricord says nothing new on this head; he simply reiterates his opinion, and complacently maintains that his experiments by inoculation have finally and for ever decided the question. Now we have already said that we ourselves incline against admitting the secondary action of gonorrhœa, excepting, indeed, the affection of the joints not

unfrequently consequent upon it, which, were we to imitate M. Ricord's courteous phraseology, we might say he is "blind" not to see. But most certainly M. Ricord's experiments are utterly insufficient to demonstrate that gonorrhœa never secondarily affects the system; and despite our own impression, we must not only admit that the question is not settled, but that the evidence is rather tending, as it accumulates, against the opinion towards which we have hitherto leaned. It would be vain to attempt here to marshal all the evidence *pro* and *con*, and useless to merely recapitulate the names of the authorities who maintain that constitutional symptoms follow gonorrhœa. We can now only notice one point, which we think is of considerable interest and importance. M. Ricord admits, in his larger work (p. 119,)—we do not observe the circumstance mentioned in his Letters,—that gonorrhœa is often accompanied by erosions or destruction of the mucous membrane, but he found that this ulcerated gonorrhœa was not inoculable; and not being inoculable, of course his theory absolutely forbids that it should be capable of contaminating the system generally. Mr. Travers, in his ' Observations on the Pathology of Venereal Affections,' gives an excellent account of these gonorrhœal ulcers, and he also describes a train of secondary symptoms which he maintains they are capable of producing. Now, Dr. Egan, for some time physician to the Westmoreland Lock Hospital, Dublin, has published in the *Dublin Medical Press*, December 10th, 1851, the results of his observations and experiments by inoculation on gonorrhœal ulceration, which form, as it were, a connecting link between the observations and opinions of M. Ricord and Mr. Travers. Dr. Egan found that the discharge from gonorrhœa accompanied by abrasions and ulcerations was *not* inoculable, but he nevertheless observed that the affection was followed by a mild form of secondary symptoms, generally a papular eruption. It is well to observe, that Dr. Egan is evidently a warm, though not a blind, admirer of M. Ricord; that his opportunities for making accurate observations were very favourable, and that he was fully aware of all the sources of error liable to vitiate his conclusions. These we give as follows, in his own words:

"1st. That the virus of gonorrhœa is different in its nature and properties from that of chancre : inoculation with the former never giving rise to a specific ulcer; while that of the latter, when inserted beneath the skin, during the state of ulceration, is generally succeeded by the characteristic pustule.

"2nd. That abrasions of the mucous membranes are likely to ensue from the irritating quality of a gonorrhœal discharge in its early or incipient stage; that although no appreciable effects result from the inoculation of these excoriations, still mild forms of secondary symptoms have been observed to supervene on this affection, where no other species could be detected on most careful and repeated examinations. In no case, however, has the matter of gonorrhœa produced venereal ulcers."

We do not know any point connected with the history of venereal affections, which it is more desirable to have fully investigated than this. It may afford the key to the solution of difficulties which now leave us in doubt and embarrassment. M. Ricord has arbitrarily taken transmission by inoculation as the sole and unerring test to determine whether certain forms of disease are specific, contagious, and capable of producing certain secondary constitutional affections. Let Dr. Egan's observations be confirmed, and this part of " the new doctrine of syphilis" at once crumbles to the ground.

Mr. Wilson, strange to say, is totally silent respecting the treatment of gonorrhœa.   M. Ricord deals most characteristically with this part of his subject, as follows:—" Many objections have been urged against the abortive treatment.   *Away with them*."

There is something Napoleonic in this method of endeavouring to settle a disputed point of practice; but as M. Ricord does not possess in the republic of letters such absolute authority as Louis Napoleon has in the republic of France, the objections to the abortive treatment will scarcely disappear at his mere command.   As M. Ricord thinks fit to advance nothing better in support of his favourite theory, than the foregoing brusque fanfaronade, we will spare ourselves the trouble of pointing out the disastrous results to which it too frequently leads.

We had intended, in considering Mr. Wilson's book, to follow the order of M. Ricord's Letters, but really Mr. Wilson's book defies all attempts to bend it under any arrangement.   The good old rule to begin at the beginning will not apply here, and we shall take Mr. Wilson's opinions respecting " hereditary syphilis" as a central group, with which almost everything further in his work that we shall have occasion to refer to, is more or less intimately connected.

Mr. Wilson, in discussing the question of hereditary syphilis, adopts as a fundamental proposition, that a person once constitutionally affected with syphilis, inevitably retains the taint throughout the remainder of his life.   The syphilitic virus, when it once enters the blood, is perennial, and Mr. Wilson might adopt *Semel infectus, semper infectus*, as the motto of his essay.   We shall state Mr. Wilson's doctrine in his own words, to avoid the possibility of misrepresenting his opinions:

" In reviewing the handful of cases," says Mr. Wilson, " collected together in these pages, if there were no other evidence of the fact, we could not do otherwise than come to the conclusion, that the poison of syphilis is of a most enduring kind; that *being once received into the blood*, it remains there for years, and possibly— indeed, *certainly—for the rest of existence*.

" In juxtaposition with this *admitted law* of the syphilitic poison, is another equally positive—namely, that a person possessed of this poison is capable of conveying it to another; and if that other be his wife, he may, through her means, convey it also to his child.

" Now, if it be true that the syphilitic poison once received into the blood remains there for life, the infected wife *must remain infected as long as she continues to live;* and, by a parallel reasoning, *the infected child must remain infected until death*.

" The question, then, comes before us—What if an infected child, grown to manhood, should marry? and with still greater force—What if an infected child, grown to manhood, should marry the daughter of an infected wife?   *There can be no doubt* but that *some evidence of the latent poison will be exhibited either by themselves or by their offspring*.   Such evidence is exhibited, and I feel convinced that a considerable proportion of those diseases which pass under the name of scrofula, are the produce of the syphilitic poison—are, in fact, *not scrofulous*, but syphilitic." (p. 158.)

But the metamorphosis of syphilis into scrofula is not the only transmutation that the former malady can sustain.   At page 165, Mr. Wilson asks, with all the emphasis of capital letters, " Is LUPUS NOT SYPHILIS ?"   And here, indeed, a doubt respecting the identity of scrofula and syphilis seems to have passed through Mr. Wilson's mind; for, five lines after the preceding question,

he again asks, also in capital letters, " Is scrofula syphilis?" The doubt, however, soon vanishes; and Mr. Wilson, passing from the interrogative to the dogmatic, enlarges the circle of maladies into which syphilis can be transformed, and includes " ordinary lepra and psoriasis" as products " of that widely-spread, *almost universal* poison, syphilis." (p. 166.) Mr. Wilson states his opinion to the same effect several times, and on each occasion is more positive in the expression of his belief. At p. 182, he says, " The *suspicion* of any connexion between these diseases . . . . has grown into a *creed ;*" and again, at p. 184, he repeats his creed in the following conspicuous form :

" In conclusion, I must repeat that I am deeply impressed with the belief that

<div style="text-align:center">

Lupus,
Kelis, and
Lepra and Psoriasis,

</div>

are forms of cutaneous disease, all having their original source in syphilis, all maintaining a relationship in different degrees of remoteness with that disease, and all, therefore, *falling into the category of hereditary syphilis.*"

We do not know whether the affections just cited, together with scrofula, are the only transmuted forms of syphilis recognised by Mr. Wilson; but from some expressions he uses, we suspect that he is inclined to refer a good many other maladies to the same original source. At page 182, he speaks of the alterations the parent disease must undergo, when " the poison has very probably been filtered through the tissues of several generations," and we have seen that at page 166 he terms syphilis " a widely-spread, *almost universal* animal poison." We shall not be surprised if some of the ingenious charlatans, so numerous in the present day, should develope Mr. Wilson's views into an entirely new system of popular pathology. Hahnemannism is rather old, and getting decidedly *rococco;* and the novelty-mongers could scarcely do better than proclaim syphilis the universal morbid modifier lying at the root of every disease. Suppressed and modified syphilis is, after all, a more plausible idea than suppressed and modified itch, and it has the advantage of being some degrees more repulsive, which might do something towards recommending it to public favour. But we must now endeavour to examine how Mr. Wilson supports his views.

And first, as to the ineradicability of syphilis, we must confess that we have failed to discover the necessary evidence in Mr. Wilson's " handful of cases." But then, Mr. Wilson says that this is an " admitted law of the syphilitic poison" (p. 158); on the contrary, it was always a very disputed doctrine, and long universally disbelieved, till revived, not, however, as *proved,* but merely as *probable,* by M. Ricord and his followers. Indeed, we go too far in saying that M. Ricord considers it *probable* that constitutional syphilis is indestructible ; it is more accurate to say that M. Ricord only maintains that the complete eradication of the syphilitic diathesis has not as yet been conclusively demonstrated. Mr. Wilson does not say how it is, or by what process of reasoning he deduces from his cases the inference, that constitutional syphilis is a life-long disease ; we can only suppose he infers it from the circumstance, that syphilis may lie latent in the system for years, and finally appear with undoubtedly characteristic symptoms. Few, we presume, will question that fact; though we are bound to say, if it rested on no better evidence than that which is supplied

by the cases given in Mr. Wilson's book, very few would admit it.   The fact, however, is conceded, for it rests on satisfactory evidence from other sources; but what warrant has Mr. Wilson to argue from the particular to the general, and to deduce an absolutely universal law from particular and exceptional cases?   In so doing, he sins against the most elementary and obvious rules of logic.   It is quite true that syphilis has held persons in its grasp throughout a long lifetime; that it may persist indefinitely; and that long years—Cazenave says thirty, and even forty years, within his own knowledge—may elapse between the disappearance of primary and the supervention of secondary symptoms.   But these cases, we repeat, are exceptional; they must abate our confidence in the efficiency of our remedial means, but they cannot serve to establish a general and inflexible law.   Exaggeration in one direction generally ends in engendering exaggeration in the opposite direction.   It is in the moral as in the physical world; the swing of opinion, like that of the pendulum, is apt to go an equal length in a contrary sense.   Hunter gave currency to the threefold doctrine—first, that the constitution, when unaided by mercury, had not, *per se*, any power in relieving itself from syphilis; secondly, that the disease, when left to itself, must inevitably continue to increase; and thirdly, that mercury has a specific power to eradicate the disease.   All these doctrines are erroneous.   Syphilis may exist in the constitution without progressing continuously; it may lie latent; but then, it may also wear itself out, without what is ordinarily termed antisyphilitic treatment.   We hold it to be a cardinal point in the natural history of syphilis, that the malady has a natural tendency to wear itself out.   This fact may be gathered from the older writers; but it appears more clearly from the experience of the non-mercurial school, and is, in truth, one of the most valuable results of the war between the mercurialists and the non-mercurialists.   So evident, indeed, is the fact, that the warmest mercurialists—men trained up in the Hunterian belief—admit that syphilis may occasionally get well without mercury.   This fact in no way invalidates the utility of mercury in the treatment of syphilis, for if the controversy has proved that syphilis *may* be cured without mercury, it has also established more firmly than ever the immense value of mercury in the treatment of the disease.   But then, again, modern observations equally show, in accordance with the experience of the older writers, that mercury has not the specific power of invariably eradicating syphilis.   Well, just as Hunter, in this respect, generalized too far in one direction, so is there a tendency now to generalize in the opposite extreme.   Some doubt, and Mr. Wilson positively maintains, that syphilis can never be eradicated from the constitution by mercury, or by any other treatment.

As to the transmutation of syphilis into other forms than those in which we commonly witness it, we have no preconceived ideas that would bias us against admitting the fact, were it supported by anything like sufficient evidence.   Indeed, our prejudice—if we have any on the point—rather tends the opposite way.   We every day see syphilis exhibit so many and such dissimilar outward characters, that we hold it to be unphilosophical to conclude hastily, that because the variations of the disease now assume certain forms only, such must, of necessity, always be the case.   We can scarcely understand how an impartial inquirer, not led away by the neces-

sity of upholding a theory, or not puffed up by an inordinate estimate of
the transcendent value of everything modern, and a thorough contempt of
everything old, can doubt that syphilis has undergone many and important
changes in its external manifestations—now presenting itself in its most
inveterate and intractable forms—then again, within a single generation,
and under the observation of men with as much brains in their head and
as good eyes in their orbits as any "modern syphilographer" (to use the
cant phrase), appearing in a milder and more manageable shape; so much
so, as to induce more than once an almost universal hope, in physicians
throughout Europe, that the malady might possibly wear itself out.   When
a malady, then, can thus alter its aspect, we cannot venture to dogmatically
deny that it *may* manifest itself in forms which we never suspected it could
assume.   Analogy also tells us not to dogmatize too confidently in such
matters.   M. Fabre published, but the other day, the discovery that a poor
little wild grass, the *Ægilops ovata*, and wheat are one and the same species—
that in twelve years the *Ægilops ovata* became wheat.   And to pass from
new forms of development, which cannot be termed disease, in the vegetable
kingdom, to diseased developments in the animal kingdom; look at what
occurs in glanders.   Who is there that, merely judging from the external
characters of the diseases, would identify acute glanders and chronic
farcy?   And yet their common origin cannot be disputed; nor can we, in
one sense, dispute their absolute identity; for chronic farcy may become in
the subject it affects, or may by transmission produce in another subject,
acute glanders; and reversely, acute glanders, though it cannot, so far as we
know, retrograde in the subject it actually affects into chronic farcy, yet
may by transmission produce chronic farcy in another subject.

If, then, Mr. Wilson gives us evidence, we are perfectly open to accept
it, but he gives us none.   The following amazing passage shows the
kind of material we have to deal with, in trying to give a fair exposition
of his opinions and arguments.   The capital letters are in the original, the
italics are our own.

"Is LUPUS NOT SYPHILIS ?   But lupus is generally regarded as owing its origin to
scrofula—the *obscure* is called forth *to illustrate the obscure.*   We are *as much in
the dark* with regard to *the cause* of scrofula as we are with regard to that of
lupus.   *Lupus is scrofula ;* and what, we might ask, is scrofula ?   Is SCROFULA
SYPHILIS ?" (pp. 165, 166.)

This is enough to take away one's breath.   First, Mr. Wilson flouts at
the notion of establishing any connexion between lupus and scrofula, as a
calling forth of the *obscure* to illustrate the *obscure;* but within less than
two lines the obscurity becomes refulgent light, doubt changes to certainty,
and—"lupus *is* scrofula."   This first step taken, it is only needed to show
that lupus is syphilis, and the identity of scrofula and syphilis is of course
established.   In examining how Mr. Wilson endeavours to connect lupus
with syphilis, we shall also consider what he says respecting the unity of
the syphilitic poison and the syphilitic eruptions, as he comes to his con-
clusion in each case by pretty much the same process of reasoning.

Mr. Wilson infers the identity of lupus and syphilis, because, he says, he
has traced a similarity between certain esthiomenic diseases of the skin,
and certain analogous forms of syphilitic eruptions, linking the two
together by a chain of successive gradations of resemblances in their

external characters. With respect to these eruptions, he says, " The penalty of pursuing their investigation is the belief in their identity." (p. 165.) And this is the whole of his proof. The resemblance between ordinary lepra and psoriasis and the syphilitic forms of these eruptions, being much greater than that between esthiomenic affections of the skin and the forms of syphilis with which Mr. Wilson connects them, our author identifies lepra and psoriasis with syphilis without any difficulty; and he clinches the question by arguing, that as we do not know the cause of these affections, they must owe their origin to syphilis. Such, at least, is the only meaning we can extract from the following passage, unless, indeed, it is only meant to convey Mr. Wilson's naked opinion, unsupported by any argument.

" It has always been a marvel to me what could be the source of this obstinate disease with two names." (Lepra and Psoriasis.) " It is an hereditary disease; it is a life-long disease; it is unconnected with any general morbid affection of the eco-nomy; it is not scrofulous; it does not owe its origin, like some cutaneous diseases, to uric-acid poison; nor does it proceed from any recognised or known poison. What, then, is its source? May it not be a product of that widely-spread, that almost universal, animal poison, syphilis? I think it may, and is." (p. 166.)

Mr. Wilson simply asserts that there is one venereal poison; and such is our own firm belief; but we shall not dwell upon this topic, so slightly is it glanced at in both the works before us. Mr. Wilson also affirms, that there is but one syphilitic eruption. This proposition is stated clearly enough, but Mr. Wilson's reasons for maintaining it are not so evident. He seems partly, if not chiefly, to found his opinion on the circumstance, that he can trace intermediate stages of development between the several forms of nearly allied eruptions, so as to form a continuous chain, con-necting, by various stages of gradation, eruptions, which, separately con-sidered, are totally dissimilar; but he also, in part, seems to rely on the fact, that venereal eruptions all spring from a common cause—from the one syphilitic poison. Now, if it is said that there is but one syphilitic eruption, because in all its forms it springs from a common cause, we have little to object beyond this, that the form of expression is not only awkward and inconvenient, but calculated to mislead those who are governed by words, as too many are. If, indeed, a common cause implies identity, then we might as well say at once, that all the forms of secondary syphilis are the same. It may appear superfluous to dwell upon this point; but Mr. Wil-son puts it forward as one of his capital discoveries, that syphilis occasions " but *one eruption*." Now this we deny, taking the words in their plain acceptation. Syphilitic eruptions differ just as much as other diseases of the skin do, and the difference is a real one, as it depends on the difference of their anatomical seat. The form assumed by any eruption does not depend, except in the secondary character of colour, shape, &c., on the cause of the eruption, but on its anatomical seat; the essential form differ-ing in a determinate way, according as this or that constituent part of the skin is affected. But this difference of anatomical seat is a real difference, and one of vast practical importance, being one of our most important guides in the prognosis and treatment of syphilis. Be the cause what it may, the more superficial the manifestation of syphilis, the more tractable, within certain limits, is the disease. The superficial roseola is a more favourable form of the malady than the deeper-seated pustule or tubercle,

and so on. With tangible and practical differences like these, it is a strange abuse of language to say, that there is but one syphilitic eruption.

Mr. Wilson's theory, however, does not lead him to neglect the distinctive characters of the various syphilitic eruptions; on the contrary, he describes them all with great accuracy and truth. Here, indeed, Mr. Wilson is completely at home. We very gladly bear testimony to the happy and graphic fidelity of his account of the syphilitic eruptions. Of the coloured lithographic illustrations, it is enough to say that they fully sustain Mr. Wilson's reputation as an admirable delineator of diseases of the skin.

But we must return to Mr. Wilson's views respecting hereditary syphilis and the transmission of the secondary forms of the malady.

No one, we believe, any longer denies that constitutional syphilis, manifest or latent, can be transmitted from either or both the parents to the child. It is now a settled point, that secondary syphilis *is transmissible* by the way of hereditary descent. But is secondary syphilis also transmissible during extra-uterine life? Can it be communicated from one person to another, each having an independent existence; from adult to adult; from nurse to child, or from child to nurse? M. Ricord and Mr. Wilson entertain diametrically opposite opinions respecting these latter questions, and both, we think, err in taking an extreme view.

M. Ricord entirely denies that a nurse can be infected by a syphilitic child, or *vice versâ.* He has repeatedly known infants affected with secondary syphilis, suckled by nurses, who never, in his experience, contracted the disease, under such circumstances as satisfied him that it was transmitted to the nurse from the child. He admits, indeed, that cases do occur in which such transmission apparently happens; while there are others in which the nurse is accused of having infected the child. But then, says M. Ricord, the nurse, when she took the child, *may* have had latent constitutional syphilis, which subsequently showed itself—or she may have had a primary sore. Again, the child *may* be born with constitutional syphilis, and the nurse *may* have constitutional syphilis, though neither exhibit, as yet, any symptoms of disease; but the malady *may* after a time break out successively, and then the first in whom it appears is accused of having contaminated the other. Furthermore, the nurse *may* contract syphilis and inoculate her nipples with the hand; or she *may* be infected by a person sucking her breasts; or the child *may* contract chancre from the mother at the time of birth, or be infected by an unsuspected stranger, or dandled by a visitor, &c., " and thus the mother, the *legal* father, and the nurse, *may* be perfectly healthy, and yet the child be affected with constitutional syphilis." (p. 25.) All these possible occurrences may be realized in particular cases; but they cannot affect cases in which there is no reason to suppose or even suspect that any of them exist. But we have so recently examined this point in reviewing Mr. Whitehead's work on ʻ Hereditary Transmission,ʼ that we may refer our readers to what is there said. We shall merely now express our conviction that the cases recorded by Barry, Hey, Colles, Whitehead, and many other writers, supply a perfectly conclusive body of evidence, that a child affected with hereditary syphilis can affect its nurse, and that the peculiar form of secondary syphilis so contracted by the nurse can be by her transmitted

to other persons, whether children or adults, and that they again can similarly communicate secondary syphilis to other individuals in their turn.   And furthermore, we think the history of these cases establishes that this peculiar form of secondary syphilis, transmitted to the child by hereditary descent, is remarkable for its eminently contagious properties, and consequent facility of transmission.

But is ordinary secondary syphilis contagious ?   We think not; or at least we are not aware of any evidence that it is, while every-day experience, at all events, raises the presumption, if it does not prove, that it is not.   Mr. Wilson, however, thinks the contagiousness of secondary poison is beyond all question, nay more, he doubts whether secondary syphilis is not *more* likely to affect the system than primary syphilis, for he gives a case (pp. 50—52), " which" (he says) " suggests the question, whether the secondary poison is not more apt to engender secondary disease than the primary poison ?"

The outline of this suggestive case is as follows: A young man had connexion with a girl who " was known to have had constitutional syphilis, *but was supposed* to be free from any local disease."   The connexion was followed by a discharge from the urethra, and some time after by sores on the penis, and in due time secondary syphilitic symptoms appeared.   And thus, because the girl was *supposed* to have no primary sore, this case is considered to bear out the twofold assumption, that secondary syphilis is contagious, and that it is even more likely to infect the system than primary syphilis is.

Another of Mr. Wilson's cases intended to illustrate the contagiousness of secondary syphilis, is as follows. (Case 22, pp. 71—73.)   A young man had a chancre six months before marriage.   " Four years after marriage he had connexion with a maid-servant, *whom he did not suspect of disease.* No apparent disorder of the genital organs resulted from this occurrence," but in the same month symptoms of secondary syphilis appeared.   The wife continued well, and nothing more is said about the maid.   Now we must give Mr. Wilson's conclusions from this case in his own words; they run thus :

" In the above case, the question suggests itself, whether the attack of constitutional syphilis, as indicated by the eruption, resulted from a secondary poison imbibed from the servant-maid two or three weeks before ? or whether the young man's constitution was already contaminated by the poison admitted into his system more than four years previously, and was now only stimulated to the *expulsive point*, by the reception of a new poison ? *I am satisfied* that the latter is *the true explanation*, and *without seeking for further evidence in support of this opinion*, I will simply remark that the eruption was of the chronic kind, such as only occurs in general, when the poison has been long in the blood and in the system." (p. 73.)

Is not this surprising?   An ordinary person would simply refer the secondary symptoms in this to the sufficient cause of the original recognised primary sore.   But Mr. Wilson assumes, without any imaginable reason, that the maid-servant, though *not suspected* to be diseased, was undoubtedly affected with constitutional syphilis.   Next he assumes that this hypothetical secondary poison was communicated by her to the gentleman ; and, lastly, he assumes that the poison so transmitted stimulated that already in the system to activity.   It would be interesting to know whether the secondary poison in the gentleman returned the compliment,

and similarly roused the supposed latent virus in the maid-servant into activity.

But again: the purest, the most uncontaminated person, the veriest vestal virgin, may exhibit symptoms of secondary syphilis for the first time in adult age, in virtue of hereditary transmission of the poison. This position Mr. Wilson endeavours to establish on the strength of some cases, of two of which we shall give the substance.

In one of these cases (Case 71, pp. 164—166), " a young lady of good family was brought" to Mr. Wilson with symptoms, which, he says, " if she had appeared among my poorer patients, I should not have hesitated to pronounce them syphilis." But " the position in life of this young lady, the care with which she had been educated, and her age (nineteen), were such that I felt the utter impossibility of her having been placed within the reach of the syphilitic poison." Mr. Wilson, however, was not long embarrassed with the difficulty of satisfactorily accounting for the circumstances of the case. " After mature reflection" (he says), " I came to the conclusion that the disease was a germ of the syphilitic poison derived from her parents, and now, for the first time, manifesting itself on her skin; in fact, that it was hereditary syphilis."

This was at all events a charitable conclusion. But really Mr. Wilson's views, if admitted, would have such very momentous social bearings, that we cannot refrain from observing, that it is tolerably certain that his inquiries respecting the history of this case were of the most superficial kind. We do not mean to admit that the case would establish Mr. Wilson's inference from it, had his investigations been as accurate and minute as possible, but merely to show that probably his conclusion was drawn without any inquiry at all. At p. 57, Mr. Wilson tells us of a gentleman who had a sore on his penis, and who said that he had had no connexion of any kind for many years. This gentleman " being," says Mr. Wilson, " a man of influence and rank, I did not venture to ask him how, in that case, he had succeeded in obtaining a syphilitic sore?" Now, Mr. Wilson was scarcely more urgent and inquisitive in the case of a young lady of nineteen, of " good family;" and we do not mean to say, that the reserve we suppose him to have exercised was not very commendable; but be that as it may, we feel constrained to say, that it is worse than trifling to draw conclusions which must be of such immense importance if they are believed, whether true or false, from such materials.

The second case (20, pp. 66, 67) we have to notice on this head, is that of an unmarried lady, aged 25, who consulted Mr. Wilson with a decidedly characteristic syphilitic eruption on the skin; superficial aphthous ulceration of the mouth, and soreness of the vulva with vaginal discharge:

" The history which this young lady gave of herself was, that twelve months previously to her present illness, she had been greatly reduced by a fever, and had been sent to the sea-side to recover her health. In the summer following her return she was one of a country party, and in the frolics of their day's gaiety she had sat for some time on the grass, and found, on her way home, that she was suffering from the effects of a chill. A few days after this adventure she was attacked with sore-throat, which was so severe as to oblige her to keep her bed; and a month later the eruption first broke out. These, in artistic phrase, are broad outlines, which leave much to be filled-in by the imagination, and the medical imagination will not fail to do justice to the picture." (p. 67.)

Well, most medical imaginations would attribute the malady to "frolics," other than those that were confessed to on the grass. However, Mr. Wilson concludes, that in this case "the poison was of secondary origin." (p. 68.)

But to turn from single to married life. According to Mr. Wilson, the secretions of a husband, affected with constitutional syphilis, but altogether exempt from any external manifestation of the disease, can communicate secondary syphilis. A similar opinion is somewhat hesitatingly expressed in Mr. Colles' book, and Mr. Whitehead also is inclined to adopt it. In reviewing Mr. Whitehead's work, we expressed our dissent from this doctrine, and Mr. Wilson offers nothing calculated to impress us with its truth. He merely gives some cases in which constitutional syphilis existed, and in which the affected parties asserted that they never had any primary sore; and on the faith of a few such assertions, Mr. Wilson adopts the doctrine of depraved secretions, capable of communicating syphilis.

But really, if we rightly understand Mr. Wilson, every obscure case of syphilis can be easily explained, for he seems to hold the doctrine that syphilis can be spontaneously engendered in the system. At page 34 he says: "The syphilitic poison originates in the human body; it is probably little more than a modification of the natural secretions." And again, at page 154, he says, in referring to a case detailed at page 152,

"Here, then, we have, passing before our eyes, the phenomena of generation of an irritant poison, by a vital-chemical action taking place in morbid fluids ; and we are enabled thereby to form an idea of the mode in which the syphilitic poison may *be engendered* by connexion between persons of unhealthy constitution, *independently of extraneous origin.*"

But if syphilis can "be engendered independently of extraneous origin," of course it can be developed spontaneously—that is, irrespectively of contagion, or transmission from any previously existing source. But though Mr. Wilson's language implicitly involves this doctrine, we are not certain that he positively adopts it, because of the looseness and want of precision with which he habitually writes. However, the doctrine is by no means new; it was relied upon three centuries since, to account for the appearance of constitutional syphilis, under circumstances somewhat analogous to those of the case of the young lady of "good family," whose case we have already seen. Victori says (we quote from Astruc), "qu'il a vu quelquefois d'honnêtes et de saintes religieuses, exactement cloitrées dans un couvent inaccessible et inviolable, qui etoient tombées malhereusement dans la maladie venerienne, à cause de la corruption de l'air, et de la mauvaise constitution de leurs humeurs, jointes à la foiblesse de leur complexion."

"The weakness of the complexion," we apprehend, played by far the most important part in the generation of the malady. And it really behoves all writers on syphilis to take this same weakness into account, greatly more than Mr. Wilson has done in his work.

We have yet another of Mr. Wilson's discoveries to consider. We have seen that he says, in his preface,

"I trust, also, to have cleared up the mystery which enveloped the induration of the true chancre of Hunter. That induration I consider to be the result of a constitutional action, and, consequently, an evidence of the contamination of the system." (p. viii.)

And again he says,

"The indurated chancre is invariably followed by constitutional syphilis; indeed, as I believe, is itself a manifestation of constitutional action; in other words, the induration is a constitutional affection superadded to the primary disease." (p. 27.)

Now, we can see nothing more in this alleged discovery respecting the signification of indurated chancre, than a reproduction, with a difference of words, of Hunter's *or* of Ricord's well-known opinion on the same subject, we say of Hunter's *or* of Ricord's opinion, because it does not appear what is Mr. Wilson's view respecting the point of difference between these two writers. Hunter did not make induration a necessary precedent condition, indispensable to constitutional infection; M. Ricord does. Mr. Wilson expresses no opinion one way or the other on this head; at least, we have failed to discover such an expression of opinion, though, *more suo*, it may be concealed in some nook where one would least expect to find it. Now, we are apt to reproach French writers with very unceremoniously appropriating the labours and opinions of English physicians and surgeons, according to the most approved doctrines of socialism; we have often had occasion to do so, and we must now purge our conscience by showing that M. Ricord, while fairly attributing to Hunter all that belongs to him, has, in the following passages, said everything that Mr. Wilson says, and a good deal more too.

"Where there is indurated chancre, there must of necessity be constitutional syphilis; with induration, the syphilitic disposition, as Hunter calls it, is acquired. There is a something, a diathesis, a special, peculiar disposition, in virtue of which ulterior conditions are produced. . . . . . Induration is the passage from the primary to the secondary stage." (p. 34.)

Mr. Wilson, at page 15, addresses himself to "the important question. . . . . What is the period which would be considered safe for a man to marry after he has been affected with constitutional syphilis?"

Were our author's doctrines founded in fact, the answer to this question, in our mind, should be—never. If constitutional syphilis is really a disease utterly ineradicable from the system, and if a parent when once affected always remains liable to transmit it to his offspring, in whom it may lurk insidiously, and be again transmitted through their instrumentality to succeeding generations—if all this, we say, be true, a code of sanitary laws, infinitely more strict than the Levitical law relating to leprous patients, ought to be enacted forthwith, and rigorously enforced. For, according to Mr. Wilson's doctrine, once an individual has contracted secondary syphilis, he or she becomes a Pandora's box, a living and unextinguishable focus of virulent contagion, and should therefore be, once and for ever, segregated from communication with woman- or man-kind, according to the sex. There is one thing, indeed, of some weight, to be said against any legislation upon the subject. It is, unfortunately, a little too late to adopt preventive measures. Mr. Wilson, in a note to page 21, speaks of "the frightful consequences, present and future, of letting loose upon society so dangerous a poison as that of syphilis." But the monster has already been let loose, and has had matters pretty much his own way for upwards of three centuries and a half; and it is more than probable (always on the hypothesis that Mr. Wilson's doctrines are sound), that he has established his

right of citizenship in ninety-nine per cent. of the population of Europe. It is vain to struggle against a *fait accompli,* and we must be content.

If, we repeat, Mr. Wilson's doctrine is sound, the occurrence of constitutional syphilis should be an imperative bar, to every conscientious man, against contracting marriage. But Mr. Wilson is never at a loss for a theory, and he has contrived one to evade the inevitable inference from his great principle of the ineradicability of syphilis. The syphilitic poison, he says, has a tendency to accumulate till it reaches "*the point of saturation*" (p. 16); it then eliminates the excess of the poison by the outbreak of syphilitic symptoms, and then, when so relieved, becomes again tranquil. This accumulation and escape of the poison is repeated at intervals, which lengthen after each recurrence, the symptoms at the same time becoming milder, till

"At last the poison is so thoroughly assimilated, that it ceases to accumulate in excessive quantities, and loses its power of exciting a febrile action in the blood of the infected person." (p. 16.)

But though the poison "may be incapable of exciting disease among tissues accustomed to its presence, it still retains the power of contaminating new blood" (p. 16); but as we are making new blood every day, we do not see much comfort in this. "It is *difficult,*" Mr. Wilson says, "to determine how long this degree of virulence continues." (p. 16.) We are glad to hear that; but if so, how comes Mr. Wilson to have the hardihood to tell us, at page 158, that it is an "*admitted law* of the syphilitic poison," that when "once received into the blood, it remains there . . . . *certainly* for the rest of existence?" To return, however, to the account given of "the *law* of action of the syphilitic poison," at pages 15 and 16. Mr. Wilson goes on to say, that the intensity of the poison goes on diminishing, till at length "only accidental conditions call up its power of doing evil." But still, he says, it remains latent for many years, "and *probably* for the rest of life." We must again revert to the circumstances so eminently characteristic of Mr. Wilson, that what, in his estimation, is, at page 16, only *probable,* has grown into *certainty* and an *admitted law* at page 158. We greatly fear that Mr. Wilson is very insufficiently impressed with the solemn importance of the subject he is discussing. Such gross inconsistencies would be blameable, but pardonable, were the matter in hand one of trivial importance; but we shall abstain from designating them by the only terms that would be appropriate, where the discussion involves the happiness, character, health, and life, of individuals and families. Well, then, such being, we are told, the natural "law" of syphilis, Mr. Wilson delivers his opinion, as follows, respecting the expediency of marrying after having contracted secondary syphilis:

"Under these circumstances, our answer to the question as to the time which should intervene between disease and marriage, must necessarily be modified by a variety of conditions; for example, by the nature of the secondary disease, by the known susceptibility of the individual, by his state of health, his occupation, and by the treatment he may have undergone; and something must be known also of the health of the proposed wife. Taking the *most favourable view* of the case, from *two to five years* should be permitted to elapse, such period being passed under the close observation of the medical man." (p. 16.)

We do not know how Mr. Wilson has succeeded in reducing all the variable

conditions mentioned in the foregoing passage into a calculable form, so as to educe the numerical result of "from two to five years." It would, doubtless, contribute to "make things pleasant" for the medical profession, if every case of secondary syphilis could be rendered a little annuity, relapse or no relapse, for a variable period of from two to five years. However, we cannot conscientiously recommend the adoption of the practice.

We have little to say respecting the portion of Mr. Wilson's book which relates to the Treatment of syphilis. We need only notice one or two points.

Mr. Wilson is a decided advocate of what has been termed the abortive treatment of sores that either are, or are presumed to be, chancres. On this head he says :

"If I were asked—Is there no alternative to such a practice as this ? I should say, none. I have heard of such appearances as I am now referring to" (a papule, a pustule, a small sore, or a simple excoriation,) " being made the subject of discussion or argument, as to whether it were or were not a chancre. Such delay is a cruel injury to the patient. If it be not of a syphilitic nature, *no harm can arise* from the use of the caustic ; if it be syphilitic, the proper and only safe treatment has been put in force." (p. 189.)

Mr. Wilson fixes no limit of time within which a sore can be successfully cauterized ; and looking to the opinions expressed in various places in his book, we are at a loss to understand how he comes to the conclusion, that when the sore has been destroyed soon after its appearance (how soon he does not say), the poison is completely eradicated, and all danger of the constitution being affected is entirely obviated. Mr. Wilson, over and over again (pp. 2, 26, 193), says, that the mode of action of the syphilitic poison is similar to that of the vaccine virus. But there is much reason to believe that vaccine matter is rapidly absorbed, and infects the system previously to the development of its local action ; nay more, that this absorption occurs so rapidly, that it is not prevented by cauterizing the inoculated spot a few minutes after the virus has been inserted beneath the cuticle. Again, Mr. Wilson says (p. 3) :

" The imbibition of the poison by the animal tissues is performed insensibly and slowly, and there is no appearance on the inoculated spot to show that any vital process is in action. This is the period which is termed *latent ;* but although *hidden* from view, we know that *the poison is passing into the blood*, and that at the end of a few days (from three to seven) certain local appearances will be seen, which will demonstrate its presence. The local action *so excited* is the primary syphilitic disease, or syphilis."

And again, at p. 190 we read,

" We have no evidence to show *how soon* the poison passes into the blood, whether the poison pass *immediately* into that fluid, whether it be retained for a time by the tissues which primarily received it, and then enter the blood, or whether the original poison simply act as a ferment, and give rise to the production of a quantity of the poison in the part where the local disease manifests itself, and that from this source the supply is derived which contaminates the entire constitution. The question *may be* one of interest in a physiological point of view, *but practically it is of little moment*." (p. 190.)

This is really most deplorable trifling about a matter of very serious importance. First we are told, at p. 3, that "*we know* that the poison *is passing into the blood*," in the interval between impure connexion and

the appearance of chancre; and that the "local appearance" "*demonstrates* the presence" of the poison in the blood.   But at p. 193 we are told that we know nothing at all about the matter; that we are utterly ignorant whether the poison passes *immediately* into the blood; or whether it is locally detained for some time by the tissues it is applied to, before it passes into the system; or whether its action remains purely local for an indefinite length of time.   And by way of climax, Mr. Wilson finally asserts that the question is "*practically of little interest.*"   Need we say, that, practically, the determination of the question is all-important as concerns the so-called abortive treatment.   If the poison is absorbed into the system previously to the appearance of chancre, then cauterization may help to heal the sore, but it cannot possibly eradicate the virus.   But our author might say—that practical experience, at all events, teaches him that a small proportion of cauterized chancres are followed by secondary syphilis.   That we can readily believe.   Mr. Wilson, we have seen, thinks there is no alternative save to cauterize papule, pustule, small sore, or simple excoriation; consequently 90 per cent. or more of the sores he so treats would quietly heal if left to themselves, and not be followed by secondary syphilis.   But then, he says, "if the sore be not of a syphilitic nature, no harm can arise from the use of the caustic."   Indeed! is that quite so certain?   Let us see what Mr. Wilson says but two pages further on.   But we must first premise, that Mr. Wilson considers not only the nitrate of silver, but even nitric acid and the actual cautery "perfectly useless" in destroying a chancre.   The two latter, at all events, we should suppose would be tolerably efficient; but at any rate, we are sincerely glad that Mr. Wilson does not recommend them.   The caustic he prefers to all others, is a pointed stick of potassa fusa, and of this agent he says,

"The potassa fusa is a remedy that requires to be employed with caution; it is, perhaps, the most powerful destroyer of animal tissues known; and although, in the hands of the experienced, it is as safe as the most harmless expedient in surgery, in the hands of the uninitiated it might be productive of the most serious injury." (p. 191.)

The abuse of a means, we admit, is no argument against its use; but before the employment of a method, confessedly so perilous unless in experienced hands, becomes generalized, we are entitled to demand that the fundamental question, on which the efficiency of the "abortive method" in chancre depends, but which our author considers so unimportant, should be decided.

We find nothing more in Mr. Wilson's other observations respecting the treatment of syphilis, which are judicious enough, so far as they go, that demands notice.   It is needless here to express our opinion as to the general merits of the work; that can, perhaps, be sufficiently inferred from what has been said in the preceding pages.—We shall conclude by drawing attention to a curious fact respecting the authors of the two works before us, Mr. Wilson and M. Ricord.   Both adopted the common principle of independently investigating syphilis, with little or no reference to the labours, writings, or opinions of their predecessors.   M. Ricord resolved, that "the question should be studied anew, and he should be solely responsible for the results;" Mr. Wilson, we have already seen, "deter-

mined to investigate the matter as it was presented to himself, and without reference to the opinions and labours of others in the same department." (p. vii.)   Each, then, relying solely on himself, set out from the same point; and the two have ended by arriving at diametrically opposite conclusions upon most of the important points of the natural history of syphilis. This result, of course, is due to intellectual differences between the two writers, which it is not our business to consider; beyond saying that the English writer seems commonly to mistake assertion for proof, and conjecture for demonstration; while the French writer admits nothing that does not strictly square with his own experiments and his own individual observation. Were all the phenomena of syphilis susceptible of being fully tested by the way of inoculation, M. Ricord's method would be faultless; but unfortunately they are not; and it is by assuming that they are, that M. Ricord has deduced some conclusions which we hold to be erroneous, or, at all events, not proved.

Art. III.

1. *Uber den Generation's-wechsel der Cestoden ; nebst einer Revision der Gattung Tetrarhynchus.* Von CARL TH. VON SIEBOLD. (SIEBOLD und KÖLLIKER'S 'Zeitschrift,' 1850.)

On the Alternation of Generations of the Cestoid Worms. By CARL TH. VON SIEBOLD.

2. *Les Vers Cestoïdes ou Acotyles, considerés sous le Rapport de leur Classification, de leur Anatomie, et de leur developpement.* Par P. J. VAN BENEDEN.—*Bruxelles,* 1850.

The Cestoid Worms, considered with regard to their Classification, Anatomy, and Development. By P. J. VAN BENEDEN.

" EA enim rerum naturalium est ratio, ut qui has illasve tantum inspiciat, nexum et ordinem earum nunquam capiat, multisque jaceat in tenebris."* Such are the words with which the great master and founder of scientific Helminthology, Rudolphi, prefaced the work in which he gave to the world the results of seventeen years' zealous and laborious study.

Great, however, as were the difficulties which Rudolphi encountered and overcame, they were, after all, but the minor obstacles in the way of the student of the Entozoa. The soft, gelatinous, semi-opaque tissue of many of these creatures seems equally to defy the scalpel and the microscope; their peculiar habitation renders their investigation difficult, uncertain, and disgusting. But suppose these obstacles surmounted; let the acute eye and the skilful hand, the pen and the pencil, have done their utmost, and there remains still a wide and perplexing field for speculation and observation.

Consider the common tape-worm, for instance, with the general structure and habit of which troublesome parasite we have been long tolerably well acquainted. It is known to be found in the intestine of vertebrate animals, and of these only. The head with its four suckers and its proboscis, armed with strong hooks, is fixed in the mucous membrane of its unhappy

* Entozoorum sive Vermium Intestinalium Historia Naturalis. Auctore C. A. Rudolphi. 1808.

host, whilst its jointed body " lies stretched for many a rood" in the intestinal cavity. Each joint develops male and female organs within itself, and as it reaches its full size, becomes detached and extruded, carrying with it millions of eggs, each ready to produce a young Tænia, whenever it reaches an appropriate dwelling-place.

But the difficulty is, to know what this appropriate dwelling-place is, and how the egg reaches it. The Tænia solium, for instance, is found only in the small intestines of man. How does it reach this locality? Human habits of feeding, of course, completely preclude the idea, that the eggs of the Tænia passed in the fæces of one man should ever take a direct course to the small intestines of another;—but in that case what becomes of them? Are they swallowed accidentally by herbivorous or other animals; and do they circulate as ova through the intestines of these, until some plebeian consumer of tripe, or more refined gourmand gloating over the " trail" of snipe or woodcock, transfer them to his own? Or does the Tænia-egg become developed, but into some different shape from its parent, passing by a sort of transmigration of bodies through unsuspected forms and gradations of life?

When Rudolphi wrote (1808), the wonderful facts which have been since grouped together under the name of the " Alternation of Generations" were totally unknown; and it is not to be wondered at, that under these circumstances, even his philosophic mind took refuge in the theory of " Spontaneous Generation," rather than in either of the suppositions which we have referred to above.

Indeed, as conceived by Rudolphi, the doctrine of spontaneous generation by no means deserved the indiscriminating reprobation with which in later times it has been visited, with more zeal than logic, by writers without pretension to a fraction of his extreme learning, his laborious perseverance, or his truly calm and impartial spirit.

Rudolphi's opinion was not a mere hasty speculation. The proposition, " omne vivum ex ovo," had not *then* the axiomatic force which it *now* possesses; as a scientific induction, it was perhaps not much better based than its opposite. And then it appeared to be a matter of direct observation, that the *cystic* Entozoa at any rate were developed by the modification of pre-existing organized matter. It was observed, that in a liver infested with Cysticercus, some cysts contained no retracted head, but a granular caseous mass; in others, *traces* of a head were to be seen; in others, it was fully developed. Now, putting these facts together, with the known non-existence of ova in Cysticercus, Rudolphi very reasonably concluded that these cysts were portions of the hepatic substance, becoming gradually developed into Cysticerci. We know now that he was right as to the facts, but wrong as to the interpretation; what he considered to be a process of development, was, in fact, a process of degeneration; but how many years has it taken to arrive at this conclusion!

For the rest, no one could have combated more strongly than did Rudolphi, the vulgar error, that the theory of spontaneous generation has anything in common with the notion of a fortuitous concourse of atoms. It is but justice to quote his words upon this point. " Hæc enim generatio æque ac reliqua, sive sexualis a naturæ legibus æternis et nunquam mutandis derivari debet, ut materie loco et quæ præterea requiruntur convenientibus convenientia—diversis autem diversa oriuntur."

With the progress of our knowledge of the lower animals, however, the occurrence of spontaneous generation has daily become, upon empirical grounds, more and more improbable; and as regards the Cestoid and Cystic worms, the two elaborate memoirs which head this article have placed that hypothesis altogether out of court, by rendering it needless.

The cestoid and cystic worms are creatures, of which the Tænia, the Bothriocephalus, and the Cysticercus, are examples familiar to our readers. Less known varieties of the former division are the Anthocephala, or "flower-headed" worms, and the Tetrarhynchi, which occur only in the lower animals.

It is upon these, that the observations of MM. Siebold and Van Beneden were chiefly made; but there can be little doubt, that the Cestoid entozoa of Man undergo similar changes.

We propose, first, to sketch slightly the anatomy of the Cestoid worms, and afterwards to inquire into the changes which they undergo, and into their relations with the Cystica.

A full-grown Cestoid worm consists of a portion which is called the head, and of an elongated jointed body, usually more or less flattened, and band-like.

This head in Tænia is rounded, and carries four suckers; it possesses, further, at its anterior extremity, a retractile proboscis, which is beset with numerous fine hooks.

In Bothriocephalus there are neither hooks nor suckers, but the head is slightly lobed on each side. In Triænophorus, the head possesses three-pronged hooks, differently arranged from those of Tænia. In the Tetraphyllidæ (Tetraphyllien) of Van Beneden, which include the equivalent of the old Anthocephala, the head is provided with four large, contractile, and very irritable, expanded lobes, giving it some resemblance to a flower. Hooks or suckers may in addition be developed upon these lobes.

But the most curious and formidable armature is in the genus Tetrarhynchus, which forms one section of the Tetraphyllidæ. Instead of the single uncinated proboscis of the Tænia, these have four tubular proboscides; each of which can be completely retracted within the head (like the tentacle of a snail), by a process of involution. In the retracted state, the inner surface is covered with strong hooks, which, as the proboscis is exserted by means of its peculiar muscular bulb, become external, and form most efficient means of attachment.

There is no digestive canal in the Cestoidea, consequently no mouth, nor any anterior apertures, such as have been described. The canals into which these supposed apertures were said to open, have no relation whatsoever with the alimentary system. They belong, in fact, to what has been called by Von Siebold the water-vascular system—a system of canals, whose nature and import he was the first to point out, and which is characteristic of, and peculiar to, that subdivision of the animal kingdom to which the Cestoidea belong. They are delicate vessels, with proper parietes, which communicate with the exterior by one or more apertures, and contain sometimes water only, sometimes a peculiar secretion. Great varieties exist in the arrangement of these vessels. The external aperture may be single, and communicate with a contractile vesicle, from which the vessels are secondarily given off (Distomata, Roti-

fera), or there may be no contractile vesicle, and the system may have many external apertures (Turbellaria, Annelida); they may be simple in their course, or very much ramified; but they are always characterized by possessing at intervals along their interior, peculiar vibratile cilia, or fringes, which would seem to exist for the purpose of keeping the contained fluid in motion.

In a Cestoid worm which has not yet cast off any of its segments, a contractile sac exists in the last, and there opens externally; from it four branches are given off, two on each side, and these communicate by transverse branches in each segment. Having reached the head, the vessels form loops, and anastomose in the lobed appendages, if such exist, and become united to one another by a circular canal. It is these canals which have been hitherto injected and described (though only one on each side has been usually seen), as the alimentary system.

Eschricht* and Leuckart† described, and Blanchard‡ appears to have injected, a superficial set of canals different from these, and which Blanchard regards as a true vascular system. He figures these vessels as a delicate rectangular superficial meshwork.

Von Siebold criticises Blanchard's statements and figures rather severely, and denies that any other vessels than those belonging to the water-vascular system exist in the Cestoidea. Some very valuable observations by Dr. Guido Wagener, however (Müller's *Archiv.* 1851), appear to us to reconcile these conflicting statements. In an encysted Tetrarhynchus, from a Trigla, after describing the principal trunks of the water vessels, Dr. Wagener proceeds:

"From these great vessels, which are easily to be found, we must carefully distinguish a very fine vascular network, which presents visible walls only at times. One's attention is called to its existence by the flickering cilia which are placed at the apertures of the vessels. . . . . As to the relation of these capillary vessels (in which I could only at times detect walls with any certainty) to the other parts of the body, it is remarkable that in Tetrarhynchus, as well as in Triænophorus and the Cysticerci, &c., it may be followed out as far as immediately under the structureless investment." (p. 217.)

Again, in Tetrarhynchus Megabothrius, he says that the " capillary system formed a very regular network, with quadrate or oblong meshes, in which solitary cilia were seen actively vibrating."

Von Siebold himself describes these " capillaries" in the head of some cestoid worms, so far confirming the account of Wagener, whose statements are for the rest so careful and detailed, as to afford, in our opinion, a complete explanation of Blanchard's injections, and, at the same time, a justification of Siebold's assertion, that none but a water-vascular system exists in the Cestoidea.

As the result of all the investigations which have been hitherto made, then, it appears that the Cestoid worms possess a peculiar system of vessels, consisting of four principal lateral trunks, which commence by a common posterior outlet, run through the whole body, communicating with one another by transverse anastomoses, and by a circular vessel in the head; and finally terminate* in a superficial network of fine vessels.   Further-

* Anatomische physiologische Untersuchungen ueber die Bothriocephalus. Nova Acta, t. xix. 1840.
† Wiegmann's Archiv. 1848.   ‡ Récherches sur l'organisation des Vers.

more, these vessels are not bloodvessels, but contain either pure water, or water mixed with an excretion, which is kept in motion partly by the contraction of the main trunks, partly by the action of the vibratile cilia seated at intervals along the canals: such a "water-vascular system" being by no means peculiar to these creatures, but having been long known to exist in the Annelids, Distomata, Turbellaria, and Rotifera,—and to these we might, perhaps, add the Echinodermata, whose water vessels, however peculiar the purposes to which they are applied, seem to be essentially homologous organs.

Muscular fibres, more or less clearly marked, exist in the suckers and lobes of the Cestoidea and Cystica. Müller* and Blanchard† have described a nervous system, consisting of a single ganglion placed in the middle of the head, and sending off twigs to the suckers, and to the sides of the body, in Cysticercus, Tetrarhynchus, Tænia, and Ligula; and rudimentary eye-spots have been observed in various species of Scolex. A higher condition of the organs of relation is hardly to be expected in animals whose life is sedentary, which have no enemies, and whose wants are all provided for without their exertion.

Such is the whole organization of the young Tænia, or of the Cysticercus; but with the development of the generative organs a wonderful activity of life replaces this sluggishness.

Political economists might draw a moral from these worms: they are a kind of zoological paupers, receiving in-door relief from their birth upwards; and they exhibit a corresponding faculty of multiplication and increase.

The Tape-worm, once fairly anchored in the mucous membrane of the intestines of an animal by its armed head, grows rapidly at the other extremity; and constrictions become visible, gradually marking off joints, which sometimes assume very definite and peculiar forms. In each joint the outlines of a double set of generative organs, male and female, gradually appear, and assume a high degree of complication. The male organ consists of a long, tubular, convoluted testis, and is connected with a sac, which opens upon either the posterior or upon one of the lateral edges of the segment. This sac contains a tubular filament, the cirrus or penis, which is capable of inversion or eversion, in the same manner as the tentacles of the Tetrarhynchus, and which, like them, is frequently armed with formidable spines.

Close to the male aperture, but distinct from it, is the opening of the female organs. These may be best understood by comparing them to a Y. Suppose that the upper narrow limb of the Y communicates with the genital aperture, and represents the vagina: then the lower limb will represent the dilated posterior extremity of the vagina, or the spermatheca, in which, after fecundation, the spermatozoa are retained.

Two sets of organs open at the point of junction of the three limbs of the Y. In one set, the germinal vesicles of the ova are formed; in the other, the yelk cells are developed. As the germinal vesicles pass out of their special nidus, they become surrounded by the yelk cells, and fecundation at the same time takes place by means of the contents of the spermatheca. The ova thus formed and fertilized accumulate in the uterus,

<div style="font-size:small">* Müller's Archiv., 1836.   † Loc. cit.</div>

whose position is represented by the broad upper limb of the Y.   As their number increases, the uterus becomes sacculated and enormously enlarged, so that it occupies the whole segment, and obscures all the other parts of the generative apparatus.

Such is the essence of Van Beneden's account of the structure of the generative organs in the Cestoidea, and it is, in all important particulars, confirmed by Von Siebold.   As to the final mode of exit of the ova, there is a disagreement; the latter observer asserting that he has seen the ova pass out by a special aperture (in Tetrarhynchus corollatus); while Van Beneden positively asserts that there is no proper aperture to the uterus, and that the ova are only cast forth by the bursting of the segment.

Van Beneden also stands alone in affirming that he has seen one of the segments impregnate itself, its cirrus being introduced into its vagina.   It is certainly difficult to comprehend how impregnation can take place in any other manner.

It is a very singular fact in the history of the Cestoid worms, and one which prepares us for the still more remarkable phenomena to be detailed presently, that they do not attain their adult reproductive state under all circumstances.   It would seem, indeed (although more evidence is required upon the point), that they *never* acquire the procreative state in any but a vertebrate animal, nor in any organ of this, save its intestinal canal. Certain it is, that particular species acquire reproductive organs only in animals of a certain elevation in the scale.   Thus, the Bothriocephalus nodosus of the stickleback, and the Ligulæ which occur in the abdominal cavity of many fishes of the carp and salmon kind, are never found to possess generative organs.   But the aquatic birds, which prey upon these fishes, contain Entozoa of the same species, in which the sexual apparatus is fully developed.   The worm appears to have required the stimulus of a residence in a warm-blooded animal, to arrive at perfection.

In other cases, however, such an explanation is by no means applicable. The Cestoid worms, for instance, inhabiting fish which enjoy an immunity from the attacks of water-birds, remain imperfect in the Osseous fishes, and, according to Van Beneden, attain their sexual state only in the Plagiostome fishes; and this cannot be explained by a mere difference of locality, as they are found in the intestinal canal in each case.

Nature, in truth, seems to have taken predatory propensities into account in her scheme of ways and means, and, as her fashion is, from destruction itself draws sources of new life.   The shark, doubtless, feels a certain satisfaction when he has dined upon salmon.   But he swims away like Dido,

"Inscia quantus Deus *in*sedit miseræ;"

and it is pleasing to reflect, that he has less reason to congratulate himself upon his piracy than he thinks.   The osseous victim has left his avenger.

These changes of state, the consequence of changes of residence,—this arrest of development, as it were, produced by external circumstances,—fitly introduce us to the consideration of other and still more remarkable phenomena, presented by the Cestoid worms.

All who have ever studied the Entozoa, have been struck by the remarkable resemblances between the Cestoid and the Cystic worms; between the

Tænia and the Bothriocephalus, on the one hand, and the Cysticercus, the Cænurus, and the Echinococcus, on the other.

If, indeed, we conceive a Cestoid worm, with a very short body, dilated at its extremity into a vesicle, we have a Cystic worm, like Cysticercus; if the vesicle becomes very large proportionately, and there be more than one head externally, we have Cænurus. If there be many heads developed internally, and becoming free, we have Echinococcus. And the heads of these Cystic worms vary in the same manner as those of the Cestoid worms, some having an uncinated proboscis, like Tænia; some four proboscides, like Tetrarhynchus, and so forth.

Facts like these, coupled with the circumstance, that no generative organs have ever been discovered in the Cystica, and that in the higher animals, these worms are always found either in parenchymatous organs, or under serous membranes, and never in the intestine, which last is the equally invariable habitation of the true Cestoidea, have constantly led to speculative approximations of the two groups.

It was observed, that the vesicle of the Cystic worm is produced by a mere dropsical distension of the body of a Cestoid worm in many cases. The Cysticercus fasciolaris, for instance, which inhabits the liver of the rat and the mouse, was clearly seen, from the peculiarities of its structure, to be the Tænia crassicollis of the cat, with an enlarged and dropsical extremity. In its youngest state, this Tænia was known to possess no caudal vesicle, so that the Cysticercus could only be a Tænia, which had in a manner lost its way, and become degenerated; and this theory was strengthened by the circumstance, that Cysticerci are found in further states of degradation, containing cheesy and calcareous deposits, and eventually becoming destroyed.

Von Siebold, who has strongly advocated this conception, considers that Cænurus and Echinococcus are simply Cestoidea in a still more distended and modified condition.

There can be very little doubt of the great probability of this view, as regards Cysticercus, Cænurus, and Echinococcus; but we think that Siebold has gone further than the facts warrant, and has involved himself in a needless controversy, when he asserts (p. 223), that with the exception of Cysticercus fasciolaris, and perhaps C. crispus, no other cystic worm can become a sexual cestoid worm.

Van Beneden seems to us to run to the opposite extreme. He regards the cystic condition as a part of the normal development of every Cestoid worm (at least of that section to which he gives the name of Tetraphyllien), or perhaps we may rather say, that his statements are wanting in clearness on this head; his facts pointing in one direction, his nomenclature in another.

The embryo " Tetraphyllien," Tetrarhynchus, for instance, is, he says, an ovoid sac, provided anteriorly with four lobes. It has, as yet, no trace of the proboscides, but it exhibits various movements and changes of form, the most remarkable of which, is that produced by a sort of self-invagination, the head becoming retracted within the dilatable posterior extremity, which invests it, and incloses it, just as a double nightcap incloses a human head, if it be pulled down over the neck.

At first this invagination is not permanent, but if the worm have worked

its way into any of the parenchymatous organs or subserous membranes, the invagination becomes permanent, and, in addition, the animal secretes and becomes invested by a transparent cyst.

In this Cysticercus-like condition, the Tetrarhynchus, shut up within its living prison, undergoes certain changes. Its proboscides attain their full development, and after a while, as Van Beneden and Wagener both testify, the hinder end of the included head becomes naturally separated from the wall of the vesicular portion, so that the former lies free and independent within its own body. In this condition, therefore, the parasite consists of three portions, inclosed one within the other—namely, the cyst externally, the vesicular end of the body within this, and lying in this, the armed head.

The cyst is of course passive, but the vesicle is often very contractile. Leblond, who first examined the Tetrarhynchus in this state, thought that the head was a distinct animal from the cyst, and gave the latter the name of Amphistoma rhopaloïdes. Subsequently, Miescher, who noticed the occurrence of a Filaria in the fishes infested by Leblond's parasites, improved upon this idea, by supposing that the cyst, which is elongated, was the result of the metamorphosis of a Filaria, and brought it forward at the meeting of naturalists at Bâle, in 1840, as a very wonderful occurrence, which, indeed, had his interpretation been correct, it would have been.

A Tetrarhynchus parasitic upon a Distoma, itself a parasite upon a Filaria parasitic upon a fish—would have almost realized Peter Pindar's famous distich :

> " These fleas have other fleas to bite 'em,
> And these fleas, fleas—*ad infinitum*."*

These observations, in fact, were long a source of serious perplexity to naturalists, and could hardly have been propounded, had MM. Leblond and Miescher borne in mind the caution which lies in our opening quotation.

Von Siebold, who admits and describes the invagination of Tetrarhynchi, appears to have examined specimens in which the separation between the head of the worm, and the sac which contains it, had not yet taken place; and he will by no means allow that the separation is anything but an artificial one. He says,

" This embryonic Tetrarhynchus is nothing else than the head-end of the cestoid worm torn out of the vesicularly-distended body. The torn end rounds itself off by means of the sarcode predominating in the young Cestoidea, so that the mutilated part is readily overlooked. . . . . When Miescher expressly says, that there is no organic connexion between the embryonic Tetrarhynchus and the Trematode-like worm which invests it, he has only overlooked the organic connexion which really exists, a circumstance readily explicable from the structural peculiarities of these delicate Helminthoida." (p. 245.)

We have thought it right to give Von Siebold's opinion, though, as we have previously stated, the evidence seem to us to be strongly in favour of the opposite conclusion. Wagener, indeed, though he does not mention Von Siebold's name, appears to have examined into this matter with especial reference to his statement; for he expressly says, that in twenty

* By the way, was the notion of which this couplet is a parody, originated by Nicolas Audry ? He says, " Si on la regarde (la mouche) avec le microscope on y decouvre sovent divers animaux qui sa succent et ces animaux sont sans doute succez par d'autres &c., selon qu'il y a de matière corrompue en chacun d'eux pour nourrir quelques autre espece d'animal dont la semence s'y puisse arrêter."—*Génération des Vers,* p. 23.   1700.

examples of an encysted Tetrarhynchus from Uranoscopus scabes, when the vesicle was opened the animal came out, and never showed any symptoms of injury; indeed, its posterior extremity was covered with short hairs of a very definite shape. Again, in Tetrarhynchus tetra-bothrius, " the knob at the end of the body was covered by a thin structureless membrane." Van Beneden is equally decided as to the normal freedom of the Tetrarhynchus within its cyst. He supposes that the terminal hairs, which he has observed in some cases, result from the tearing away of the posterior end of the Tetrarhynchus from its cyst.

Van Beneden does not assert that all Tetrarhynchi become invaginated and detached, in the interior of their own caudal vesicles; but that as those Tæniæ which pass out of the intestinal canal into some other viscus become Cysticerci, so those Tetrarhynchi which take a similar course become encysted.

" The most of these worms continue their development directly in the intestine of the same animal, or more frequently in the intestine of some other fish, which has swallowed the animal in which it is lodged. The Tetrarhynchidæ make an exception to this rule; they ordinarily perforate the parietes of the intestine, and pass under the folds of the peritoneum. A sac is formed at the expense of this membrane, and a cyst appears within this, which corresponds with the cysts formed by Cercariæ which are becoming transformed into Distomata.

" This cyst is formed in the same manner as the tube of many Annelids, &c., by the excretion of a mucosity, which invests the animal on all sides; the creature, then, by a backward and forward motion, converts the cyst into a proportionally large cell. New layers are deposited from within outwards, gradually thickening the cyst, and diminishing the interior space, until at last the worm is quite closely embraced by its sheath." (p. 82.)

Van Beneden adds, that he has actually seen the invagination and excretion of the cyst take place under the microscope.

To the whole phase of the life of a Cestoid worm, included between its embryonic and its encysted condition, Van Beneden gives the name of " Scolex" stage. We cannot but think that this is altogether a most unfortunate denomination. In the first place, Scolex is a term that has already been long employed in a generic sense, and its use for a mere phase of a species, without any warning, can only tend to produce confusion. Then, under this name of " Scolex," two very different states of the animal are included—its normal embryonic form, and its permanently encysted condition—which must be considered to be more or less a deviation from the regular course. It is only by accident, as it were, that the encysted Tetrarhynchus becomes capable of resuming the proper course of its development; if the fish, in which it is parasitic, be not devoured by some other fish, so as to bring the Tetrarhynchus into its normal habitat again, there is no evidence to show that it will ever be further developed; indeed, the probabilities are, that under these circumstances the encysted Tetrarhynchus, like the Cysticercus, will become atrophied and degenerated.

Surely, then, it cannot be proper to include a monstrous form, produced by the accidents of its habitation, as a regular stage in the development of the cestoid worms? It seems much more correct, with Von Siebold, to consider the encysted " Scolex" stage as an essentially diseased and dropsical form.

In whatever manner the young Cestoid reaches its proper locality—the intestine—the succeeding changes are the same. The tail-end rapidly lengthens and enlarges; it becomes divided, in most cases, into well-marked articulations, in each of which a complete set of generative organs is formed. The development of the joints takes place, so that the hindermost is the oldest—the others being developed successively *in front* of one another; differing in this respect from the development of new segments in Annelids and Myriapods, in which the new formation of segments takes place between the last segment and the last but one. To this state, which is that under which the Cestoid worms are commonly known, Van Beneden has given the name of Strobila, from a just perception of the close analogy between it and the Strobila condition of the Medusæ observed by Sars.

Finally, the segments become detached and free, the posterior segment usually dropping off very early; and for these detached segments, to which the old writers gave the name of " cucurbitary worms," from their resemblance to gourd seeds, Van Beneden retains the name of Proglottis, first applied to them by Dujardin. The Proglottides become rounded off at each end, and take on a certain rough resemblance to a Distoma; never acquiring, however, any trace of the complicated organization of the latter, though they exhibit independent contractions. They are, as Van Beneden himself expresses it, but " a sort of sheath to the sexual apparatus."

How long they are capable of maintaining this form is wholly unknown; but it seems probable that, discharged among the fæces of the animal in which they were formed, they become the food of some of Nature's many scavengers,—of some Mollusk, or Osseous Fish, for instance, in whose intestine the eggs are set free, and the embryos developed into their " Scolex" form. In this state, it would seem that they remain until the Mollusk, or Osseous Fish, is devoured by one of the carnivorous Plagiostomes, or some other higher animal, when the Cestoid worm starts from its wholly buried and half-dead state, into new life.

By the discovery of these extraordinary facts, the problem of parasitism among carnivorous animals seems to be pretty clearly explained.

It is not so clear how the younger phases of existence are passed by the parasites of herbivorous animals. The young Cestoid worm leaves the egg in a state which differs only in *degree* from the adult form, and would seem to require the same conditions of existence. There is no evidence that it even leads an aquatic life, which would render its ingestion comprehensible, nor does it seem in any way fitted for such a mode of existence.

Still more difficult is it to understand the mode in which these parasites are introduced into the human body. Tenacious as is their hold of life, it is difficult to imagine that, under any form, they can bear cooking; and yet, unless the Tænia is capable of living under some shape in water, it is incomprehensible by what mode it reaches the intestine of man.

After all, then, the question which we put forward at starting—viz., how do the Cestoid parasites reach their appropriate subject?—cannot yet be regarded as quite clearly settled; but these researches (and here we refer particularly to those of Professor Van Beneden) have thrown very great light upon it, and are of the highest value as indications of the road which must be pursued in future.

The labour of such investigations must have been very great. A cestoid worm is not to be found in all its stages in a single animal. Discovered in a cartilaginous fish in an adult state, for instance, the prey of that fish had to be found out, and in these animals the search was carried on for the earlier forms; perhaps, again, the worm was to be found, in its very earliest state, only in the food of these;—so that the youngest form of the parasite of a Ray might be ultimately traced to a Crustacean, to a Mollusk, or to an Annelid.

" Those alone," says Van Beneden, very truly, " who have worked far from their study, know what labour and trouble these researches cost; how wearied one gets in soul and body, when one has to look to everything oneself: to seek out fish, then to hunt for their parasites; when these are found, to examine, to dissect, to draw, to preserve them; to make one's notes, and to do everything in a short time,—for, generally, they die very soon, and then undergo immediate altera- tion." (p. 10.)

We are heartily sorry to find that such labours have seriously impaired the Professor's health, and that we must not hope for a continuation of these researches from the same learned and conscientious observer. We trust, however, that his prognosis is not so trustworthy as his observations, and that we may yet have a renewal of the long series of contributions with which he has enriched the Comparative Anatomy of the Invertebrata.

We have endeavoured to avoid purely zoological discussion in the course of the present article, but there is one subject on which we cannot be silent, as it forms a very prominent part of Van Beneden's speculations, and one on which we differ very widely from him:—" Are the Cestoidea simple or compound animals; are they mono- or poly-zooic; and is the detached segment, or ' Proglottis,' a *part* of the animal, or the animal itself in its adult state?" (p. 94.)

To this question a special chapter is devoted; and the conclusion arrived at is, that the Cestoidea are Polyzooic, and that the Proglottides are separate individuals; the " Strobila" being a compound animal.

The main argument relied upon is the unquestionable homology of the Proglottides with the Medusæ produced by the fission or gemmation of Polypes, and with other free forms developed by gemmation.

The general bearings of the " Alternation theory," and the interpretation of the phenomena classed under that term, have already been discussed in this Review (vol. i., pp. 204, 205). Insisting, as strongly as Professor Van Beneden can do, upon the homology of the Proglottis with a Medusa-bud, we should, consistently with that interpretation, come to precisely the opposite results with regard to the nature of the Cestoid aggregation.

We regard it as a single individual animal, the generative organs of which, as in Polypes, &c., take on an independent existence.

It is, indeed, difficult to imagine, how the doctrine of animal indi- viduality, which we advocate, could have a better illustration than in the case of the Cestoidea. Does the learned Professor of Louvain question the monozoic nature of Ligula? And yet Ligula differs from the other ces- toids in absolutely no point of importance, except that it does not break up into segments.

In the young Cestoid, the nervous system has its centre in the head, and sends branches to the posterior extremity. The water-vascular system, on the

other hand, has its centre in the posterior extremity, and sends branches into the head.   There can be no doubt that in this state, at any rate, the whole of these two sets of organs are organs of the same individual.   When the body lengthens, and becomes annulated, new nervous or water-vascular centres are not developed, but each system remains as before—one and indivisible; demonstrating clearly that the totality still forms only one organic whole.

Is there any alteration in the facts, when the articulations become separated?   Truly not; indeed, each detached segment carries, in the portion of a water-vascular and nervous system which it contains, a fragment utterly meaningless and useless for the separate segment; a clear indication that it is a part, and not an organic whole.   The water-vascular system here plays the part of the coloured thread, which is sometimes woven into a particular rope, in order that any portion of it cut off and stolen, may be identified as a part of that individual rope.

In fact, apart from all consideration of philosophical zoology, the following passages from his own work alone, might make us hesitate to adopt Van Beneden's Polyzoic theory :—

"There is a complete community among all the segments.   The skin and the canals extend from one to the other, and the movements are equally combined. The Strobila, in some species especially, dilates strongly in one region and contracts in another ; and these motions alternating through the whole length of the body, give it exactly the appearance of certain Annelids when making violent progressive efforts." (p. 89.)

"The animal (Proglottis) consists in strictness of a sheath, which surrounds a male organ and a female organ.   It is, in truth, only the case of its sexual apparatus." (p. 93.)

Professor Van Beneden seems to think that he has the authority of antiquity upon his side of the argument, and considers that he has but returned to the views originally entertained by Nicolas Andry, Vallisnieri, and Ruysch.*   His citations are, unfortunately, not so accurate as his observations.   The fact is, that neither Andry nor Vallisnieri entertain any such notion as that imputed to them.

Nicolas Andry, a celebrated French physician, published in the year 1700 his treatise, 'De la Génération des vers dans le corps de l'homme.' It is a very cleverly written essay, and must be considered as the first attempt to give a scientific direction to helminthology.

Andry maintains that there are two kinds of flat worms found in the intestine of man, which must be carefully distinguished.   To one of them he restricts the name Tænia: it has a very finely-pointed head, and exhibits no movement : to the other he limits the name Solium; this has a pear-shaped head, and moves actively.   According to some writers, he says, there is a third flat worm, called "Cucurbitaires," which, when joined together, form a long chain.   But—

"In the chain of 'Cucurbitaires,' we see neither head nor neck, by which we may judge of what it is, and the portions of which this chain is composed, appear

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* "Je n'ai pu me procurer le mémoire de Vallisnieri que je ne connaissai que par des extraits ; aussi je me bornerai à dire que ce naturaliste considére un Cestoïde comme un animal composé, opinion à laquelle on doit nécessairement revenir après un étude approfondie.

"Nicolas Andry et Ruysch sont du même avis que Vallisnieri sur la nature de ces vers."—p. 14.

"Toutefois, parmi les anciens il est curieux de voir Vallisnieri, Nicolas Andry, et le célèbre Hollandais Ruysch, regarder les Cestoïdes comme des vers composés."—p. 94.

to be connected together only like the excrements of certain animals, especially dogs, whose adherence closely resembles that of the pretended worms. *These we must regard only as the excrements, or as the ova of the flat worms, instead of confounding them together with the latter, as some moderns do, who ought to have been a little more accurate.*" (p. 93.)

And at page 91, he mentions, " Fernel, Perdulcis, and some others, who wrongly imagined that the flat and long worm, which the ancients described, was nothing but a chain of cucurbitary worms joined together." The Solium "*is a single worm, and not many worms joined together.*" (p. 311.)

It would be easy to multiply quotations to the same effect; but these are sufficient to show that Andry, so far from asserting the composite nature of the Cestoid worms, stoutly and expressly maintains the opposite doctrine.

As little is Vallisnieri* an advocate of the Polyzoic theory. In the midst of a somewhat abusive polemic against " Il Signor Andry," he never opposes him upon this point. On the contrary, he brings forward every argument within his reach in support of the monozoic nature of the Tænia, in order to force into greater prominence his peculiar speculation, that the cucurbitary worm is something of an altogether distinct nature from the flat worm. We have seen that Andry describes Tænia and Solium to be two kinds of flat worm, the cucurbitary worm (Proglottides of Van Beneden) being the eggs of Tænia adherent to one another. Vallisnieri, on the other hand, having compared the cucurbitary worms with the joints of a worm which he had identified as the " Solium" of Andry, was struck by their resemblance, and justly maintained that the " Solium" was an aggregation of the so-called " cucurbitary worms." Upon that very ground, however, he expressly denies its affinity with Tænia; asserting, that what Andry says about the Solium having a head is all a mistake. There are two kinds, he says, of true worms, and two kinds of false worms. The true worms are the Tænia and the " Verme lato," or " cucurbitary worm;" the false worms are, the " Fascia," and the " Solium,"—these being nothing but cucurbitary worms joined together voluntarily, like " bats in autumn."

The resemblance of the " cucurbitary worm " to a Distoma, was evidently appreciated by Vallisnieri, for he compares it to the " worms found in the biliferous ducts of cattle." The Tænia, on the other hand, is, he says, a worm " like a ribbon, with a head, neck, long belly and tail, provided with an alimentary canal along its whole body, with other organs and vessels necessary for a single animal (ad un solo)." p. 96.

As to the points in dispute between them, Andry and Vallisnieri were both more or less right, and both more or less in error; but whether rightly or wrongly, it is quite clear that they were agreed as to the monozoic nature of the true Cestoid worms. The Tænia was considered by both to be a true flat worm. The " Solium" of Andry was asserted to be a flat worm by that writer; but it was denied to be one by Vallisnieri. The latter, therefore, in affirming the composite nature of the Solium, was was by no means countenancing the polyzoic theory of Professor Van Beneden.

With regard to Ruysch, we have been as unsuccessful in finding any

* Considerazioni ed esperienze intorno alla generazioni di vermi del corpo umano. Padova, 1710.

passage in support of that theory, as with Andry. In the 'Theatrum Animalium,' *De Insectis*, lib. iii. cap. 3, Ruysch says, that the Tænia is composed of " many *parts*, each of which is like a gourd-seed." We think, then, that Professor Van Beneden cannot claim the authority of antiquity for his view; and we must confess, that we consider he has still less right to look for the suffrages of his contemporaries.

It sounds ungrateful, however, to dismiss a work from which we have derived so much instruction, with an expression of fault-finding; we would rather leave upon the mind of the reader the impression of our strong sense of its merits, and of its value as the most important contribution to a very difficult branch of zoology, that has been made for many years.

Art. IV.

Southern Medical Reports: consisting of General and Special Reports on the Medical Topography, Meteorology, and Prevalent Diseases of the following States,—Louisiana, Alabama, Mississippi, North Carolina, South Carolina, Georgia, Florida, Arkansas, Tennessee, Texas, California. Edited by E. D. Fenner, M.D., of New Orleans. Vol. II. 1850. 8vo, pp. 498.

The first volume of this work had already impressed us very favourably with the industry and activity of the medical profession in the United States; the second volume, now before us, shows no falling off. We can never fail to feel a warm interest in the reputation and success of our Transatlantic brethren, in whatever department of human knowledge they may be engaged. It is, however, in literature and science that we are most closely related. The vanity imputed (not ill-naturedly) to the citizens of the United States, is no more manifested in their general and professional literature, than in the professional literature of the United Kingdom—if so much. Of the English republic of letters it may be truly said, that it is " one and indivisible;" and while the distant provinces look to the parent-source of English literature with respect, nay, we may even venture to add, affectionate reverence, the fundamental principle of its code is " liberty, equality, fraternity." With very distinct feelings of fraternity we read Dr. Logan's account of his doings in the distant California, and the Report from remote Arkansas by Mr. Coolidge, of the United States' army. Dr. J. C. Simonds and Dr. Barton excited our sympathy for their efforts to awaken the inhabitants of New Orleans from their hallucination, that that large city, with an annual mortality of 6·22 per cent., or 1 in 16, and in which " *only*" 3000 persons died of yellow fever alone in the year 1847, is by no means an unhealthy place. The reports of the proceedings of various medical societies or associations in the United States, showed very conclusively that they and we are one body, and elicited our best wishes for their prosperity.

We do not propose to make a critical analysis of the numerous communications contained in this volume, but shall simply select a few *excerpta*, taking those which are most likely to prove interesting to the British reader, but cordially recommending the volume to those who wish to

enlarge their views as to medical meteorology, hygiène, general pathology, and, in particular, the pathology of fevers, and general therapeutics.

Relation of Temperature to Yellow Fever.—Dr. Barton observes, in his Essay on the Meteorology, Vital Statistics, &c., of Louisiana,

" It has already been proved, in relation to yellow fever in Philadelphia, in a series of years from 1793 to 1817, embracing many epidemics, that it occurred in no year when the average thermometer, at three o'clock, was under 79° during the summer, and that the extent and malignancy of the disease was proportioned to the extent in which it exceeded that height; and that the average temperature of June and July at that period [hour ?] governs the season in relation to health, insomuch that if, by the 1st August in any year, the average shall be below that degree, we should feel full confidence that during that season yellow fever will not occur. In relation to this country [Louisiana], although this precise degree does not apply in an examination I have made of some nineteen years, yet the principle, that the salubrity of the city greatly depends upon the elevation of the temperature, is fully borne out." (p. 109.)

Dr. Barton writes somewhat obscurely, a fault much too common with medical authors; but the facts he records are interesting. The want of *exact* coincidence between Philadelphia and Louisiana he thinks may be explained by the circumstance, that the latter State (including New Orleans) is in a " transition-state." Dr. Barton points attention very emphatically to the *hygrometric* condition of the atmosphere, extreme variations in which, he shows, exercise an important influence on public health, the development and spread of epidemics, &c. We regret that our restricted space will not permit of extensive quotations. We trust, however, that Dr. Barton will steadily pursue his investigations on this point of medical meteorology, as we are satisfied that important results will ultimately reward labours sufficiently persevering. Referring to the modifying influences of bodies of water, Dr. Barton remarks :

" My impression is, that Lake Pontchartrain will actually one day materially aid in protecting New Orleans from the violence of pestilences, by furnishing a moderate moisture to the atmosphere, and lessen that desiccating power that usually prevails at those periods when the swamps to the east and north-east of us are dried up. The modifying influence of a body of water of less than a mile in breadth, is conclusively shown by the difference between the two banks of the Mississippi river, where it runs east and west, the south side having a milder climate—vegetation earlier advances in the spring—the cane has a longer period to mature in autumn, and fruits that are occasionally cut off by the severity of weather on the north bank, are uninfluenced on the other." (p. 115.)

We ought just to remind Dr. Barton, that he would not do well to ignore all experience as to the action of *marsh* emanations. He seems, also, to overlook an important point in the hygrometric relations of the atmosphere to epidemic disease—namely, that the aqueous vapour therein renders febrific emanations more *soluble*, consequently more accumulative in the atmosphere, and more transmissible from person to person, or from town to town. Hence the effect of sudden *calms* (calms being always attended, as Dr. Barton's tables prove, by a large increase of vapour) on the outbreak and spread of epidemics; hence, also, in an auxiliary degree, the injurious epidemical influences of damp situations and dampness. On the other hand, it is well known, that certain epidemics are arrested by states of the

atmosphere, in which it is nearly deprived of vapour. Thus, an intense frost, or a very dry wind (as the sirocco), will stop the spread of variola. Some such hygrometric cause is in operation in the village of Lowndesboro', Alabama, of which Dr. Wooten relates the following circumstance:—

"The village is divided by a street of one hundred feet, running north and south, there being but a single row of habitations between the street and the brow of the ridge; and it is a fact, very generally observed and admitted, that persons residing in this row west of the street are more subject to disease than those on the east. In 1845, there occurred a remarkable circumstance, bearing on this point, which may be worth relating. A mixed and irregular form of scarlet fever prevailed. There were, upon the west side of the street—the dwellings generally fifty or sixty yards from it—eleven families, most of them containing a large number of children. The three first cases occurred on the same day, at three different houses; one at the north end of the street, another three hundred yards south of this, and the other two hundred yards still further south, between which and the south end of the town there was but one more house on that side of the street. Upon that side of the street there occurred about thirty-five cases and four deaths, while not a single decided case occurred on the east side, where the larger part of the population resided. This visitation was not brought hither by contagion; every fact disproved the idea, and it was not transmissible from one person to another here, because families within a few yards of the sufferers escaped entirely, while communication was not forbidden, and many members of the suffering families escaped it." (p. 334.)

Such statements are not uncommon in the histories of contagious and infectious epidemics, and deserve much more careful investigation than they have had hitherto. The reader will not fail to observe how curiously similar the facts and inferences in the above extract, regarding the outbreak and spread of scarlet fever, are to many stated in regard to epidemic cholera, even to the same assumptions, and the same *proving by negatives*.

The influence of *barometric* changes has been investigated by Dr. Barton, by means of the vital statistics of Mexico.

"This large and magnificent capital of that once wonderful people is situated at an elevation of about 7700 feet above the level of the sea, or our level [New Orleans], and accordingly, disease is here modified by a pressure and elasticity due to a removal of near 15,000 pounds weight . . . . and what we should theoretically anticipate from this condition of things, is actually found to take place, and that the diseases of the *thoracic cavity*, with a few of the liver (and these mostly of abscess), and a large proportion of dropsies, contribute nearly thirty-four per cent. of the entire mortality, calculated from an aggregate of a series of years most carefully by myself." (p. 118.)

Dr. Barton also calls attention to the recurrence of meteorological cycles, especially that of seventeen years—a cycle made out principally from the recurrence of cholera and other epidemics, and from the fact of the cane dying and being reproduced, flights of pigeons, visitations of locusts, mayflies, &c., at intervals of seventeen years.

Epidemic boils and abscesses appear to have been prevalent during the year 1850 in certain portions of the United States. This is an interesting fact, because they have also been unusually prevalent during the last two or three years in the United Kingdom. Dr. Andrew Kilpatrick, in his report on the 'Medical Topography, &c. of Trinity, Louisiana,' observes:

"There have been an unusual number of abscesses and boils since the subsidence of the water. I have inquired of my *confrères* the cause of this, and the opinions

are various. One charges it to the late overflow, another to the want of vegetation, and another to the dry fall, by which the blood has become thickened and enriched." (p. 178.)

Dr. Arnold noticed that prickly heat and boils prevailed to a great extent in Savannah (Georgia) during the summer of 1850, previously to an outbreak of the "dengue" (the scarlatina rheumatica of Copland), which we shall presently notice. Precisely the same facts were observed with reference to Sullivan's Island, South Carolina. In June and July, 1850, "prickly heat (*lichen tropicus*) was universal—from infants to threescore and ten; *lichen urticatus* common; herpes frequent; boils were almost universal; and carbuncles were common." And again, the same writer (Dr. Porter, of the United States army) remarks, as to the phenomena which ushered in the "dengue,"

"On a person covered with prickly heat, more than twenty small boils from one elbow to the wrist were counted, answering the description of the phlegmonoid variety of the denguists, but the person in question did not have a single eruption of the epidemic fever. Cases occurred in children, long before the epidemic broke out, in whom boils were almost universal on the chest, neck, and scalp, there being at the same time nearly a perfect sheet of prickly heat. They were common before the epidemic, and were regarded as estival." (p. 379.)

Dr. Dickson, of Charleston (South Carolina), notices a similar state of things in that city, in his 'History of the Dengue.' He gives nine varieties of the eruption which accompanied the fever—as to his seventh, the "phlegmonoid," he observes:

"It was ascribed to the heat of the season that boils were so common and troublesome, not only to children, but among adults. Great numbers suffered from these annoying tumours, and carbuncle also was of frequent occurrence. But it was among the patients recovering from fever, or in its second stage, that these affections were most complained of, seeming in many to take the place of the more ordinary eruptions." (p. 388.)

In those cases of dengue, in which the pulse was slow (so low as forty) during convalescence, the cutaneous affection assumed the erysipelatous character, or was carbuncular. The intense heat of the summer of 1850 was also remarkable for being protracted, and this would suggest a cause for the cutaneous affection, if it had been general, which it was not. Professor Dickson remarks:

"Cutaneous eruptions were almost universal [in Charleston]. Adults, who had never remembered themselves affected with prickly heat, were covered with it. The cutaneous integument was in all, either irritated or excessively relaxed with profuse sweating, or both. Boils were a common infliction, both in children and adults. I think I can safely say, that in thirty years' practice, I never saw anything comparable with it. . . . . Anthrax was also of singularly frequent occurrence, and tormented some of the most temperate and healthy of our population. There seems to be some reason to expect that the skin would be the seat of special determination in any form of disease that might arise under such contingencies of atmospheric condition. But we do not thus arrive at any suggestion of sufficient cause for the effect;—the latter being local and circumscribed, the former very extensively diffused." (p. 393.)

Dr. Wragg, of Charleston, gives rather a different account of these boils in relation to the "dengue;" we subjoin his statement, because it corresponds, in some particulars, with the origin and progress of the disease in this country.

"In some cases the fever appeared to be wanting entirely. The patient would suffer for one or two days with pain in the back, head, and limbs, with giddiness, foul tongue, nausea, restlessness, and fatigue, all of which symptoms would gradually wear off, and then would follow that extraordinary weakness, which was perhaps the most important of all the symptoms in a diagnostic point of view. After a few days, the eruption would come out in the form of heat, boils, carbuncles, or abscesses. This array of sequelæ would satisfactorily indicate what had been the matter." (p. 400.)

It is to be observed, however, that Dr. Holt, in his letter to the editor (Dr. Fenner), describing the "dengue" as it prevailed in Woodville, Mississippi, makes no mention of boils, either as premonitory to, or coincident with, the disease. The eruption is described by him as being similar to that of measles, although, in a few cases, it was that of nettle-rash.

In this country, the extraordinary prevalence of boils, during the last two or three years, has been generally noticed; but there has been no particular mention made of it in the journals, except in a clinical lecture by Dr. Laycock, of York,* and one or two casual notices in the *Lancet.* Within the last few months there have been, also, allusions to carbuncular disease, in the returns of mortality of London issued from the Registrar-General's Office. In the return for the quarter ending March 31st, 1852, it is remarked (p. 4), " one of the most singular facts in the return is the increase of *deaths* by carbuncle, which, from an average of about two, rose to seventeen in the quarter." If we take " phlegmon" to be an allied affection, it is observable, also, that the deaths from that affection were nearly double the average. These facts show that a high temperature is not the exciting cause. This state of things continues still, for we find the deaths during the first half of the *spring* quarter to be, " carbuncle," 6, " phlegmon," 7. It is worthy of notice, too, that when boils and carbuncles were so prevalent in Charleston, Sullivan's Island, &c., that is to say, during the summer of 1850, there was an increased mortality from carbuncles in London, the number registered during that summer being 9. Dr. Laycock had noticed this increased prevalence about a year and a half previously to the date of his lecture, but they had evidently been most frequent in the summer and autumn of 1850. He describes the disease as continuing for from two weeks to six weeks. The eruption assumed three forms, the ecthymatous, or phlyctænoid, the furuncular, and the carbuncular—two or more of these occurring in the same case. It was attended by general disturbance of the health, languor, loss of appetite, and feverishness. If persons thus affected applied a blister to the skin, it would immediately be followed by a crop of boils. So, also, the irritation of a poultice, applied to a large or carbuncular boil, would develope a line of furuncular satellites. Dr. Laycock had positive information of the spread of the disease over a great part of England. Warm purgatives, the warm bath, and quinine, were found the best remedies, with water-dressing to the boils.

As to the origin or etiology of this disease, little is known. Dr. Laycock refers to the literature of the subject, and traces out the relations of similar epidemics and epizootics, which the reader would do well to refer to. We have a very ancient record of a furuncular epidemic in one of the

* On a New Epidemic Exanthem: a clinical lecture delivered at the York Medical School, Feb. 25th, 1851, by Thomas Laycock, M.D., Physician to the York Dispensary, &c. London Medical Gazette, of March 7th, 1851; and The Medical Times, of March 8th, 1851.

books of Moses—namely, the plague of blains and boils of Egypt; and, what is still more remarkable, in reference to the *epizootic* origin of the disease—an idea which Dr. Laycock seems to entertain—it followed immediately upon an epizootic amongst the cattle of the Egyptians—" a very grievous murrain." The word translated " blains" is, in the Septuagint, " phlyctides," another point worthy of notice. The interest this epidemic furuncular disease is at last beginning to excite in England, has induced us to call the attention of our readers on both sides of the Atlantic to these various facts, with the hope that a further and more enlarged investigation may be made into its etiology.

The Dengue—Concurrently with the boils, and subsequently thereto, an epidemic fever was universally prevalent throughout the population of certain towns, termed the " Dengue," or the " break-bone fever." Dr. Copland has already given this a place in nosology, under the term " Scarlatina Rheumatica." * It has had various other names given to it, as " dandy," eruptive arthritic fever, &c., as well as the term " peculiar," evidently because it is not described in systematic works. It is not a little remarkable, that none of our most recent systematic writers have taken notice of it, except Dr. Copland, although several practitioners had recorded their observations on the disease when it was first noticed, twenty-five years ago. *E. g.*, Dr. Wood, of Philadelphia, gives no account of the epidemic in his systematic work, yet his countryman, Dr. Dickson, fully described it when it first appeared in Charleston, in 1828.

Although Dr. Copland states that " the first account of its existence was brought from Rangoon, in the East Indies, in May, 1824," it seems probable, as Professor Dickson shows, that it is the same disease described by Rush in 1780. It was in Calcutta, however, in June, 1824; in Berhampore and places in its vicinity, in March, April, and May, 1825; in the island of St. Thomas (*West* Indies), in September, 1827; in New Orleans in the spring, and in Savannah and Charleston in the summer, of 1828. It is not improbable that it was in Paris about the same time; as a very curious epidemic, similar in one or two leading points to this fever, prevailed in certain districts of that city.

Dr. Mouat, who saw it at Berhampore in 1824, says it was characterized by " the suddenness of its attack, the redness and watering of the eyes, the acute pain in all the joints, rendered excruciating on the slightest touch, the scarlet or crimson efflorescence on the surface, and its sparing neither age, sex, nor habit of body." We quote the following from Dr. Holt's article on the Dengue, as it appeared in the village of Woodville, Mississippi, in September, 1850. It commenced about the first of that month, and by the first of October there were very few in the village who were not either in bed, or just recovering from it. The duration of the attack was generally from four to six days.

" The precursors of the attack were those usually attendant upon ordinary influenza, patients often expressing themselves as feeling 'like they were taking cold;' then a distinct chill, followed by a high grade of febrile excitement, accompanied with agonizing pain in the head, eyeballs, back, and extremities, and oftentimes a stricture across the chest. The eyes, in a majority of cases, presented a streaked appearance (yellow and red); the tongue broad and moist, but heavily

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* Dictionary of Practical Medicine, vol. iii. p. 662.

coated; thirst slight, and, except in the advanced cases, often altogether absent; and the bowels generally evinced a tendency to constipation. During the first days of the attack, the restlessness was a most distressing feature of the disease; and throughout, the mind, whether the patient were asleep or awake, seemed in a dreaming condition [delirious?]—confused thoughts and painful visions being constantly present. An eruption, similar to that of measles, made its appearance in many instances; while, in a few cases, the eruption was that of nettle-rash. The fever had no tendency to remission—one paroxysm with no return, except from too great haste on the part of the patient to leave the sick-room. The debility following the attack was excessive; and I could not see much difference in this respect, whether the attack was mild or severe. It is proper here to state the interesting, and to those who have once suffered with this disease, the very agreeable fact, that in the fall of 1848 there occurred in Woodville some fifteen or twenty cases of dengue, and that but one of those was attacked last fall. These were more exposed, too, than any other citizens of the village, from the fact that they were the only individuals that escaped in their respective families, and consequently suffered greatly from loss of rest and fatigue in nursing. All who visited Woodville from the surrounding country, during the prevalence of the disease, were taken sick soon after their return home. The country, during the summer and fall, was generally very healthy, notwithstanding." (p. 43.)

We have selected this *village* description, because we think epidemics generally are more complicated in large towns with other febrific causes, or modifying agencies.

In Charleston—to mention an example—scarlet fever both preceded and followed the outbreak; hence the probability that the poison of that epidemic rendered the phenomena of the dengue of a mixed character. In the same way arose, probably, some of the varieties of the eruption observed by Professor Dickson—as the erysipelatous, variolous or varicellous, phlegmonoid, purpurous. The miliary or urticarious, and the scarlatinous, rubeolous, lichenoid, and papulous, seem forms of the specific eruption. Professor Dickson (we may here observe) is singularly inelegant and unclassical in his nomenclature; the termination in *ous*, in the greater number of the terms used by him, is quite unwarranted either by idiom or custom. The eruption was frequently followed by desquamation. The disease, according to Professor Dickson, came on slowly in some cases, but in the majority suddenly and violently. In these,

"'The patient was seized with severe headache, intolerance of light, and universal distress and oppressive debility. The skin was hot and dry; the face flushed; the eyes red and watery; the pulse tense, quick, and frequent. The patient was exceedingly restless, and soon complained of intense pains in back and limbs, and large joints. In some the stomach and bowels were the seat of violent pain. The febrile paroxysm was of very various duration; and the local determinations might also vary, except as to the head, which, as far as I have seen or heard, was universally affected. In some, there was delirium throughout the febrile part of the attack. I noted, carefully, in several, the length of the first, or febrile stage. . . . . The average, I think, was about thirty-six hours. In a few instances, it lasted for two and three, and seldom for four days. I saw no remissions nor intermissions, but heard of many cases, both remittent and intermittent. There were in the fully-formed attack two very definite stages, with an interval clear of fever. This interval, of two to four days, was sometimes so free from suffering, that patients would rise from bed and lounge about the house, complaining only of debility. . . . But on the fourth or fifth day, they would again yield to oppressive malaise and weakness, and seek the recumbent posture. A return of pain in the head and limbs, if it had disappeared,—often a recurrence of febrile excitement,—would mark the access of this second stage. The tongue, which until now had

been clean and red, would put on a thick, yellowish fur; the stomach would be more or less nauseated—very seldom to the extent of retching or vomiting; and this state of things would remain until, on the fifth, sixth, or seventh day, some cutaneous eruption would exhibit itself, usually with a sense of heat and itching. . . . With the subsidence of the eruption all ailment usually disappeared, but slowly and gradually. Almost all complained of a most annoying degree of muscular weakness." (pp. 385, 386.)

In some the eruption occurred in the first stage, but these had in it the second also. The epidemic was universally prevalent; numerous large families were attacked, without a single exception. "In any given domicile the attacks were apt to be simultaneous, or in rapid succession; so that it often happened that there was in a family no one well to attend the sick." The latent period was in some instances only a few hours, seldom longer than five days. Visitors to the city who remained only a few hours, were often attacked soon after their return home. The prognosis was very favourable, death by the disease being very rare. For other particulars we must refer to Professor Dickson's very lucid history, only making one additional quotation. As to the immunity from a second attack, mentioned by Dr. Holt, Professor Dickson, (who was the only exempt in his own family), having had it probably in 1828, observes—

"I am persuaded that the dengue of 1828 gave a certain degree of immunity from the epidemic of 1850. On looking over my notes, I find among the names of those whom I then attended, several of the few exempts of this year." (p. 392.)

The question arises, What is the nosological position of this epidemic? If we look only at the eruption, we ought to place it between scarlatina and rubeola. Epidemics of scarlatina, however, occur (as all practitioners practically acquainted with the disease well know), manifesting important variations in the leading symptoms. It has been observed occasionally with rheumatic pains, and even redness and swelling of the joints, as a leading symptom. In this respect scarlatina presents closer relations to the dengue, than rubeola. Nevertheless, there are important differences between the dengue and scarlatina, which are so obvious that we need not mention them, and which as obviously ally the new epidemic to influenza. There is the same suddenness of attack, the same cerebral disturbance, the same prostrate debility, the same universality and simultaneousness, the same brief latent period, the same evidence of infectiousness, the same immunity from a second attack. The principal difference is, that it is the skin, and not the pulmonary mucous surface, which is the seat of the disease in dengue; and perhaps it is this which renders the articular and muscular affections more *prominent* in the latter, for they have been observed in influenza pretty constantly, in a greater or less degree. The scarlet eruption has also been observed, as in Bath, in the epidemic of 1782, and on various other occasions. "In the greater number," observes Dr. Nutty, of Dublin, "a sweat about the third day proved critical; yet in several there was observed an efflorescence on the skin, like the measles, or a red rash with violent itching, which was observed to carry off the disorder more completely than the sweating."*

* Annals of Influenza, or Epidemic Catarrhal Fever in Great Britain from 1510 to 1837. Prepared and Edited by Theophilus Thompson, M.D., p. 81.—This is one of the publications of the Sydenham Society, and will afford the American physicians abundant means for a comparison of influenza with the dengue, such as no other work could possibly afford. It is an excellent illustration of the value of this class of reprints, and of the usefulness of the Society.

We could mention other points of similarity, as occasionally enlarged glands and sore-throat, sense of constriction of the thorax, red and watery eyes, intense headache, absence of thirst, the duration of the fever, &c.; we have, however, probably mentioned a sufficient number of important points of similarity between influenza and the dengue, to attract the attention of American physicians to the circumstance. An interesting point of *difference* is in the mortality; because it illustrates how little dangerous the merely febrile disturbance is, as compared with the local complications. In the two diseases we have compared, the febrile phenomena are about equal in intensity; but the local disorder being, in the one, seated in so important a surface as the respiratory, the mortality is comparatively very high; in the other, being limited to the derma, it is very low.

There are other articles in this volume well worthy of notice; particularly one by the editor on lead-poisoning, from the lead contained in the water supplied by public companies, in soda water, &c.,—a very interesting and important document, in which the author endeavours to show that this is the hidden source of numerous diseases in town populations. The evidence which he adduces is, for the most part, of a kind already familiar to us; but we think it worth while to transfer to our pages the following very remarkable case of lead-poisoning, which extended over a period of *four years.* We give the case without abridgment, that our readers may form their own judgment of its character. It is recorded by Dr. Edward Murphy, of New Harmony, Indiana, who thus introduces it:—

" I submit the following case to the profession, in the hope that it may lead to further investigation into the causes of neuralgia, convulsions, cachexia, paralysis, amaurosis, mental derangement, &c.; believing that, in many instances, these affections, with other anomalous phenomena, may depend on a latent poison either taken into or generated within the body. I am further urged to its publication by my patient, who is of the opinion, that many cases of confirmed bad health among mercantile men, especially in small towns, may have a like insidious origin.

" In giving the history of cases of indisposition, extending over a long period of time, there must necessarily be a good deal of repetition in describing the state of the patient from time to time; but as I am strongly impressed with the belief that the above-named affections may originate from a poison in the system, I have been particular in giving the condition of the patient, throughout the entire period, at different times.

" Mr. R——, merchant, aged 42, of medium height, and rather stout habit of body—of bilious temperament, and sound intellect—has always enjoyed good health, and had no hereditary liability to disease; has always been temperate, and a close, but active, business man. During September, 1843, had a slight attack of autumnal fever of short continuance, and throughout the following winter had been often afflicted with pains in his abdomen, which disturbed him a good deal.

" About the last of February, 1844, was confined to his bed for several days, with excessive, intermitting abdominal pain, and obstinate constipation of his bowels, but, he thinks, without fever, and was treated by his physicians for an attack of acute peritonitis. The constipation was very obstinate, and only yielded, after several days, to very large doses of medicine. But I consider it impossible that acute inflammation within the abdomen should have continued so long as this attack did, without producing some organic change among the abdominal viscera.

" After an imperfect recovery, Mr. R. went to Louisville on business, during the following March, where he was again attacked with the same symptoms— though not quite of the same severity—and was attended by a distinguished

physician, who pronounced his disease to be abdominal neuralgia, stating, that it was a rather frequent complaint among mercantile men in that place, and prescribed accordingly. He also gave it as his opinion, that his former attack was the same disease, and not peritonitis. Since that time, Mr. R.'s complaint has been considered neuralgia, and treated as such.

" From that time up to the 22nd February, 1846, he has been suffering, almost constantly, with excessive pain in his abdomen, radiating from thence to all parts of his body, often of very great severity; obstinate constipation of his bowels, accompanied often with nausea and vomiting (the patient attributing the nausea and vomiting to the *very* large doses of opium which he was sometimes obliged to take); was frequently confined to his bed; he lost flesh and strength, notwithstanding a constant good appetite, and had a bloated, though anæmic, countenance. He had very much the appearance of a person in cachexia from malignant disease. There was a dirty yellow colour of the skin, with a yellow discoloration of the albuginea oculi, simulating jaundice the whole time. Sometime during this period, he became affected with slight paralysis of the extensor muscles of the fingers of the right hand, with the exception of the index, which rendered him unable to write; his vision became imperfect; there was great mental prostration, approaching hypochondriasis,—indeed, he was totally unable to do business throughout the greater part of this period, from mental imbecility, sometimes being unable to perform the minutest calculation, or to attend his customers, who generally considered him insane; was very irritable the whole time.

" About this time, Mr. R. was attacked with what was thought to be apoplectic fits, having had four or five, and on the 24th I was called in consultation. He was confined to his bed, very pale and feeble; sensible, although very weak in mind; would give an answer in relation to his case, and immediately forget that he had done so; sometimes became alarmed at persons present, and again was much terrified at absent imaginary enemies, who were conspiring against him—a state resembling delirium tremens; speech, faltering and hesitating; sight, defective. His face was frequently affected with choreic convulsions, when he would complain of severe shooting pains through his body, and of which he was in constant dread; tongue, soft and broad; pulse, feeble, but almost natural as respects frequency; bowels, constipated; stomach, very irritable; chest, perfectly sound; sounds and rhythm of the heart natural; nothing unusual in the appearance of his urine; although very feeble, would sit up for a short time when desired. Considering it impossible that an individual should have four or five fits of apoplexy in two or three days, without any lesion to the brain, or symptoms denoting such, and on carefully interrogating his family—the physician in attendance not having seen him in a fit—I made out his attacks to be of an epileptiform character, being preceded by the horrid scream of epileptics, accompanied with evident convulsions. I advised opening the bowels by active purgatives, opiates, nourishing diet, blister to the nuchæ, and sulph. quinia, when the bowels were well opened, and took my leave, after assuring his family that I did not consider his present attack to be apoplexy, but probably a part of his old complaint, and gave an unfavourable prognosis.

" Mr. R. remained in nearly the same state, but without another fit, until the 3rd of March, when I was again called in and associated in the treatment of his case. By persistence in the above remedies, to which was added wine and brandy, he very gradually recovered to his late state of health. When so far recovered as to be able to sit up, his defective vision became almost complete amaurosis, which continued some time, then gradually disappeared, but was not entirely recovered from; the patient was fully of the opinion that it was caused by the quinia he had been taking, although never more than six grains in the twenty-four hours, and with no idiosyncrasy to its action. There was also, at this time, increased paralysis of the right hand, the left also becoming slightly paralyzed.

" From the 16th of March, at which time my attendance ceased, up to January, 1847, when he placed himself in my hands for treatment of fistula of the anus,

complicated with fissure, he continued to have the same attacks, of greater or less severity, with only short intervals of repose, being nearly worn out with constant suffering and bad health. As opium was his only relief, he generally prescribed for himself throughout the lengthened period of his sickness, except when his attack was unusually severe. After the cure of his fistula, his disease returned with greater severity, and of a more alarming appearance than ever.

" On the 10th of June, in the absence of his regular physician, I was again consulted. Mr. R. was confined to his chamber and almost to his bed, the mere wreck of his former self, scarcely able to sit up, weeping from excruciating pain, and in such a state of mind as to express a wish to commit suicide, and indeed he was afraid he should do so. His face, pale and wan, was marked by the deepest despair, from extreme suffering, imploring me strongly for relief; wrists entirely dropped, from complete paralysis — being perfectly helpless, and unable to straighten either hand, unless by the aid of the opposite arm, and requiring all the care of an infant, in being fed, washed, &c., yet a comparatively good grip with his hands. His extremities were dwindled away to the mere sheaths of the muscles ; his abdomen seemed to be the centre, as usual, from which his pain radiated, and it was with the greatest difficulty that I could persuade him, after a careful examination, of the non-existence of organic disease there. The slightest touch of the skin over the umbilicus, and indeed over other parts of the body, produced such terrific pain as almost to throw him into convulsions, producing all the effects of an electric shock ; while the greatest pressure over the same place gave him no uneasiness, but rather relief: his bowels were always constipated, unless moved by medicine. Was the constipation produced by the large quantity of opium which was taken, or did it depend on paralysis of the muscular tunic of the intestines? There was sometimes vomiting of a greenish watery fluid ; tongue flat and broad ; pulse very feeble, and more frequent than natural ; his cachectic appearance was that of a person in the last stage of malignant disease ; appetite comparatively good; his suffering was much more intense during the night than the day, unless relieved by excessively large doses of opium. From the balls of both thumbs, which were much atrophied, excruciating pains would arise, shooting with great severity up his arms and shoulders, to the back of his neck and head ; the shoulders were affected with constant pain, especially the deltoid muscles, which also were slightly paralyzed. The pain in his lower extremities was also very severe, commencing in the soles of his feet, which were so sore that he dreaded to touch the floor with them, and shooting up the limbs to the lumbar region with dreadful suffering. There was also at this time a new source of suffering—shooting pain through his testicles, of such severity as almost to produce fainting ; indeed, to see him in his suffering, was the most heart-rending sight I ever witnessed, and I was greatly astonished to see how any human being could so long survive so much and such constant misery.

" I stated to Mr. R., which I had done several times before, though not when attending him, that he presented in the strongest light all the symptoms of poisoning with lead, and had it been possible that he could in any manner have been exposed to its influence, I should have no hesitation in attributing all his sufferings and bad health to that cause : but Mr. R. was a merchant, and in no way liable to be acted upon by lead, or any of its salts, in his business. There were no lead pipes or utensils used about the house ; nor had he taken it in any form as medicine during his whole life. The autumn before the commencement of his sickness, he built a new store and repaired his house, which were painted in the usual manner ; and this was the only exposure to the influence of lead to which he could refer. I, however, considered that this could not be the cause in itself, as I thought it impossible that its influence could have extended over a period of nearly four years.

" His case seemed perfectly hopeless, and I firmly believed he would never leave his chamber again alive. As all the remedies recommended for neuralgia had been exhausted without any benefit, and as he had taken so much medicine

from time to time, that his stomach gave way almost at the bare mention of it, I felt very much at a loss what to advise. I, however, advised Mr. R. to submit to an alterative course of mercury, as a last resort; giving him to understand that I considered neuralgia, convulsions, and various anomalous affections, might depend upon a cachectic state of the body, from some poison either taken into or generated within it, and preventing its proper nutrition, and which might be controlled or removed by a course of mercury, as constitutional syphilis and malaria often were; at any rate, it was possible it might produce a new action in his system. This he dreaded very much, and offered a great many objections, which I removed; but he declined for the present.

" June 17.—To-day Mr. R. consented to take mercury. I gave him a one-grain blue pill, four times a-day, with an occasional aperient, and continued the opium to relieve his sufferings. I applied blisters over various parts of his spine, which increased his pain so much, that I was obliged to heal them directly. This treatment was continued about three weeks, with an occasional rubbing-in of mercurial ointment over his abdomen, when a considerable improvement was manifest. Treatment continued.

" July 15.—Was summoned to Mr. R., when I expected another unfavourable turn in his disease had taken place, but was agreeably disappointed in finding him much relieved and improving, and down stairs. The statement which I had before repeatedly made to him, that he presented all the phenomena of poisoning with lead, made a very strong impression on his mind, so much so, that it constantly occupied his mind, and just brought to his recollection that he had been in the habit, for many years, of chewing lead, and that this habit extended so far back that he was unable to date its commencement. Formerly, being very fond of his gun, he frequently took hunting excursions, on which occasions he always had a piece of bullet or shot in his mouth; when in the store, he seldom ever passed by the box containing the shot without putting some in his mouth to chew. But what he most liked, from its agreeable taste, and of which he chewed a great deal, was the lead lining of tea-boxes; besides, he considered that the pressure of the teeth on the metal enabled him the better to bear his pain. I immediately replied, that the cause of all his suffering and bad health was perfectly clear, and at once assured him that he might yet be a sound man. I at once examined his gums, for Dr. Burton's symptom, and found the blue line over four or five teeth. I considered the case fairly made out, and never felt so much rejoiced as at that moment, to think that an individual, after such a prolonged period of suffering and bad health, whom all considered as beyond recovery, and almost in the grave, should, by this discovery, be yet restored to health and usefulness. Not so my patient, however; he was very sceptical of my prognosis, not conceiving it possible that his disease could have originated from what to him appeared so slight a cause. I assured him that his case always appeared a very strange one to me, and I was always astonished to think that a healthy individual, as he had always been, should have been reduced to such a protracted state of bad health, without any organic disease, unless from some evident cause, which had, at last, been discovered; that it was now rendered almost certain, that his first attack—which was considered acute peritonitis—and many subsequent ones, were attacks of lead colic. Further, that his attack of autumnal fever, from which I date the commencement of his disease, had probably produced a debilitated state of the body, rendering it more susceptible to the influence of minute portions of the metal. Also, that nearly every symptom which writers have laid down as indicating poisoning with lead, had in this case been repeatedly and severely manifested; and the only reason why he did not before recover, was the continued renewal of the poison whenever he was present where it could be obtained. Also, that we were now in a fair way of proving it; the cause being discovered, would in future be avoided, and he would continue well. I pointed to the present amelioration of his disease from the treatment he was pursuing, as a favourable indication that it depended on some removable cause, as idiopathic neuralgia of such long standing

was seldom benefited by any treatment. From all this, it will be seen that I had to urge a number of reasons to convince my patient of the real nature of his case, but without convincing him. I added acid sulph. aromat. to the former remedies, and by the 1st of August his pains had nearly entirely subsided, his bowels were acting naturally, and he left off medicine, even opium, for the first time since the commencement of his sickness. I ordered splints to his wrists and hands, which gradually recovered their natural state.

" Mr. R. entirely recovered his health in every respect, and has continued well up to the present time (February, 1850), being again a *strong, active* business man. *He thinks* that the extensor muscles of his wrists and fingers are not *quite* so strong as before his disease commenced; which is probably true, as muscles which have long been inactive require frequent and strong exercise to recover their proper tone, which cannot be given to these muscles; their function being merely extension, they cannot be exercised to any extent: however, the defect is very slight indeed.

" *Remarks.*— 1st. In consulting all the authorities within my reach, I have not found a single case having any resemblance to this; yet nearly every symptom described by the various authors I have consulted as belonging to or arising from poisoning with lead, with the exception of apoplexy, which generally terminates the case in death, were all combined in this case, and fully manifested. Indeed, this case would in *itself* afford all the materials for a complete history of the toxicological effects of lead on the human system. It also shows that the effects are the same, whether produced by the metal itself, or its various salts; probably the former is converted into one of the latter, in all instances, before it is absorbed into the system. Further, I think it may be reasonably inferred, from this case, that lead is a cumulative poison; and when once the body has been brought under its influence, very minute quantities are sufficient to keep up its effects. Also, that although the effects of poisoning with lead are very distressing, yet it cannot be considered as very dangerous to life, even when long continued in small quantities.

" 2nd. We have here additional evidence of the effects of a latent poison, either taken into or generated within the body, in producing and keeping up neuralgia, convulsions, cachexia, mental imbecility, paralysis, amaurosis, &c., showing the necessity of tracing these affections, and various other anomalous phenomena, to causes of this nature, in many instances.

" 3rd. The beneficial influence of mercury in controlling the effects, or removing latent poisons from the body, was strikingly manifested in this case. In consequence of the low state of the patient, I did not like to venture on a larger dose than the one given, believing it to be more advantageous to introduce the medicine gradually, than to produce a sudden or violent effect, which I considered would only add to his danger. Now, in inflammation the case is different; this process being certain to damage the part in which it is located, or to destroy life if not checked, we wish to have the influence of the medicine on the system as soon as it can be produced, where the case demands it. It was really curious to see how symptom after symptom gradually gave way from day to day, the patient gaining flesh and strength, and yet the only sensible effect of the medicine ascertainable was an indistinct mercurial fetor of the breath once or twice.

" 4th. This case is very instructive in another point of view,—*the great necessity in all cases of tracing diseases to their causes;* exemplifying the old adage, 'that knowing the disease is half its cure;' for it was only by the frequent repetition that his case resembled the effects of lead on the body, that the patient was brought to the recollection of his almost fatal habit.

" 5th. As the great drawback to the progress of the science of medicine is the almost universal tendency amongst its cultivators to draw sweeping generalizations from a single or a few cases, I shall close this too lengthy paper before I indulge *too much* that way; yet a great many beautiful illustrations of disease, and useful reflections, might, in my opinion, be drawn from this single case. I, however,

venture to think that no one, in reading the history of this case, its happy termination in perfect and robust health, the patient continuing so up to the present time, can for a moment doubt but that all the extreme suffering and disease before mentioned were rightly attributed to the cause assigned. — *Western Lancet, July,* 1850." (pp. 273—280.)

We must not pass over a very extraordinary paper by Dr. Samuel A. Cartwright, read before the Medical Association of Louisiana, in which the author endeavours to prove that the negro races constitute a debased species of man, so different by his natural conformation from the white race, that it is "absolutely necessary for the safety of the state and well-being of society, that the latter should be subjected to different laws and institutions from the former." In the course of his arguments, he represents British physiologists as distorting the facts of science,—mentioning "a certain Dr. Robert Bentley Todd,"—"also James Cowles Prichard, author of . . . *an abolition work,* disguised under the pretence that the authority of the Bible would be impeached," &c.,—meaning Dr. Prichard's immortal 'Researches.' The whole article breathes the spirit of a tyrant, and particularly of a tyrant over dark-coloured men. We might excuse the truculency and vulgarity of the author, as an associate of slave-owners, if it were not obvious that *one drop* of negro blood in his brother *white* man's veins would be to him a warrant to *sell* and *make gain* of that brother. It is, however, an agreeable circumstance to learn from Dr. Cartwright, that the large majority of the medical profession—his countrymen—repudiate his notions, and agree with James Cowles Prichard and "one Dr. Robert Bentley Todd." We do not fear that our brethren in the United States will be such renegades to science and humanity, as to allow themselves to be biassed either by "British abolitionists" or Louisiana man-dealers. They will, we are satisfied, diligently and (if let alone) safely work out the difficult problem, which the presence of so large a number of the coloured races amongst the whites developes for solution. One proposition they will stand by—that oppression will never raise a debased race, although it will degrade a superior race ; and that the best way to elevate a man, is to treat him as a man.

We cannot close our notice of this volume, without mentioning one or two facts which we have collected from it. In its advertising sheets there are twenty public medical schools announced, as attached to either colleges or universities : amongst the names of the professors we recognise some with a European reputation. Again : the list of exchange journals (placed under the head of "literary notices") comprises thirteen medical journals or periodicals published in the United States. Two of these are reprints—namely, this journal and Ranking's Abstract; the remainder are American, and comprises one weekly, one semi-monthly, five monthly, and four bi-monthly periodicals. When we remember that the population of the United States does not yet equal that of Great Britain and Ireland, these two facts as to the medical educational institutions and periodical press of the former are very significant.

Art. V.

Maladies de l'Algérie.   Par le Docteur A. Haspel.

*On the Diseases of Algeria. Being an Account of the Causes, Nature,
    and Treatment of the Endemo-Epidemic Diseases of the Province
    of Oran.* By Augustus Haspel, M.D. Vol. II.—*Paris,* 1852.   8vo,
    pp. 430.

The present volume is no unworthy sequel to the one we recently introduced
to the notice of our readers; presenting, like it, a happy union of philo-
sophical generalization and practical tact, and dealing heavy blows at the
so-called " physiological" system of medicine, which still, in spite of all
that is said to the contrary, holds far too great an empire in the Paris
schools.   The former volume exhibited the pathogenetic effects of the
Algerian climate, as manifested in the Liver; and the present one is
chiefly devoted to the consideration of Dysentery, and the various forms of
Paludal or Miasmatic Fever.   For such of our readers as have become
familiarized with the subject by the works of our own writers upon the
diseases of warm climates, the views here advanced possess little novelty;
but it is interesting and instructive to trace out these analogies as deve-
loped by writers of ability, stationed in different, though similar, fields of
observation.

DYSENTERY.

M. Haspel's experience in Algeria soon convinced him, that to consider
this disease as a mere *colitis,* as taught in the French schools, was to
take far too limited a view of its pathology.   In the following extracts he
expresses his idea of its nature, as seen in Algeria, and of the relationship
it bears to the other endemic affections of the country :

  " To consider the entire disease as an inflammation of the colon, is to take within
our view a mere corner or fragment of the pathological picture, and to risk the
commission of serious therapeutical errors. . . . . . There are certain pathological
conditions, which, while they certainly possess great affinity to inflammation, must
nevertheless not be confounded with it,—such as congestion, vital turgescence, and
nervous erethism.   It is only in consequence of the disease having been incom-
pletely studied in its advanced stages alone, and upon the dead body, that it has
been set down as being always of a truly inflammatory character.   To get at an
exact idea of its nature, the affection must be especially examined at its commence-
ment, as well as in relation to its causes, its general phenomena, its progress, and
its treatment.   The utility of most of the published cases is impaired by their hav-
ing no commencement.
  " I am well aware that, in the present state of our ideas, and our inveterate
habits of thought, the statement that dysentery is a general affection springing
from miasmatic poisoning, and inducing vital reaction, especially in the alimentary
canal, in the form of congestion or inflammation of the large intestine, and some-
times exhibiting itself by hepatic fluxion or paroxysmal fever, will be received by a
great number of pathologists as a mere chimera. . . . . But the diseases produced
by paludal emanations are essentially the same, differing only by the forms they
assume or the more marked character of their symptoms.   Intermittent fever is,
perhaps, one of the most constant of these symptoms; but are we on this account
to consider it as the sole disease produced by marsh influences ?   May we not also
apply the term miasmatic poisoning to that which exhibits itself only by hepatic or
dysenteric symptoms ?   Then, dysentery, hepatic congestion, and intermittent

fever, become symptoms which are associated, and the expression of the different stages through which the general condition passes to attain the fortunate or fatal issue of the disease. Or is it more reasonable, taking (with the generality of practitioners) the branches for the stem, to consider these different symptoms as so many distinct diseases? It seems a more physiological procedure to indicate a single and identical morbid condition, differing only in its forms, and in the absence or presence of certain symptoms, and to apply to it the general denomination of *miasmatic affection, or poisoning,* than to invent, as is daily done, as many morbid principles, as there are distinguishable groups of symptoms.

"From what precedes, it results that the essential cause of dysentery is not to be found in an inflammatory condition of the intestinal mucous membrane, in the presence of acrid bile in the alimentary canal, or in a pathological condition of other tissues or organs. Any lesions of such organs are subordinate to a general condition, upon which our attention should be fixed—a condition resulting from the operation of the deleterious cause upon the economy, and, without doubt, consecutively to changes produced in the blood." (pp. 7—9.)

Forms of Dysentery.—The dysentery of Algeria manifests, as a general rule, very great differences, both in the symptoms and results, at different periods of the year. So much is this the case, that M. Haspel founds upon this fact his divisions of the disease. (1.) *The Spring Dysentery* is observed in the sporadic form during May, June, and July, and then usually becomes an obstinate disease only under the influence of injudicious treatment. The symptoms are mild, and health becomes at once re-established on their cessation, the disease rarely passing into the chronic stage. A chronic form is indeed occasionally met with, even at this season, in soldiers whose powers have become exhausted during prolonged expeditions. It usually supervenes on obstinate diarrhœa, into which it is very apt again to pass. Dysentery is frequently accompanied by some one of the various types of intermittent, which requires to be first subdued by the aid of quinine, before the dysentery is treated. M. Haspel has seldom, if ever, met with the *intermittent dysentery* of Torti and Dreyssig, in which the dysenteric symptoms themselves assume a paroxysmal character. M. Rietschel, however, states that this form of the disease is of common occurrence in the province of Algiers.

(2.) *The Autumnal Dysentery* usually prevails epidemically, and is a much more serious disease, some cases assuming the most malignant typhoid form. As autumn approaches, and remittent fever becomes prevalent, and complicated with a bilious element, the dysenteries assume a more intense form, and become also complicated with hepatic disease. In those years in which fever is most prevalent, dysentery is usually seldomer met with; but hepatic congestion always manifests a prevalence proportionate to that of dysentery. Autumnal dysentery may be observed under two principal forms; one of these M. Haspel terms *acute* or *bilious* dysentery, the liver being in a state of active hyperæmia, and on the verge of phlegmasia. After it has existed some time, typhoid symptoms may appear; and, in certain exceptional cases, these may set in at once. Death occurs, in the majority of fatal cases, by reason of purulent resorption, or from exhaustion from the number of stools. A far more serious form is the *putrid, malignant,* or *adynamic* dysentery, which is more especially met with when the colder and damp weather succeeds the heats of summer. The bilious symptoms are now promptly replaced by adynamic and typhoid ones, or these may manifest themselves from the beginning; from thirty-

six to forty-eight hours, in the worst cases, sufficing to induce a state of helpless exhaustion.   Treated with promptitude, many of these cases may recover; but convalescence is tedious, and often harassed by relapse or the supervention of chronic diarrhœa.

"In this season of the year, every individual seems to be endowed with an especial susceptibility to the development of typhoid symptoms, when he becomes the subject of dysentery, intermittent or remittent fever.   But these accessory phenomena—the stupified countenance, the restlessness, the heat of belly, &c.— quickly disappear at the same time with the principal disease, under the influence of an evacuating plan of treatment.   We must distrust the fulness of pulse, the false plethora, which manifest themselves during the prevalence of the great heats, and which seem to call for bleeding.   If we yield to this perfidious indication, we find our patients fall into a state of adynamia, without the dysentery undergoing any amendment; or if the abstraction of blood produces some relief, it is but a temporary amelioration, to be speedily followed by a sensible aggravation of all the symptoms." (p. 58.)

(3.) *The Winter Dysentery.*—Although it is rare for any new case to manifest itself at this period of the year, yet persons who have suffered from dysentery at other periods, and are exposed to excessive labours or insalubrious residences, are very liable to relapse.   The disease has, however, lost its original violence, and when not properly treated at first, leads to a most obstinate form of diarrhœa, which gradually, but completely, undermines the patient's powers.

Pathological Anatomy of Dysentery.—The changes which are observed in the intestinal canal are thus summed up:

"1. Partial or extensive injection of the mucous membrane, either with *ramollissement* or increase of consistence, and sometimes, but more rarely, thinning of this membrane, or of the entire walls of the intestine.—2. Superficial or deep-seated ulcers, which may invade all the coats of the intestine.   They are not constant, but are of such frequent occurrence that their importance is almost the same as if they were never absent.   Notwithstanding this, they are not to be considered as the mere result of a local affection.   They are only, as the lesion of other organs, one of the modes of manifestation of a general morbid condition.   At all events, it is certain that the intestinal mucous membrane possesses an extreme tendency to ulcerate readily in this affection.—3. False membranes, of greater or less extent, are found at the surface of the mucous membrane, to which they are connected by a series of minute bloodvessels. — 4. The mucous membrane itself may become detached, in more or less broad portions, being separated by sub-mucous sero-purulent collections.—5. Gangrene.   In some cases we observe shreds of livid, ash-coloured, or blackish mucous membrane attached to the intestine, and resulting rather from a necrosis of the gut than from a specific secretion.   Their intimate union with the rest of the intestine is shown in the difficulty that exists in separating them, without causing hemorrhage, at those points where the gangrene has made least advance." (p. 85.)

M. Haspel observes, that several cases in which it is related that large portions of pseudo-membrane have become developed at the surface of the intestines, were really examples of detachment of the mucous membrane itself.   The older writers have indicated such detachments, several examples of which are recorded by Morgagni and others.   M. Haspel himself met with a case in which a vascular, blackish, gangrened, friable membrane, half-a-foot long, was discharged *per anum;* and at the autopsy the rectum was found completely deprived of its mucous membrane, and of a portion of

its muscular coat. In most recorded cases it has been discharged only in fragments. Among the numerous cases of discharged membranes which have been recorded, the authors have usually been so pre-occupied with the idea of their *pseudo-membranous* nature, that they have not supplied the details necessary for testing the accuracy of their opinions. Two specimens obtained, however, by M. Catteloup, in Algeria, and examined by M. Begin and other competent authorities in France, have proved, without doubt, to have consisted of portions of the mucous membrane itself.

Treatment of Dysentery.—In the following passage, M. Haspel states his general views upon this part of the subject:

"The therapeutical indication is, in our view of the disease, a complex one; and it is, I believe, because too much importance has been attached to certain of the elements which constitute it,—as the organic changes, for example,—that so many disappointments have been experienced in these later times. Far more legitimate conclusions, as to what had to be done, and what had to be avoided, would have been drawn, if the attention had been carried back to a consideration of the nature, or probable nature, of the morbid process at its commencement, and the deduction hence derivable of the genius, the indications, and the curability of the affection.

"Although there have been a multitude of books published during several centuries, vagueness, uncertainty, and indecision characterize what their authors have communicated to us in respect to treatment. Hence the monstrous catalogue of anti-dysenteric and anti-diarrhœal remedies, which have been in turn proposed for the cure of dysentery and diarrhœa. With some, bleeding and leeching are made to predominate over all other measures, while others exclaim loudly against any kind of blood-letting. Some recommend an union of opium, ipecacuanha, and calomel; and a great number of practitioners regard each of these remedies administered alone as the true sheet-anchor. Emetics and purgatives are exclusively used by some, while others precede their employment by a single bleeding. Some have praised beyond all measure, albumen, nux vomica, &c.; and others have especially relied upon astringents. I have considered all these matters with attention; and having tried most of these means, I have, after ten years' practice in Algeria, selected a mode of treatment that has furnished the most satisfactory results. I do not mean to say it is the only one applicable in all places, or under all circumstances, even in Algeria. Dysentery is not a simple disease exacting always the same treatment; for it presents many varied shades and forms, of which great account must be taken, as each furnishes its special indication." (p. 108.)

Perhaps M. Haspel bears a little too hard upon his predecessors in some of the foregoing observations, inasmuch as he advocates the treatment by *calomel* quite as warmly, and almost as exclusively, as they have championed the cause of their respective remedies. Against the employment of *bloodletting*, his personal experience enables him strongly to protest. Having witnessed its employment with some success by M. Peysson, at Lyons, he put it into force on his arrival in Algeria. Even in cases of spring dysentery, occurring in robust subjects, he found its utility very equivocal; and in the majority of cases the most it effected was to procure a temporary alleviation of pain. As the year advanced, he found the symptoms of "false plethora" yield less and less easily, while in many cases they became rapidly exasperated; and in the autumnal dysentery it was found absolutely necessary to restrict its use to cases in which actual inflammation of the liver or other organ certainly existed. Even the application of *leeches* to the abdomen or anus was by no means always of use, while sometimes it was positively injurious—proving of use chiefly in

cases in which the dysentery was connected with an engorged state of the liver. Such cases were quite exceptional; and even when the dysentery was inflammatory, purely antiphlogistic means could not always be borne. Of all the therapeutic agents he has resorted to, M. Haspel gives a distinguished preference to *calomel*, which he believes produces its good effects by exerting a modifying influence on the languid abdominal circulation, and re-exciting the dormant action of the liver into activity.

"Under the influence of this agent, I have seen, day after day, the most severe dysentery and diarrhœa disappear with a wonderful rapidity; the affections so frequently complicating them yielding in like manner. These results, seeming sometimes miraculous, might indeed, in some cases, be attributed to coincidence or chance; but when they are daily repeated, and under the same circumstances,—and when after relapses we see the same symptoms reappear with all their primary intensity, and again cease under the influence of this same remedy,—it becomes impossible not to refer this to the relation of cause and effect, and to acquire an unshakeable conviction, since it is impossible we can be deceived by clinical facts, susceptible of verification by daily observation. The cure produced by this substance is indeed brilliant, and I do not believe that any other mode of treatment can furnish such results. But its success will not appear so extraordinary if we reflect, that almost all physicians who have recommended evacuants, have practised in marshy and southerly regions—that is, in climates in which the function of the liver has predominated, and in which the liability of the hepatic apparatus to become affected, impresses peculiar features on the lesions which are then manifested. And although Pringle observed the disease in cold countries, yet he remarked that the patients were usually much relieved when the evacuant acted so as to bring away much bile. It is this which has led observant practitioners to establish the distinction of bilious dysenteries—an eminently practical one, since its effect is to render more methodical our appreciation of the various therapeutical means. Is it not a wiser and a more medical procedure, in place of directing all our attention to the inflammatory phenomena of the mucous membrane of the colon, to examine when and how, in what seasons, and under what circumstances, these phenomena originate? If an obstruction of the circulation of the vena porta, due to an engorgement of the liver, or to poisonous miasmata, have given rise to them, of what importance is the congestion of the capillaries of the intestinal mucous membrane? If the dysentery depends upon an altered condition of the blood, and hepatic turgescence, this is the chief point for consideration; and want of success is very frequently due to the fact of the original cause of the disease not being attacked. It is, then, chiefly in the *bilious* or *hepatic* form of dysentery, that we have found calomel so useful." (p. 123.)

M. Haspel administered a single dose per diem of from fifteen to thirty grains, combining it with nearly, or quite, equal parts of ipecacuanha (to which he does not seem to attribute that share in the production of the beneficial effect which some would claim for it), and occasionally with a few drops of laudanum. In different seasons and cases, the relative proportions of the ipecacuanha and calomel were somewhat varied. In many instances, even the first dose arrested the dysentery with surprising rapidity; the abdominal pain, red tongue, and tenesmus, undergoing simultaneous amendment. Even when this amount of success was not obtained, each daily dose was followed by remarkable improvement. When old chronic dysentery had to be dealt with, the calomel was administered in divided and quickly-repeated doses, the quantity of laudanum being increased. When, towards the end of autumn, the disease put on the *putrid* or *adynamic* form, the powers of the patient required to be recruited before the medi-

cine could be actively given; and in the very chronic and advanced cases met with at this season, it was not admissible. So far from *salivation* being desirable, it should always be avoided, if possible; and, in fact, it is not easily produced in Algeria.

When the dysentery and tenesmus were intense, mild *laxatives*, such as manna, tamarinds, cream of tartar, or sulphate of soda, were given, in order to prepare the way for the calomel, and render salivation of less likely occurrence. In some cases the dysentery amended after their use, and in a few slight ones was even thus cured. These means were, however, not continued too long, owing to the tendency to intestinal ulceration. M. Haspel refers to the advantages derivable from purgatives recorded by the older writers, and he considers that their disuse, in more recent times, has arisen rather from the prevalence of theoretical views of the inflammatory nature of the disease, than as a result of experience. He speaks highly of the advantage derivable from the use of *emetics*, at the very onset of the disease, when they sometimes at once check its farther progress. During the damp winter season of Algeria, the administration of ipecacuanha in five-grain doses, two or three times a day, as recommended by Pringle, is a beneficial practice, by exciting the skin more effectually than by the ordinary emetic dose. Rarely of use during summer and autumn, and contra-indicated in bilious dysentery, *opium* succeeds best in the winter season. It is useful in cases where a chronic diarrhœa is the only remaining symptom, and where an obstinate flux seems to depend rather upon an irregular action of the skin than upon a changed condition of the mucous membrane. Its influence is, however, frequently quite temporary, the disease appearing as intensely as ever when it is discontinued. Its too long continuance only aggravates the disease, as already observed by Freund and Pringle. Of the class of *astringents*, M. Haspel does not seem to entertain a high opinion, preferring, however, catechu as the best among them, resorting to it especially in the chronic dysentery of the winter season, or when the disease assumes the hæmorrhagic form. In some cases, the chronic morbid condition of the intestinal canal underwent remarkable improvement, by the use of nitrate-of-silver enemata.

The regulation of the *diet* is a matter of importance. This requires, in those soldiers whose powers have become exhausted by fatigue, inclemency of season, want of sleep, or insufficient food, to be of a restorative description, given in small quantities at first, proportioned to their digestive powers. Others, in whom the disease has been induced by excess of eating and drinking, require a severer regimen; but low diet is ill-borne, even in acute cases, for more than two or three days, and its protracted maintenance is the error into which the young practitioner fresh from France usually falls. In the treatment of the chronic dysentery of Algeria, the enjoyment of several months of complete rest, and a nutritious diet, often proves the most efficacious of means, and is a far more rational mode of treatment than either debilitating or stimulating procedures.

MALARIAL FEVERS.

A small library of works on the paludal fevers met with in Algeria, has resulted from the meritorious labours of the military medical officers; and with the satisfactory result of elucidating, in a great degree, their

true nature, and very much diminishing the proportion of deaths among those who become victims of their influence.   Still, M. Haspel regards many of these productions as too hypothetical, and as manifesting too strong a predilection to bend facts to preconceived views and theories.   In consequence of the great differences observed in the fevers at different seasons of the year, he prefers considering them separately at these several epochs.   Before reviewing these divisions, we may extract the following general summary :

" Of all the types of *intermittent* fever, the quotidian, tertian, double tertian, and quartan, are those which are oftenest met with in Algeria ; and, according to M. Maillot, the number of quotidians is to that of tertians as 1582 to 730.   *Remittent fever*, he observes, is that in which the paroxysms are not separated by any interval, and in which the accidents, after becoming mitigated, resume suddenly and periodically their highest degree of intensity.   These periodical aggravations, termed paroxysms, may or may not be preceded by rigors, and followed by sweating.   The *pseudo-continued* fevers are those in which there is neither remission nor interval nor returning paroxysms, and which only reveal their true nature by the sudden occurrence of accidents exclusively peculiar to marsh fevers.   Still, on the other hand, these continued fevers, quite peculiar as they are, require a denomination distinguishing them from ordinary continued or typhoid fevers.   M. Maillot has, moreover, shown how these fevers, though always identical in their nature with marsh intermittents, may degenerate, either by spontaneous evolution, or under the influence of irrational treatment, into the continued, nervous, essential, ataxic, adynamic fevers, which Broussais referred to gastro-enteritis, and which are now designated as typhoid.   He divides these pseudo-continued fevers into two varieties—viz., one in which the fever, at first intermittent, gradually becomes transformed into continued ; and the other, which, continued at first, although intermittent in its original nature, gradually assumes the intermittent form.   M. Casimir Broussais does not admit this species of fever, which is neither continued, intermittent, nor remittent, but, in M. Maillot's words, *pseudo-continued*, and in those of Torti, *spuria continens.*   He believes the remittent character to be always recognisable, even in the worst cases ; but in spite of the numerous cases he has collected in proof of his opinion, we feel obliged to affirm that this character of remission is by no means so striking as he affirms it to be.   It becomes very difficult, or even impossible, to seize it ; and in some cases it is only recognisable by the periodicity of the pains.   Nothing is more common in Algeria than these transformations of type in paludal fevers, especially in certain marshy regions. . . . . . .

" Intermission had long been considered the prototype of marsh poisoning, especially in northern latitudes.   The expeditions into the Morea and Italy, but especially the conquest of Algeria, overthrew this exclusive theory ; and the works of MM. Maillot, Worms, Boudin, &c., have demonstrated beyond contradiction that the most varied types, from the most complete intermission to the most absolute continuousness, may result from the effects of malarial emanations, and be cured by similar procedures.   It is now acknowledged that the fevers described by Hippocrates, the history of which is so different from that of the fevers observed at the present time in Paris, are exactly those which the military practitioners have met with in the Morea and in Algeria.   They are the intermittent, remittent, and pseudo-continued fevers of warm climates, which have also been so well described by Torti, who studied them at Rome." (pp. 157—161.)

As we have said, M. Haspel describes the fevers as they are found to exhibit themselves in the respective seasons.

1. *The Spring, or Vernal Fevers*, are characterized by their mildness, and their distinctly intermittent character.   Usually quotidian in type, they much resemble those met with in temperate climates, and do not become

transformed into pseudo-continued or pernicious fevers. Small doses of quinine serve to cut them short, as well as to relieve the visceral *engorgements* which sometimes complicate them.

2. *Summer Fevers.*—During the heats of June, July, and August, the fevers assume a much more serious aspect. Their paroxysmal character becomes lost, and they may take on the remittent or pseudo-continued type, or even, to appearance, all the characters of advanced typhoid, for which, indeed, during the early part of the occupation of Algeria, they were frequently mistaken. Sometimes they are accompanied by marked gastro-intestinal, or bilious symptoms, so as closely to resemble yellow fever. In fatal cases, the most varied lesions may be observed without any one of these being of constant occurrence; and under certain circumstances, nothing whatever abnormal can be found. In most cases there is abnormal development of the spleen and liver, together with *ramollissement* of these organs,—this latter condition being also met with, in some cases, in the heart and brain.

3. *Autumnal Fevers.*—In the early period of autumn, while the temperature is still high, and the night-dampness excessive, the paludal fevers appear in all their intensity, sometimes at once taking on typhoid or pernicious characters, or becoming complicated with dysentery or hepatic and splenic disease. Amidst the remittent and pseudo-continued fevers, others, of an irregular tertian or quartan type, spring up, re-appearing at different epochs with determined obstinacy, and thus leading to a progressive deterioration of the constitution. At this period of the year, everything is to be avoided that debilitates the economy, and deprives it of its power of resistance. This malignity of the autumnal season is, however, only exhibited in the cases of persons who have had fever at prior portions of the year; for the effect of the miasmatic influence on the healthy and on new comers is no longer observable. Towards the end of October the rains begin, the temperature is lower, and the number of fevers undergoes diminution. Prostration now takes the place of the pseudo-phlogosis of the former period. The remittents are frequently transformed into dangerous tertian and quartan fevers, which become interminable, and are characterized by the prolongation of the cold stage and defective reaction, as well as by the visceral engorgements, the dropsical effusions, and the chronic diarrhœas that so frequently ensue.

4. *Winter Fevers.*—In the winter season the pseudo-continued fevers have almost disappeared. Tertians now predominate; but although they seem to resemble those of spring by the mildness of their paroxysms, they essentially differ from them by their great tendency to relapse, the irregularity of their progress and crises, and by their obstinate resistance to treatment. They are, in fact, usually but relapses of fevers which had resisted treatment in the prior autumn; and frequently they are only finally removed by nature effecting a favourable crisis in the system in the following spring. Though milder in appearance, these fevers are no less dangerous than those of the two former seasons, on account of the serious visceral obstructions to which their prolonged duration and frequent recurrence have given rise.

Pernicious Fevers.—This name is given to those miasmatic fevers, which, by reason of their rapid progress, speedily terminate in death if not

promptly treated. The writings of the ancients contain only a few vague indications respecting this terrible form of disease; and it is in the writings of Mercatus and Morton that we first find it treated of with any precision. The later works of Werlhof, Lautter, Senac, and Torti, have thrown great light upon the subject. That of the last-named writer is, indeed, a remarkable production; for the most recent investigations have added little to the precepts it contains.

Among the most prominent *symptoms* are sudden and complete alteration of features, a cadaverous aspect, extreme prostration, cold sweats, and an irregular, intermittent, depressible pulse. In various cases, some special symptom predominates, impressing upon the disease particular forms, which will be noticed below. It may affect any type, but the tertian and double tertian are the most common. In most cases, the progress of the disease is irregular, its stages being prolonged, and its intermissions imperfect. At its commencement it sometimes puts on all the appearance of a simple, mild intermittent, the severe symptoms coming on from the second to the fifth paroxysm. In some regions of Algeria, so violent is the onset of the disease, that death takes place without any precursory sign, at the second or even the first paroxysm; and in bad cases it is rarely delayed beyond the fifth. The attack is sometimes so sudden, that all may seem to be going on quite well but the day before, when some slight cause, as a change of temperature, or slight fatigue, may at once place the patient in a hopeless state; and even in cases wherein amendment has taken place, the same train of dangerous symptoms may be reproduced by the slightest causes.

There are various *forms* of the affection, characterized by the predominance of certain phenomena. (1.) *Comatose Fever* is the commonest of these, and that to which the greatest number of victims is sacrificed in Algeria. The presence of *coma* is its chief characteristic, and has obtained for it the appellation of *soporosa, apoplectica, carotica*, &c. This varies in degree from simple somnolence to complete coma; and it may come on in the course of simple intermittent, of remittent, or of pseudo-continued fever. So sudden sometimes is the attack, that death speedily takes place without any preliminary symptoms. This, however, is rare; for in most cases such symptoms have existed in the form of pain in the head, lassitude, &c., but from their mild character or insidious appearance, have not excited alarm. After a duration of from twelve to thirty-six hours, if the violence of the cerebral congestion has not proved fatal, reaction takes place, and a partial restoration of the faculties occurs for a time,—the coma again recurring, however, and the patient sinking into a state of adynamic typhoid. Sometimes the reaction is decided, accompanied by profuse sweating, and soon followed by convalescence; but at others, no improvement follows this sweating. The cerebral congestion may be confounded with encephalitis, if the sudden and remittent character of the affection, and the prevalence of pernicious intermittents at the time, be not borne in mind. The diagnosis between this affection and encephalitis, or cerebral hæmorrhage, is, however, often difficult.

(2.) M. Haspel terms that variety of pernicious fever which is especially characterized by violent delirium, *delirious fever*—nervous and irritable subjects being particularly liable to it. It exhibits itself under two varieties,

in one of which there is great determination to the brain, while the other is purely nervous; the distinction between the two being highly important as regards treatment.   The transformation of a delirious into a comatose pernicious fever is of the worst augury; while favourable expectations may be entertained when a delirious succeeds to a pernicious comatose fever.

(3.) The *algid fever* is one of the most alarming diseases that can attack man. It is not the mere prolongation of the cold stage of an intermittent; and the two conditions even present striking contrasts.   In the cold stage, the sensation of cold is out of all proportion to the actual diminution of temperature; while in algid fever, the cold is not perceived by a patient even when his skin is icy to the touch.   This condition may set in during the hot stage of an apparently mild intermittent, or during abundant sweating, which seems to announce returning health.   A subtle poison appears to be preying upon the vitality of the economy, the patient resembling a breathing corpse, respiration being the only function in any activity. The tongue and breath are icy, the voice extinct, while the features assume the utmost impassibility, or only convey the idea of utter exhaustion.   Amidst all this, the patient complains of pain and heat of the abdomen; but the intellectual faculties remain completely intact.   There is no distinct paroxysmal condition observable; the coldness of the surface, as well as the abdominal pains, only acquiring greater intensity at irregular intervals. When the disease does not prove fatal, which it rapidly does if not promptly treated, the return towards health is announced by a gradual increase of temperature, and a recovery of the power of the heart.   In some cases, the reaction extends beyond normal limits, and visceral irritations are produced.

In relation to a case in which no reaction was set up (bleeding having been resorted to during the paroxysm of an apparently mild intermittent, which was, however, succeeded by the algid condition), M. Haspel makes the following remarks upon the *condition of the heart* in this affection :

"Is not this absence of reaction an indication of a radical defect of energy, and of an inability on the part of the heart to struggle advantageously against the morbid cause which seems to concentrate all the fluids in the viscera? M. Maillot long since pointed out the frequency of cardiac lesions in algid fevers; and MM. Antonini and Monnard, still more explicit upon this point, regard as constant characters of this form of pernicious fever a *ramollissement* of the heart, as well as of the other chief parenchymatous viscera, and a gorged condition of the mesenteric vessels.   In the numerous necroscopies which we have had to perform, especially at Bona, we have very frequently met with various lesions of the heart, such as hypertrophy of its cavities, flaccidity or *ramollissement* of its muscular tissue, thickening of its valves, polypoid concretions in its cavities, or concretions in the large vessels.   These lesions, which have also been recorded by M. Kaltner, may indeed be the consecutive effects of repeated attacks of intermittent fever; and, in point of fact, almost all the patients in whom we met with them had suffered from relapses of this disease.   Still, we do not hesitate to say that these lesions, although secondary, ought to be regarded as among the most characteristic and significative facts in the history of algid fever; for, from a knowledge of this frequent occurrence of cardiac lesion, important therapeutical conclusions may be drawn. In fact, if, as necroscopies so frequently show, the heart becomes inert and powerless under the influence of organic lesion, bloodletting can be supported with difficulty, and will deprive the organ of the little energy it still retains, and thus only hasten on the attack of pernicious fever." (p. 292.)

(4.) *Pernicious Choleriform Fevers.*—After a more or less severe attack of shivering, vomiting, and purging, &c., abundant yellow, bilious, or green stools occur. The powers of the patient are speedily prostrated, and the surface becomes icy cold, and of a blue or violaceous colour. The urine is suppressed; burning heat is felt in the belly, and the cramps compel violent cries. If a patient in this condition be not promptly treated, he soon dies asphyxiated. M. Haspel states that he has found free frictions, with croton oil, the most speedy means of inducing reaction.

(5.) *The Cardialgic Pernicious Fever* is not an infrequent form, and is characterized by the intense pain which is referred during the cold stage to the upper orifice of the stomach. It is a sense of tearing or gnawing which calls forth the cries of the patient, and may be accompanied by vomiting, fainting, and prostration of strength,—death often occurring at the second or third paroxysm.

(6.) *Indeterminate Pernicious Fever.*—There are numerous cases which do not admit of being brought under any of the above categories. Several present a confused assemblage of important symptoms, characterized by *malignity*, without any one of these acquiring a special predominance. Their irregularity, due to this malignity, prevents their course being predicted, and gives rise to great embarrassment and error. Various affections serve to mask them, while they open the door for their admission into the economy. This is especially the case with children, who are carried off in great numbers by unsuspected pernicious fever.

Diagnosis of Pernicious Fever.—So varied are the forms, and so insidious the onset, of these fevers, that the most skilful are sometimes at a loss to pronounce upon their true nature, especially as predominant symptoms are sometimes absent, a general malignity alone being present. In cases in which doubt prevails, experience has shown that it is far more dangerous to overlook a pernicious fever, than to mistake and treat a typhoid fever for one. The practitioner may be deceived by the benign character of the early stages; but it is an error to suppose, that these malignant forms of fever set in suddenly, without the occurrence of any premonitory symptoms, the treatment of which would arrest the disease and save many lives. The condition of hypertrophy of the spleen and liver is of little account, as this is found in old simple intermittents and remittents that have been ill-treated, and is often absent in pernicious fever. The diagnosis is especially difficult in *infants;* pernicious fever, as it occurs among them, bearing indeed little resemblance to that of adults. The cold stage is absent or very slight, the hot stage is exaggerated, and the sweating one is abortive. One of the most important characteristics is the irregular paroxysms of increased heat of skin, occurring especially at night, and that prior to all other precursory signs. At intervals the child may seem to recover its spirits completely; and the practitioner is thus too often thrown off his guard. The paroxysms become more and more aggravated; and at last symptoms exhibit themselves, which too surely announce the serious nature of the case.

Prognosis of Pernicious Fevers.—How serious this is, may be judged of by the mortality which takes place among the patients of able practitioners long accustomed to observe the disease. Of 866 such cases treated in the hospitals of Rome, 341 (or 1·2¼) proved fatal. M. Maillot lost 38 in 186,

and of M. Haspel's patients, rather more than 1 in 3 died. The chances of recovery are greater as the intervals separating the paroxysms are longer, more time being thus obtained for the administration of remedies. The prognosis, sufficiently bad at the first paroxysm, becomes infinitely worse at the second, and especially at the third, particularly if the true character of the others has been overlooked. A fatal issue may be looked for when there is constant jactitation, severe suffering, diminution or loss of sensibility, irregular or intermittent pulse, syncope, difficult respiration, coma, &c. Constant vomiting, which prevents the administration of quinine, is of very bad augury. Algidity, as ascertained by the temperature of the skin, is the most alarming symptom, indicating not only a stasis of blood in the capillaries, but a prostration of the vital powers. Imperfect reaction, inducing a slight heat of skin, but without any effectual re-establishment of the circulating power, is often a bad sign; the patient soon sinking again into a worse state than ever. Among the most favourable critical phenomena are abundant sweats, which, persisting during convalescence, usually produce an invigorating rather than an exhausting effect. The disease proceeds with great rapidity, and if left to itself infallibly kills in from the first to the fourth paroxysm—rarely being prolonged to the fifth. Its entire duration occupies from twelve to seventy-two hours in adults, and from twenty-four to forty-eight hours in children—slight remissions, which seem like a halting in the struggle, always occurring.

Pathological Anatomy of Pernicious Fevers.—M. Haspel, after adverting to the numerous observations that have been of late accumulated upon this important point, insists again that we must not overrate their value in elucidating the nature of the disease.

"In almost all the cases, the autopsy, carefully conducted, has only shown us great vascular repletion, inducing redness of different shades of intensity, an increase in the size of organs, and a great injection of membranes. But I repeat, in our view these characters are not essential, absolute conditions of the disease; and they possess, moreover, too little identity, whether as regards their nature, or the portion of the economy they affect, to allow of great importance being attached to them. Almost all the bodies of those who succumbed were minutely examined, none of the cavities escaping our search; and yet it has often happened that we could discover no organic changes whatever. Like lightning, the toxic agent which induces the paroxysms of pernicious fever, destroys and annihilates the vital principle before producing any organic changes. We are thus led to the recognition of the fact, that this fever is produced by a cause which affects the entire organism, the local lesion being but its external manifestation. The latter is therefore not necessary for the production of a paroxysm of pernicious fever. Little does it matter to us whether this cause first exerts its operation upon the blood or upon the nervous system, our object being to demonstrate the universality of its character." (p. 316.)

Among the *post-mortem* appearances noticed, the mucous membrane of the *stomach* was found to present various shades of redness, and very frequently a partial or general *ramollissement.* In the *intestinal canal,* similar lesions were far less frequently observed. In some cases, Brunner's and Peyer's glands were found enlarged; and in others, the large intestine was in a state of great venous congestion. In respect to the *spleen,* the most careful percussion failed to detect its enlargement at the commencement of mild intermittents; and at the autopsies of some of the remittent and pseudo-continued fevers, provided that these had not been of long dura-

tion, it was found quite normal. Frequently, however, it was observed twice its normal size; and this, even after only twenty-four hours, though usually at the end of the second or third day. The not unfrequent absence of all change of size in this organ in pernicious fever, and that even when the brain, lungs, or other organs were in a state of great congestion, is therefore a very remarkable circumstance. Its *consistency* was often found changed, so as to become nearly semi-fluid. In a great number of cases the *liver* was hypertrophied, and the seat of enormous congestion. Its consistency was usually diminished. The *lungs,* also, usually exhibited venous congestion; but at other times they were pale and flaccid. Great changes were observed in the size, colour, or consistence of the *heart,* as already mentioned. In other cases it was normal. The most common appearance found in the *brain,* especially in the comatose and delirious varieties of pernicious fever, was great injection of the pia mater. Blood also was found accumulated at the surface of the brain, or effused into its substance, into the ventricles, and especially at the base. Yet again, it was by no means rare to find the brain wholly devoid of morbid appearances, in cases in which the symptoms quite justified the anticipation of their discovery. *Ramollissement* of the *spinal cord* was observed in some cases, in which the disease was of short duration only. The *kidneys* did not usually offer any lesion; but occasionally they were injected.

Epidemic Putrid, or Scorbutic Fever.—During the wet winters of Algeria, when the occasional appearance of fine days causes great vicissitudes of temperature in short periods of time, the fibre undergoes great relaxation, and the vascular system possesses little reactive power. The fever now observed may offer a confused mixture of typhoid symptoms, accompanied by scorbutic or gangrenous degeneration. The affection is not so remarkable by the numbers it attacks, as by its insidious onset, slow progress, and great severity—the disposition to malignity being very marked. It does not usually attack new comers, but persons who have lived in Algeria fifteen or eighteen months; and in this respect, it is quite opposed to ordinary typhoid fever, which in many of its symptoms it so much resembles. It differs from it also, however, in the entire absence of organic lesions. It especially attacks persons enfeebled by disease, fatigue, or dissipation, and who on any account are retained in bed, deprived of air and exercise. Persons of fresh-coloured appearance and powerful muscular development also become sometimes its victims. The precursory symptoms are usually of the slightest kind. Sometimes a mild intermittent precedes the affection; and at others it declares itself without obvious causes, among those convalescent from other diseases. From the sixth to the tenth day the symptoms assume an adynamic character, combined with more or less disposition to scorbutus; and the disease then continues as a low continued fever, with evening exacerbations. A general disturbance of the entire economy, rather than of any particular organ, is observed; while a horrible fœtor proceeds from the scorbutic gums, and gangrenous ulceration may be set up in various parts, especially the cheek. Life may be prolonged for weeks. The viscera of all who died were found in a normal state, except perhaps being too devoid of colour. The blood was found in a diffluent state; and sanguineous or serous transudations existed in various cavities. M. Haspel regards the disease as exactly similar to the

putrid fever of the low countries, described by Pringle. It is in nowise due to overcrowding, as the wards of the hospital are most empty at this season of the year. The large doses of quinine found suitable in other bad forms of Algerian fever, caused exasperation of the symptoms; so that this remedy had to be employed only like other tonics. By such, with good support, the use of acidulated drinks, and the application of muriatic acid to the diseased parts, some cases were rescued.

Another form of fever met with at this period of the year, exhibits the symptoms of *pernicious typhoid*, with such predominance of cerebral symptoms as to give it a great resemblance to *cerebro-spinal meningitis.* Sometimes the affection is preceded by simple intermittent; and at others the patients are, when seized, in a state of apparent health. The patient may die in this state; but usually, after some days, the cerebral symptoms abate somewhat, and he feels better for three or four days. Pain in the head and vomiting again come on, to be again followed by some improvement; and in this way alternations may go on for a month or longer. It becomes a prolonged marasmus, with imperfect crises. In other cases it assumes a continued form, the patient getting lower and lower, and dying in complete adynamia—no lesion being discovered in the alimentary canal, as in ordinary typhoid fever. In almost all cases, however, some cerebral lesions are met with, such as purulent, serous, or sanguinolent effusions, &c. Quinine, given in large doses, cut short the coma, delirium, or convulsion, observed during the first few days; but its continuance in large doses acted mischievously in respect to the secondary lesions of the brain. As soon as the symptoms denoting these latter became moderated, it was resumed in smaller doses; and occasionally revulsives or small bleedings were resorted to.

TREATMENT OF THE ALGERIAN PALUDAL FEVERS.

This may be embraced under two heads—the primary, or specific, indication of attacking the paludal element by means of the *sulphate of quinine;* and the secondary, or accessory, indication of removing any complications that may have sprung up, by appropriate measures. The *dose* of quinine must vary according to numerous circumstances relating to the course of the disease and the condition of the patient. In simple spring intermittent, when there are usually no remarkable visceral obstructions, M. Haspel gives a single daily dose of from ten to fifteen grains. If uncomplicated, the disease usually disappears after the first or second paroxysm; but if it persists, this dose must be increased. On his first arrival in Algeria, M. Haspel gave the quinine in small repeated doses; but he now feels convinced that by such only a slow and uncertain action results, the violence of the disease being mitigated, but its cure delayed. Under such treatment, simple autumnal fevers acquire great obstinacy; for a repetition of the paroxysm disposes the organism to the reproduction of the fever, while we have no security that what is now mild may not soon become malignant. In this divided mode of giving it, double or triple the quantity is required, that would be necessary if administered with energy at first. If the season is advanced into summer or autumn, if the fever affects the remittent or pseudo-continued type, if it prevails epidemically in an insalubrious locality, and especially if there are symptoms threatening

pernicious fever, we must act more boldly, and give at once from twenty to thirty grains. When the fever is *pernicious,* we should give from thirty to sixty grains, raising the quantity even to seventy-five if these doses prove insufficient; and such doses, in case of imminent danger, may be given for several days in succession. The belief that quinine only exerted beneficial power over the periodicity of the disease, is exploded; it being now amply proved that its efficacy is just as great, whatever form the paludal disease may assume. M. Haspel has never seen any ill effects whatever result from these doses. Although Sydenham's plan of giving it during the interval of the paroxysms is a good one, when applied to the mild fevers of spring, no delay whatever is admissible in the case of remittents, pseudo-continued, or pernicious fevers, notwithstanding any complications that may be present. In such, everything is sometimes so confounded together by the sedative power which weighs down the entire economy, that the quinine must be given at once, as soon as we have detected the serious nature of the disease; or even in fevers of mild appearance, if an epidemic of pernicious fever prevails.

When quinine has been administered for a prolonged period, the stomach often rejects it; and bark, either in powder or extract, given in wine or brandy, may be advantageously substituted. In several patients great epigastric tenderness, anorexia, and an abnormal condition of the tongue, have been considered, erroneously, to contra-indicate its administration; but, in fact, these conditions disappear with the fever, under the influence of the quinine. Advantage is sometimes derived by giving a teaspoonful of syrup of poppies a quarter of an hour prior to taking the quinine, or by adding a few drops of laudanum to the dose itself.

We must not be content with cutting short the attack; for the apparent amelioration will be followed by cruel deception. Smaller divided doses may now be substituted; but the quinine must not be prematurely left off. It is impossible to lay down rules for each case here; but it may be generally stated, that bark in some form should be continued for from twelve to fifteen days after the disappearance of a simple intermittent, twenty to thirty days after summer fever, and two months or longer after obstinate autumn or winter fevers.

We need not pursue M. Haspel's account of the treatment necessary for the removal of the affections which are complications and consequences of Algerian fevers. In respect to other remedies than quinine for the fever itself, he speaks disparagingly. He points to the far greater mortality which prevailed in Algeria when bleeding was practised, and quinine given with a timid hand; and he has no opinion of arsenic as a substitute for cinchona.

SPORADIC AFFECTIONS.

M. Haspel furnishes a few notes with respect to some of these. In speaking of *Diseases of Brain,* he observes:

"One fact well worthy of attention is, that during the heats which usually attend the prevalence of dysentery, hepatitis, and the most fearful fevers that can be witnessed, and in spite of vomiting oftentimes frightfully violent, there is scarcely ever any inflammatory complication on the part of the brain; and yet we might have anticipated the production of encephalitis under the rays of so fierce a sun. In the worst cases, all is limited to a single cephalalgia, and to more or less violent but

transitory congestion, presenting nothing of an inflammatory character. When delirium does occur, it does so only during the evolution of other diseases, as remittent or pernicious fevers; being there, too, an accident, or epiphenomenon, and in nowise a primary condition connected with an affection of the brain itself. We may even say, at no epoch are there so few primary diseases of the brain as during the prevalence of these heats." (p. 417.)

When the transitions of temperature are frequent, *catarrhal* and *rheumatic* affections prevail, but yield readily to emetics, cathartics, and opiates. As already remarked by Hippocrates, pleurisies and other acute membranous inflammations are rare in malarial regions. *Pneumonia* is not manifested in the distinct form observed in other climates. The symptoms are less decided, and more fugitive; and if the active bleeding employed in Europe were resorted to, it would prove fatal. In rare cases it may be employed; and where there is continued pain, leeches and blisters are resorted to; but antimony is the main remedy. *Phthisis* is very rare in North Africa. M. Haspel only had 3 cases at Oran among 1480 patients; and but 1 death in 138 arose from this disease. M. Jourdain met with but 13 cases in 8485 patients, and but 10 deaths from phthisis in 871 deaths. M. Casimir Broussais also had but 8 cases in 116 patients, while in the military hospitals of Paris he had 1 in 41. Out of 41 deaths he had in Algeria but 2 (1 in 20) from phthisis, while in Paris he met with 1 in 5 from this cause. In most of the few cases in which the disease does occur in Algeria, the patients have shown themselves proof against the malarious influence; thus so far confirming the belief held by many, of the *antagonism* which prevails between phthisis and marsh diseases. M. Haspel believes in the truth of this doctrine, although he considers it is not yet completely demonstrated; and adds, as corroborative of it, that phthisis is more prevalent in the mountainous regions of Algeria.

" Not only does phthisis progress so slowly here as to allow nature to organize her means of defence, and consequently of cure,—but also, by modifying the constitution, she causes it to lose the aptitude to tubercle. And, in fact, nothing is more rare than to meet with phthisis in acclimatized Europeans. Every winter or spring-time brings young soldiers to our military hospitals, who, more or less exhausted by endemic diseases, remain a long time in these establishments, where they very frequently contract pneumonia and bronchitis, without ever, so to say, leaving their beds, so feeble are their powers of reaction. Well, these chest affections may become prolonged for months, and pass into the chronic state, if they have not even commenced in the latent form; death may result from them, but rarely indeed will tuberculization be found. Never, I may affirm, during the several years my attention has been turned to this point,—never has a pneumonia, originating under my eyes, thus degenerated. I do not mean to say that such a thing has never happened, but I only assert its extreme rarity, as contrasted with what is seen in Europe; and I conclude that there is in Algeria a kind of antipathy between its climate and the generation of tubercle of the lungs. Hence the advantage which would result to phthisical patients from their inhabiting this country, whether as a means of eradicating the disease, or of moderating its symptoms, and thus enabling them to reach the fatal boundary less painfully and less rapidly, or even of effecting their cure." (p. 427.)

Although the symptoms known as those of *gastro-enterite* are of so common an occurrence during the whole course of the diseases met with in Algeria, *post-mortem* examination exhibits no proof whatever of such an affection having been present. Indeed, nothing is more common than to

find, while the lower intestine is in the most diseased condition, the small intestine and the stomach continue quite normal. Simple *typhoid fever*, with its characteristic lesions, is rare in Algeria, except in the case of young soldiers recently arrived from France. It is probable that the so-called typhoid fevers of various hot climates are, in fact, but examples of those forms of malarious fevers which, in their symptomatology, so much resemble true typhoid fever.

M. Haspel narrates a great number of cases as illustrative of his descriptions of the various diseases.

Art. VI.

Om Sverges Endemiska Sjukdomar. Af Dr. Magnus Huss.—*Stockholm,* 1852.

On the Endemic Diseases of Sweden. By Dr. Magnus Huss.—*Stockholm,* 1852. 8vo, pp. 131.

The modest, unpretending appearance of the works that issue from the Swedish press, forms a marked contrast with the medical essays that are now published in other countries, arrayed in all the glories of the binder's and the printer's art. It may be, that, conscious of the intrinsic value of their productions, the Swedish bibliopolists care little for external show, or good workmen are perhaps scarce in those northern lands; but it is perfectly evident that Swedish authors and booksellers do not rely on hot-pressed paper, or beautiful type, and richly-gilded binding, for the sale of the volumes they publish. Already, however, an improvement is manifesting itself in this respect; the work before us is better "got-up" than many others that we have seen ; and if the type be not quite perfect, nor the paper unexceptionable, there are, at all events, remarkably few errors of the press. Almost all the valuable essays and reports of our friend Dr. Huss have been issued in this almost pamphlet form ; and we have, in course of time, become reconciled to the modest aspect of his volumes, for we know that under this unpretending exterior we shall discover a rich harvest of carefully recorded facts and observations. The volume before us, though of a different character from any of his preceding publications, will sustain the justly-acquired reputation of the author. A carefully digested statistical account of the diseases of Sweden, is an easier task in that thinly-peopled land, than in the over-populated island that we inhabit. With a population not twice as large as that of the metropolis of England, Sweden has the advantage of having been foremost among the countries of Europe in statistical details ; and, aided by returns from the various medical district officers, Dr. Huss has succeeded in giving us a very satisfactory sketch of the diseases that prevail over a country extending from north latitude 69° to 55° south, or from beyond the Arctic circle to the parallel of the northern counties of England. In a land of such extent, we necessarily find great differences in climate, and corresponding varieties in the food, the clothing, and the social condition of the people. The western and northern parts of Sweden are mountainous, the hills rising gradually from the flat shores of the Baltic on the east, till they reach their greatest altitude on the ridge that separates Sweden from Norway. The southern

part of Sweden is, on the contrary, comparatively level, and, like the northern, is covered with huge pine-forests and morasses; but it is, on the other hand, more densely populated; and, as might be expected, it enjoys a milder climate.

The labour of compiling the present essay was entailed on our author by a promise which he made at the meeting of Scandinavian naturalists at Copenhagen, in 1847, to prepare a statistical account of the diseases of his native land. Beginning at the extreme north, he has collected details and reports upon each of the Swedish provinces and districts, the results of which are printed in the first eighty pages of this volume; and appended to this is an essay of great value on the prevailing diseases of the country, on the causes that produce them, and the remedies that are urgently called for to stay their progress. To the general reader, this latter portion will undoubtedly prove the most interesting part, and we shall consequently be very brief in our analysis of the statistical reports contained in the first eighty pages.

All the travellers who have visited the northern parts of Sweden and Norway, have noticed the prevalence of conjunctivitis and snow-blindness among the Lapp population of these wild regions. "To have sore eyes like a Lapp" is a common saying in Sweden; and Dr. Huss tells us that hardly one of the Lapps escapes from the consequences of living throughout the winter in close huts, surrounded with a dense atmosphere of smoke, and of exposure to the glare of the sun on the trackless snow-fields in spring. During the winter, the only artificial light used by the Lapp is obtained from resinous pine-branches, which, of course, add much to the density of the acrid smoke with which their huts are constantly filled. In summer, the same smoke is imperatively called for to drive off the swarms of mosquitoes and gnats, which are there scarcely less troublesome than in tropical regions. The inhabitants of the coast of the Baltic, at the head of the Gulf of Bothnia, from Tornea to Pitea, are grievously infested with tænia-lata, or tape-worm. Dr. Wretholm states, that in Haparanda, the suburb of Tornea, there is scarcely a family, whether rich or poor, of which some of the members are not afflicted with this disease. He has observed it in children at the breast, who had never imbibed anything but their mother's milk. From the coast the disorder spreads inland, following the course of the larger rivers; and it is ever most prevalent where large surfaces of water are found. Tænia solium occurs but rarely. The disease affects chiefly young people, and especially females. Dr. Waldenström ascribes its frequency to peculiarities of diet; the people live chiefly on fish and milk, while the mountain Lapps, whose diet is almost entirely of animal food, never suffer from tape-worm. Still, there must exist some other causes yet not ascertained; for the wealthier merchants, who live as others of their class do in southern lands, are yet as liable to the disorder as the poor, whose only diet is milk, unleavened bread, and salt fish. Dr. Wretholm observes:

"As the result of the attention I have paid to this subject for several years, I may observe that the prevalence of tape-worm is in a direct ratio to the nature of the soil and of the water used for drinking. In the higher tracts of land, where the drinking water is derived from springs or from the mountain brooks, tænia is hardly known; but as soon as we descend to lower ground, and especially to the banks of

the larger lakes and rivers, where the houses are often built on the former beds of the lakes, and where the water, coming from morasses and bogs, is impure, and derived from wells sunk in the soil, and filled with extractive matter rapidly passing into decay, there tape-worm prevails among persons of both sexes, and in every condition of life." (p. 5.)

Following the western coast of the gulf, we come next to the district of Western Bothnia, where the tape-worm becomes less frequent, so that in the southern part of this district it can scarcely be said to exist as an endemic disorder. Its place is occupied by rheumatic disorders and their complications, which especially prevail during the months of August and September. At this time of the year, the labouring classes are employed in gathering-in hay for winter fodder for their cattle, upon the large flat mossy tracts where grass grows in great luxuriance. In the evenings and mornings of these months, cold fogs hang over these districts, till dispelled by the warmth of the sun; but the morasses themselves are so wet, that the labourers often stand the whole day mid-leg deep in water. From time immemorial the people of this country have employed a rude form of moxa, as a counter-irritant in rheumatic disorders. A piece of amadou or dried fungus from the birch-tree is laid on the affected part, lighted, and allowed to burn till an eschar is produced. Dr. Hallström saw an old man of eighty years of age in Lycksele, whose whole body was so marked by these eschars, that no space of whole skin remained larger than the palm of the hand.

As we advance further towards the south, we meet in Angermannland, latitude 62° 64′, two forms of endemic disease, which, though not unknown in the extreme north, yet are to be found there only in occasional and isolated cases. These disorders are scrofula and intermittent fever. Most writers on ague tell us, that intermittent fever is rarely to be met with beyond the fifty-sixth degree of north latitude, or, in other words, that within the Scandinavian peninsula this scourge is unknown, for the extreme point of Southern Sweden is in latitude 55° 30′. And we are further told, that it requires for its development a continuous temperature higher than 60° of Fahrenheit's thermometer. Here, however, according to Dr. Akerblom of Solleftea, we learn, that from time immemorial ague has been endemic in two places near the mouth of the great Angermann river—namely, about the saw-mills of Lo, and the lading-place of Nyland. Here the sea and fresh waters of the river intermingle, the surrounding country is low and marshy, and fogs are very frequent in the early part of the year. The ague takes almost invariably the tertian form; it shows itself generally in March, and continues to appear till the end of June. In general, the disorder has been confined to these two spots, which lie opposite to each other, with the river between them; but in the year 1838, the malady spread along the shores of the Angermann river, as far as forty or fifty miles from the sea, and attacked three per cent. of the population. Scrofulous disorders are, according to Dr. Akerblom, a new importation, and he observes that they have greatly augmented since the disappearance of the more inflammatory types of disease. Dr. Akerblom believes, too, that scrofula may be propagated by vaccination (?), for he has often observed it to affect healthy children after they had been vaccinated with matter taken from a scrofulous infant. The suggestion is worthy of attention, but the

most scrofulous children often have an exceedingly healthy aspect during the first year of their existence. Two other disorders, which were formerly endemic in this district, are now fast disappearing, though in Norway and other countries they are still regarded as scourges of the most fearful character. These are the radesyge and the spetälska, both of which have been described in former numbers of this journal and its predecessors. Their gradual disappearance is here, as elsewhere, to be ascribed to the improvements recently effected in the social condition of the people, to the establishment of hospitals, and to the more judicious treatment of the disease. From 1780 to 1800, almost every case received into the hospital at Hernösand, was one of radesyge. Spetälska (elephantiasis) is ascribed by the people to peculiarities of diet, and especially is thought to arise from living upon the spawned salmon (gra lax), which are caught in immense numbers when descending the rivers in autumn. The malady is certainly not contagious; a husband is frequently affected by it for many years, while his wife remains in perfect health.

Of the high inland district of Jemtland, a most favourable report is given; endemic diseases are almost unknown, though rheumatism certainly exists, and young females during the haymaking season are often affected with erysipelatous swellings of the legs, the result of standing for days together in the wet marshes, whence the hay is brought. It is a proverb in Sweden, that Jemtland is a bad locality for our profession:—" Jemtland ei har nagot läkareklimat." Strangers settling in this part of the country sooner or later become affected with herpes on the hands and face.

On the banks of the great Ljusna river, in Helsingland (lat. 61°, 62°), a stream liable to frequent and sudden floods, spetälska, or elephantiasis, is still endemic, and Dr. Wikblad reports forty-two cases from this district. It generally assumes the tubercular form, as represented in Danielsen's and Boeck's work (plate 2); and Dr. Wikblad has found all remedies unavailing, excepting creosote, both inwardly and as an external application. It evidently is connected, more or less, with marshy exhalations, for it occurs almost exclusively in those who dwell on the banks of the river, where numerous shallow pools are left by the floods.

In Gestrikland (lat. 60°, 61°), ague becomes endemic and frequent; further north, as we have seen, it is confined to one or two particular spots. Here it occurs along the coast of the Baltic, but at the distance of ten or twelve miles from the sea its influence is no longer felt. Tænia lata is, again, common here, as in Lappmark, and the reporting physicians ascribe its prevalence to the great consumption of salmon, a fish much infested with tænia. Is, then, the tænia of the salmon identical with that found in the human species?

Goitre and scrofulous diseases are often met with; indeed, it is about Gefle that scrofula first assumes the character of an endemic disease, becoming more and more prevalent as we advance farther towards the south. Goitre, however, is chiefly prevalent in the districts called the Dales, around Fahlun, where we find narrow valleys surrounded by steep mountains; and yet, in many parts of Norway, where the valleys are less broad and the mountains much more precipitous, goitre is unknown. Dr. Huss suggests that the acid vapours, with which the air is constantly filled from the copper-works, may act injuriously on the health of this district. We

are not aware that any magnesian limestone occurs here, but the Dales are the only part of Sweden where goitre is endemic. We are, ourselves, inclined to regard goitre as more influenced by locality than by the character of the water that is drank; though the observations of Mr. Macpherson in India, and of Dr. Inglis in this country, would favour the opposite opinion. It is certain that goitre is neither confined to the magnesian limestone districts, nor is it to be found in all parts thereof; thus, in the county of Durham, we have seen but few cases of goitre, yet the magnesian limestone occupies a large district of its eastern part.

The only spot in Sweden where dysentery occurs as an endemic disorder, is on the flat shores of the great Siljan lake (lat. 61°). It appears regularly every year, in August and September, when the nights have become cool, and thick fogs rise from the marshy tracts around the lake. The malady, at times, assumes a severe character: thus, in August and September, 1839, out of a population of 138,000, there died of dysentery 2043 individuals. Of this number, 146 were under one year, 676 between one and five years, 442 between five and ten years, 347 between ten and twenty, and 432 above twenty years of age. The disorder is evidently fraught with peculiar dangers to youth and infancy, for more than one half of the deaths occurred in children below ten years of age. Much of this mortality is perhaps to be ascribed to the difficulty of procuring timely medical aid, for the people often delay to apply to the district physicians, in the hopes of effecting a cure by popular remedies. Of these, one of the most favourite is pulverized brick or tile, which is administered in spoonfuls to the patient, and, as Dr. Wettergren justly surmises, can only tend to aggravate the malady. The same reporter has likewise observed a remarkable coincidence between abundant harvests and the prevalence of this disease. If the harvest has been indifferent, and corn consequently scarce and dear, but few cases of dysentery occur in the succeeding year; but if a plentiful harvest has afforded a good supply of meal for the ensuing summer, the disorder prevails with great severity.

"It is difficult," observes Dr. Huss, "to account for this; it is perhaps the only instance we know of, where dysentery is produced by abundance of food. It is possible that the rye-meal, when consumed in large quantities during August and September, may undergo a species of fermentation-process in the intestines, and thus give rise to dysenteric affections. Much larger quantities of meal are consumed in good years than in seasons of scarcity. Still, after all, the abuse of spirituous liquors (corn brandy) may be the principal cause of this difference. In years of scarcity, hardly any brandy is consumed by the peasantry; in seasons of plenty, every cottage contains a superfluity of the deleterious drink." (p. 25.)

Dysentery is, however, not the only disorder that is peculiar to this part of Sweden. A good many of the inhabitants of the Dales, especially in the neighbourhood of Orsa, are occupied during the winter in quarrying and dressing grind-stones; they are constantly exposed to the dust formed by the finer particles of the stones, and while working in the ill-ventilated and confined quarries, their food is of the worst possible description. The malady appears closely to resemble in character the needle-grinder's asthma of this country: the finer particles irritate the mucous membrane of the air-passages, and produce, sooner or later, chronic inflammation of the bronchial lining membrane. According to the report of the pastor Sernander, of Orsa, the boys who begin to work in the stone-quarries at the age of

twelve to fifteen, and some begin even earlier, soon become affected with this disorder; so that by the time they have reached their eighteenth year, they are often unfitted for further labour, and rarely reach the age of thirty-five. Those who begin work at a later period of life are less rapidly affected, and may reach the age of forty-five, or even fifty-five; but rarely are their lives prolonged beyond the latter period. During the period of the year that the quarries are worked—viz., from the first of October to the middle of November,—the food of the quarrymen consists of salt herrings and hard bread; brandy is consumed by them in large quantities, and the water they drink is strongly impregnated with lime. After the blocks of stone are raised from the quarries, the so-called hewing or dressing process is carried on during the winter months, in small and ill-ventilated chambers, constructed for the purpose. The fine powder raised by the tools of the workmen floats in these chambers like a mist; the windows are kept constantly closed to exclude the severe cold, and in such an atmosphere they pass the greater part of the twenty-four hours, from December till April.

The disease commences at variable periods, according to the constitution or idiosyncrasy of the individual affected; sometimes, hardly twelve months elapse before the unmistakable symptoms show themselves. The patient begins to complain of a sense of weight in the chest; the breathing is short and sibilous, and the voice harsh and rough; but there is little or no cough. The aspect of the patient is soon materially changed; the skin becomes of a pale, dusky hue, even though previously the colour was fresh and red; the adipose tissue diminishes, and emaciation comes on. Should the patient now relinquish his deleterious occupation, he may yet regain his health; but in many persons, even change of work will now afford no permanent relief. Should he continue to labour in the quarries, other symptoms begin sooner or later to show themselves; the oppression of breathing increases, especially at night, so that the patient cannot lie down in bed; there is occasional cough, but rarely any copious expectoration, though the bronchial passages, from the sounds that are audible therein, obviously contain large quantities of frothy mucus. The feet and legs now begin to swell, ascites follows, and death generally occurs from effusion into the thoracic cavity, and often in a manner so sudden, that the patient seems all at once to be suffocated.

As scrofulous complaints are by no means rare in this part of Sweden, it is possible that many of those who are cut off so early in the stone-quarries perish really from tubercular phthisis, the outbreak of the disease being accelerated by the irritation of the bronchial mucous membrane. The population of the district is about 4000; and out of seventy-seven deaths, fourteen have arisen from bronchial disease contracted at the quarries.

The districts of Sweden around Upsala and Stockholm do not appear to present any particular endemic diseases. We turn, therefore, to the westward, to the borders of the great Wener lake, on whose northern shores croup is peculiarly prevalent, and produces a frightful degree of mortality among children. The proportion of boys that are affected is much greater than that of girls. Scrofulous children do not suffer more than healthy ones, and the offspring of the poor are less frequently attacked

than the children of the rich. It has been generally thought that croup was not in any way a contagious disorder; but both the reporting physicians from the district now referred to, regard the disease as communicable from one child to another. When they first commenced practice in the neighbourhood, they both were non-contagionists, and therefore neglected the ordinary precautions against the transmission of disease; but experience soon showed them that croup was often communicated to a healthy child, if it was allowed to remain with one already affected with the malady. The average number of cases has greatly diminished, since a system of strict separation has been adopted. Dr. Segerstedt gives a melancholy example of the neglect of these precautions. A physician who had recently settled in the country, denied the possibility of croup being communicated by contagion. To prove this, he placed his own son, a boy of three years old, in the same bed with a child affected with croup; but on the following day, the previously healthy child was affected with the disease, and died in spite of all the efforts of the distressed father, who himself, horrified at the unlooked-for results of his experiment, soon followed his son to the grave. Dr. Huss observes, that an isolated case of this kind would by no means prove the contagious nature of croup; but the facts accumulated by the medical reporters go far to confirm their assertion, that croup, in this district at least, may be communicated from one child to another. The disorder is here so prevalent, that, according to Dr. Ekegrén, 75 per cent. of the children of the district are attacked with it; the disease prevailing generally from the end of November to the middle of May. It occurs almost always during the prevalence of northerly or easterly winds, and especially during snow-storms from those quarters; and he has remarked that the children in the houses that are well sheltered from the northern and eastern blasts, escape comparatively unharmed. The district in which croup especially prevails is a low-lying tract of ground around the town of Amal, where the wood that covers the face of the country has been entirely cleared away, and which is consequently exposed to the full force of the winter north-east gales. The disorder frequently breaks out after the rooms in which the children live have been scoured in winter, or when linen in a damp state has been brought from a cold room, and hung up within the heated chamber to dry.

The district of Bohuslän, to the north of Gotheborg, was formerly much affected with spetälska; but since the deep-sea fisheries on this coast have been relinquished, the disease has almost entirely disappeared. Possibly an improvement has taken place in the social condition of the people; but the other scourge of radesyge still remains. Both these maladies seem to be confined to districts upon the coast, as in Norway; and it has been observed that they prevail along the shores of the deep inlets or fiords which run far up into the country. For the characters of these two remarkable disorders, and for full details of their symptoms, we refer our readers to the works of Drs. Danielsen and Boeck, and of Dr. Hjort, of Christiania. Dr. Kjerrulf, of Uddevalla, is disposed to regard a diet of stale fish as one of the principal causes of these disorders. The fishermen on the coast are notoriously poor, and uncleanly in their habits; they rarely taste bread or milk, which forms a considerable portion of the food of the inland population; and their clothing is very insufficient for the severe labours they

have to undergo on this inclement shore. There is one circumstance regarding this disease mentioned by Dr. Kjerrulf, which we do not remember to have seen previously mentioned. He informs us that radesyge can only once attack the same individual. While the disease exists, the patient is never affected with phthisis, nor is he ever attacked with ague : a child labouring under the malady escapes the perils of scarlatina and measles; and, lastly, the humpbacked never become victims of radesyge.

An interesting report has been forwarded to Dr. Huss from Dr. Horlin, of Marstrand, a small island and fortress lying about four miles from the shore, a little to the north of the mouth of the Gotha river. The island is, in fact, little more than a barren rock, there being only a thin layer of vegetable earth in the immediate vicinity of the town. The climate is considerably milder than that of the mainland, and to this, and to its insular situation, is to be ascribed the remarkable immunity from tubercular disease enjoyed by the inhabitants. Marstrand, in fact, appears to be the Madeira of Sweden. Phthisical patients from the mainland rapidly improve upon this desolate rock ; and even when tubercular disease is far advanced, and cavities have been formed in the lungs, the condition of the sufferers is often greatly altered for the better by a residence on this island. Its beneficial influence is, however, chiefly confined to the summer months ; a more southern climate must be sought by the consumptive in winter. In Westergothland, besides rheumatism and ague, the majority of the population are affected with scabies combined with eczema. The malady, however, is confined chiefly to those who live on the low, cleared lands ; the inhabitants of the woodland tracts and forests are scarcely affected by it. Medical aid is seldom sought to effect a cure ; indeed, this is regarded as not desirable by the poor, who associate with the itch an idea of household prosperity ! Perhaps it was here, or among a similar people, that the notorious Hahnemann gathered his most original doctrines regarding scabies, and its influence on the human system. The cause of its prevalence among the inhabitants of the low lands, undoubtedly lies in the filthy habits and uncleanliness of the people, as compared with the forest population. In the fortress of Karlsborg, there prevailed, some years ago, a remarkable amount of night-blindness (nyctamblyopia) among the soldiers of the garrison. It occurred chiefly in summer ; and out of a corps of 300 men, 60 were often affected at one and the same time with this singular disorder. As soon as twilight set in, the sight began to fail ; and it was not uncommon to see a troop of soldiers led back to barracks, totally unable to find their way by themselves, and stumbling and falling over the smallest obstacles in their path. The pupils of the eyes were generally dilated at these times, and were not affected by artificial light. It occurred in men of all ages, constitutions, and habits of life, and appeared sometimes to be connected with dyspepsia or the rheumatic diathesis,—at least, it yielded generally to remedies directed against these. Every effort was made to discover if the disease was not simulated, for it was confined solely to the corps of pioneers, and did not extend to the other soldiers of the garrison. Dr. Marin used every means to discover the cause of this remarkable affection, but was not more successful than in the case of the Prussian garrison at Ehrenbreitstein, where, in 1834, not less than 138 soldiers were affected with night-blindness. The disorder

first showed itself at Carlsborg in 1830, or thereabouts; and since 1842, has gradually decreased, so that in 1850, Dr. Collin reports only two cases.

Oster Gothland is, in general, free from endemic disease; the land is well cultivated, and supports a hardy and prosperous population.   In one district or parish, however, purulent ophthalmia has prevailed for the last forty years.   It bears the character of the true Egyptian ophthalmia, and is readily propagated by the contact of the purulent secretion from the conjunctiva.   Its origin has been accurately traced to two soldiers who returned to the parish of Wänga from the campaign in Germany of 1814. The uncleanly habits of the people tended greatly to propagate the dis· order, and their distance from adequate medical aid rendered it hardly possible to obtain assistance at the commencement of the disease, when it is in general cured without difficulty.   Dr. Hansen, the district physician, instructed an intelligent female of this parish in the usual mode of treatment, and her efforts were soon crowned with the greatest success. The sulphate of copper, either solid or in solution, was the chief remedy applied, and since that time the disease has gradually been on the decline.

In the district of Smaland, children are constantly infested with the ascaris lumbricoides, while tape-worm but seldom occurs.   Children of all ages, and of all conditions of life, are equally subject to these tormenting parasites; and females often suffer from them during their whole lives.

In the district of Halland, lupus of a severe character has long prevailed, along with other less serious skin-diseases.   This is one of the poorest districts of Sweden; and here, consequently, uncleanliness is seen in its highest degree.   Ascarides are likewise common here; and strangers, who have never before been affected by these parasites, soon complain of their presence when they have settled in the country.

In the southern part of Sweden, in the district of Scania, scrofula is observed to a greater extent than in any other part of the Scandinavian peninsula.   The disease does not present any peculiar varieties, nor is it in general of severe character, and yields easily to treatment, especially when combined with cleanliness, good ventilation of the dwelling-houses, and nourishing food.   Nor does it appear to be so often combined with, or followed by, tubercular disease of the lungs, as elsewhere.   The causes of the prevalence of scrofula in this low-lying, but rich and well-cultivated district, are well depicted in the following report by Dr. Boström, of Christianstadt:

" The causes of the constant prevalence of scrofula in this town (Christianstadt), and among the poor of the surrounding country, are to be sought for, first, in the uncleanly habits of the people, with the breathing of contaminated air in their dwelling-houses, and scanty and indigestible food.   In no part of Sweden are the habits of the poor so uncleanly as in Scania.   Nor are the better classes free from this reproach.   If their outward clothing be clean, the garments worn next the skin are black with filth.   No one ever thinks of bathing or washing.

" The garments next the skin usually consist of thick leather drawers, which are never changed till they are in shreds; they are worn night and day, and emit a peculiar odour, by which, even at a distance, the presence of the Scanian peasant may be known.   Children are never bathed, and the consequence is, that they become frightened at water, and if the doctor prescribes a bath, the mother listens with horror, and omits to fulfil the order, making the excuse that the child nearly

fell into fits when about to be put into the water. Thus the little creatures soon acquire an absolute terror of water. The dwelling-houses are close and damp, without any ventilation; the floor is often merely of earth trodden down to a level, and if there be, indeed, a wooden floor, it is concealed by a thick layer of dirt. Warmth is kept up by iron stoves filled with turf, but these do not assist the ventilation, as they are generally placed outside of the dwelling-rooms, so that, when extinguished, the temperature is rapidly lowered. Children suffer much from these rapid alternations of temperature, especially as they are hardly permitted to go out into the open air in winter; while the catarrhs and inflammation of the conjunctiva that ensue, are all ascribed to some draught of pure cold air that has insinuated itself into the carefully-closed rooms. Nor is the children's diet of a suitable kind. Directly after meals, they are stuffed with coffee, sour rye-bread, and above all, with potatoes!" (p. 69.)

In the district of Bleking, around Carlshamm and Carlscrona, children are liable to another form of scrofulous disease, the "meningite tuberculeuse" of the French, and the mis-named hydrocephalus acutus of English writers.

The disorder appears, from the reports, to be extremely frequent and fatal in the particular district above named, and this is ascribed to the peculiar position of the town, exposed as it is to the sharp winter winds, from which most of the other towns on this coast are sheltered by the Skärgard, or reef of high rocks and islands, which skirts the coasts of Sweden and Norway.

The island of Gottland, the last upon which we have a report, appears to be, on the whole, remarkably healthy; but in autumn many cases of so-called nervous fever occur, and from the description given, they bear a close analogy to our milder forms of typhus.

We have now brought to a close our brief analysis of the first part of Dr. Huss's volume, and before leaving it, we must express our deep obligation to the distinguished compiler, for the care with which the reports have evidently been examined and compared. Even in so small a population as that of Sweden, the task of Dr. Huss has been no easy one, and required a master mind for accomplishing a duty so arduous. We do not know if any special report has of late years been published on the diseases of other countries, but this work of Dr. Huss is an example that we trust will soon be followed by others. The remaining fifty pages of this volume are devoted to an examination of the causes and treatment of the more prominent maladies of Sweden; some of these being common to the whole country, while others are confined to peculiar localities. The disorders here treated of are, ague, scrofula, cardialgia (magsyra), and chlorosis; and finally, a supplementary chapter is added on the most serious disorder of all, the unlimited indulgence in ardent spirits in Sweden.

Ague.—The northern limit of ague is fixed by Dr. Huss in latitude 60°, in Sweden; though, as before stated, a few spots are found further north by two degrees, where the malady is endemic. Over the whole of Sweden that lies to the south of this line of 60° N. latitude, ague prevails more or less. It occurs chiefly in spring, is less frequent in autumn, and is but rarely observed during the summer months. Cold and damp are, in Sweden, as elsewhere, the principal causes of its prevalence. In Sweden, as in the rest of Europe, it was remarked that intermittent fevers dis-

appeared for several years, after the great epidemic of cholera, from their usual habitats. This was remarked, not only in places that had been visited by cholera, but likewise in localities where that disease had never appeared. After the decline of the cholera, intermittent fevers again made their appearance, but it is observed that they rarely now show the malignant character they before assumed.

Scrofula.—Scrofula appears first in Angermanland, as we pass from north to south, and its extreme northern limit in Sweden is therefore the sixty-third degree of north latitude. The characters of scrofula do not differ there, from those that it presents in other countries. In the towns it is seen under all its diverse aspects; in country districts it shows itself by swellings in the glands of the neck, but in certain localities the eyes are the parts chiefly affected. Out of the towns, scrofula is less frequently connected with, or followed by, tubercular disease of the lungs, than in England, or in Germany or France.

Scrofula is said to have become more frequent in Sweden since the introduction of the potato as an article of food; and a remarkable confirmation of this opinion is afforded by Count Wachtmeister, who states, as the result of his observation, that during the prevalence of the potato disease, when the labouring classes of Sweden were reduced to great distress from the want of their usual food, the children, though they looked pinched with hunger, had lost the characteristic pale, flabby, scrofulous appearance, and though much emaciated, were certainly more lively, and, if anything, more healthy, than heretofore.

" There can be no doubt," observes Dr. Huss, " that our climate is, to a certain extent, responsible for the prevalence of scrofula, for children are in this country carefully kept within doors during the intense cold of winter, and are thus deprived of that fresh air which is so indispensable for their healthy development. The malady, therefore, most frequently makes its appearance in winter and in spring; and the longer the bad weather continues, the more likely is it to be developed. I consider the want of ventilation of the apartments, the want of due exercise, and a diet consisting chiefly of potatoes, to be the chief cause of scrofula.* Hereditary predisposition is another great cause, along with the habit of spirit-drinking in the parents. Indeed, nothing is more injurious than the custom of giving to a crying and impatient infant a piece of linen or cloth dipped in brandy to suck. The child is thereby temporally pacified, but as the remedy must often be repeated, the advantage is but too frequently gained at the expense of health." (p. 89.)

There must, however, be other causes of scrofula, with which we are as yet unacquainted. In the north of Sweden, where the climate is severe in the extreme, the malady appears to be scarcely known; but, on the other hand, it is possible that there is less spirit-drinking, and less perhaps of the exclusively potato diet, in those districts, and that the people are a more hardy and more temperate race. It cannot, we think, be denied, that scrofula appears to spread with the increase of civilization; but we agree with Dr. Huss, that this must be ascribed to the vices that follow in the train of civilization, and to the evils consequent on the association together of men in large numbers.

Pyrosis, Cardialgia.—Dr. Huss has under this head classed together many

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* We are not aware that scrofula is peculiarly prevalent in Ireland, where the diet of the peasantry is almost exclusively potatoes; but then, from the greater mildness of the climate, children are not shut up within doors all the winter.

various complaints, including many of the Protean forms of dyspepsia, because, as he observes, pain at the epigastrium is one of the most general symptoms, but it is accompanied with many others, as waterbrash, flatulent distension, acidity, &c. &c. From the southern extremity of Sweden to the extreme north, pyrosis (for so we shall indicate the disease) is absolutely endemic, affecting all, but principally the lower classes, both in town and country. The female servants in the towns are particularly subject to the malady, and the female sex in general suffers much from it; but both in the towns and in the country, it attacks the male sex too, and this in a greater degree than is witnessed in more southern lands. Patients who have once suffered from pyrosis are exceedingly liable to relapses of the disease, especially in summer and in autumn. The malady is generally obedient to proper remedies, and rarely assumes a dangerous character; indeed, it is surprising how long it will endure without passing into ulceration, or without the supervention of cancerous disease in the stomach. Such untoward terminations are most frequently observed in the towns and cities. The malady has prevailed in Sweden from time immemorial, but has greatly increased within the last fifty or eighty years.

The causes of this disorder are undoubtedly to be found in the social condition, habits, and food of the Swedish people. Although in various provinces of this extensive territory considerable differences are observed among the people in this respect, still they present everywhere certain customs and certain peculiarities of food, &c., which all seem to conduce to the same end.

"As a general rule," observes Dr. Huss, "the stomachs of the Swedish peasantry are overloaded with meal and potatoes; they seldom eat flesh meat of any kind, the only variety being salt herrings and other small fish, with abundance of milk, which, however, is rarely taken fresh, while their general drink is a preparation of milk that has undergone a partial acetous fermentation.* The peasant of Scania fills his stomach many times in the day with sour rye-bread, the Dalecarlian prefers enormous quantities of groats and oatmeal; in Halland, and in a great part of West Gothland and Smaland, the people live chiefly on potatoes with sour milk, in which are boiled the acid berries of the whortle, or bilberry; in Wermland, oatmeal and potatoes are the staple food; in Norrland the poor live on salt herrings, with oat or barley bread, potatoes, and sour milk. It is only as an exception that the Swedish peasant ever tastes flesh meat, and this is usually bacon or dried mutton. To these causes of indigestion and acidity must be added two others, one for either sex—namely, spirit-drinking, and the inordinate consumption of coffee. The consumption of brandy, which has of late years been so much increased by the removal of restrictions upon the distillation of this spirit, is now general in Sweden, and bids fair within a short time to destroy both the mental and bodily powers of the people, while it is well known to be an active agent in producing those symptoms which we have classed together under the name of pyrosis. One of the most esteemed physicians of Sweden thus expresses himself on this subject: 'Ere long the Swedish people will acknowledge no God but the brandy-flask.'" (p. 93.)

As an instance of the enormous consumption of brandy, Dr. Huss states, that in the town of Norberg, not less than 24,000 " cans"† of brandy are

* We presume this to be the old Scandinavian drink " bland," which is still used in Iceland, Färoe, and Shetland; and, from experience, we can assert it to be an agreeable, if it be not a wholesome beverage.

† The Swedish " kanne" weighs 48083·72 English troy grains, and is equal to 189·9413 English cubic inches.—Duncan.

retailed, and two-thirds of this are consumed by 300 quarrymen, so that each labourer may be assumed to drink 60 " cans" in the year.

Coffee has only become an article of general use within the last few years, but has undoubtedly contributed its part to the great spread of pyrosis and dyspeptic affections, especially in the female sex, who are as yet comparatively free from the vice of spirit-drinking. The introduction of coffee has been much favoured by the Swedish government, in the hopes of its superseding the use of brandy; but as the latter is almost confined to the male sex, there can be little hope of the desirable object being thus attained.

Chlorosis (*Bleksot*) has only become endemic in Sweden within the last twenty or twenty-five years; at least, previous to this time it was confined to the better classes, and to the poorer inhabitants of the towns. Now, however, chlorosis seems to be spread throughout the length and breadth of the land; it extends to the north as far as the sixty-fourth degree of north latitude, and seems to be gradually advancing, and ever upon the increase. It is remarkable how unanimous all the medical reports are in stating, that chlorosis has only shown itself in the respective districts within the last ten or twenty years, and that before that time the disorder was almost unknown in the country districts. Dr. Backström, of Wrigstad, reports that the malady has appeared in his district within the last ten years, and has now become so general that it has excited the fears of the peasants, who, seeing that it is a new disease, " believe it to be the precursor of the day of judgment, for that before that day the race of men shall become more and more deteriorated."

The symptoms of the malady do not differ from those that characterize it in other countries. It attacks chiefly young females from the fifteenth to the twenty-fifth year, and is rarely observed after the thirtieth year of age. The unmarried are more frequently affected than the married, but the latter do not by any means escape, especially if they have married when very young, or if they have suffered from the disease in early youth. Occasionally, youths from fifteen to twenty exhibit all the characteristic symptoms.

The following remarks of Dr. Huss, on the prevalence of this malady in Sweden at the present day, are full of pregnant meaning:

" As this disorder indicates great debility in the individuals affected with it, so when we find it to prevail endemically over large tracts of country, as in the greater part of the Swedish territory, we may justly infer that the race of inhabitants there are fast degenerating in bodily strength and health. It may be urged that this malady affects only the female sex, and consequently but one-half of our population. But it will naturally follow, that a chlorotic and debilitated female will give birth to weakly children; and besides this, we have documents to prove, too truly, that the rising male population of our country is fast deteriorating in strength. The inquiry into the causes of this lamentable decay is of the highest interest, and with that view I would divide the Swedish nation into two classes, the labouring population and the others. The first question that occurs is, whether the labouring classes of this country have undergone such marked changes in their social condition, or in their diet, as to account for the appearance amongst them of a malady hitherto unknown. The increase of poverty has been suggested as a cause, but the poor have only increased in the towns; their number has remained nearly stationary in country districts, while it is precisely in the latter that chlorosis has so greatly increased; and, as we have learned from the reports, and by inquiry,

the malady prevails quite as much among the children of the richer peasants as among the cottagers and farm-labourers.

"A great alteration has taken place of late in the mode of life of the females inhabiting country districts. The women formerly took part with the men in field labour; they lived much in the open air, and from their earliest years were inured to work requiring corporeal exertion, and thus they became hardy and strong to a remarkable degree. Of late years, field labour has been relinquished, and the women now are occupied within doors, and as a natural consequence the children, too, are kept at home. In those districts where women are still engaged in out-door work, chlorosis is still rare.

"The abandoning of the ancient Swedish style of dress, so well fitted for the severe climate, may be regarded as another cause. The clothing worn at the present day is much thinner than formerly, and the peasant-girl tries by force to produce a narrow waist like that of the town-bred lady. Chlorosis is yet uncommon in those districts where the old fashion of dress is maintained." (p. 105.)

The abuse of coffee has before been referred to as one of the frequent causes of cardialgia, and Dr. Huss is fully convinced that it likewise tends to produce chlorosis, by weakening the digestive powers.

But the excessive use, or rather abuse, of spirituous liquors, appears to Dr. Huss one of the most important and active causes of this gradual decay of the Swedish race. It is well known that parents given to intoxication will beget scrofulous and weakly children; and that such children are cross and peevish is well known to mothers. In England, the fatal Godfrey's cordial and other narcotic preparations are employed to still their cries ; in Sweden the panacea is brandy—a remedy still more likely to be administered when the parents are in the habit of resorting to it as a solace for their own evils.

We have heard much, of late years, from travellers such as Mr. Laing and others, of the general want of chastity among the female peasantry of Sweden. We know not how far this accusation is founded upon truth, for Mr. Laing can hardly be regarded as an impartial writer, so strongly is he prepossessed against Sweden in favour of the sister kingdom of Norway; but Dr. Huss likewise throws out hints, at page 106, which would seem to confirm this report.—The practice of bloodletting, at certain times of the year, yet exists among the Swedish peasantry, as it did among our forefathers, and it must be acknowledged to be highly injurious, especially in the case of young people who have not attained their full growth.

Turning to the male population, Dr. Huss satisfactorily proves, from the reports of the militia enlistments, that the Swedish youths have most materially degenerated, of late years, both in height and in corporeal strength. General debility, and being under the required height of five feet five inches, Swedish measure, are both sufficient reasons for exemption from service in the militia (*bevaringsmanskapet*). Exemptions from general debility have, within the last ten years, increased $1\frac{1}{4}$ per cent.; exemptions for being under height, not less than $2\frac{1}{2}$ per cent. The number of exemptions for general debility, as compared with the total of exemptions from other causes, has increased by $6\frac{1}{5}$ per cent.; and for being under height, $9\frac{1}{8}$ per cent. Truly, if these figures be correct, it must be acknowledged that the boasted strength and power of the Scandinavian peasant is fast disappearing. Of the causes that conspire to debilitate the Swedish peasant, none, however, is so fatal and so general as intemperance. We are bad enough,

God knows, in England, in this respect, and still worse in Scotland, and, till recently, in Ireland; but the Swede seems to bear the palm among all other nations for devotion to spirituous liquors. Gradually, but surely, has this fatal habit extended itself over that fine country; the hardy peasant is degenerating into a drunken sot; the very character of the nation seems in danger of undergoing a change. In the extreme north of Sweden the evil has not yet taken root, though the Lapp, about Haparanda, is devoted to brandy, and regards the fiery spirit as his chief and only good.

Gustavus Adolphus had already foreseen the danger to his country from unlimited access to ardent spirits, and he forbade the making of brandy by private distillation, under the severest penalties. Under the luxurious reign of Gustavus the Third, the spirit trade was greatly facilitated by the establishment of crown distilleries.

It is a remarkable fact, that Gustavus the Third commenced his reign by altogether forbidding private distillation. He subsequently established the crown distilleries, and ended by not only permitting private stills, but by forwarding their establishment by all the means in his power.

The population of Sweden consists of about three millions ; the amount of brandy annually distilled in the country amounts to between thirty and forty million of " cans," all of which is consumed in the country itself, being a proportion of ten to thirteen cans yearly to each individual. Subtract from the population the women and children, there remains half a million of adult males, to each of which must be allotted from twenty to twenty-six cans of ardent spirit. The consequences of this fearful indulgence in a deleterious compound are manifold; the Swedish peasantry, as has been before shown, are degenerating in corporeal strength, are becoming stunted in their growth, and deteriorated in their morals. In 1780, while Finland was yet a part of the Swedish kingdom, only six million cans of brandy were distilled in the whole country, and now, when the population has comparatively but little increased, the enormous quantity of from thirty to forty million cans is made and consumed by the people. Poverty, sickness, and premature death, are the well-known consequences of this fatal indulgence. It was said, long ago, by the great Linnæus : " If I were to attempt to enumerate all the diseases that ensue from the immoderate use of brandy, my catalogue would be almost endless." The examples adduced by Dr. Huss of the influence of intemperance upon the relative mortality of the male and female population, are numerous and well chosen ; it is needless to repeat them here in detail ; they serve, however, fully to confirm the general report of the medical men of Sweden, that spirituous liquors not only weaken and deteriorate a race both morally and physically, but that their abuse tends most materially to shorten the term of human existence. The increase of crime, of suicide, and of insanity, is likewise touched upon by our author with a masterly hand; but these are matters that do not come so completely within the province of this journal. We have seldom read a more striking picture of the evils entailed on a land by intemperance, and the sketch is the more valuable, as it is from the pen of one high in the estimation of his professional brethren and of the Swedish public. Founded as it is on well-ascertained facts, we would strongly recommend the translation of this part of the present volume to some of our temperance journals, in the hope that it may serve

to check an evil but too prevalent in this country, and which seems to be even on the increase.   Dr. Huss deserves the thanks of the public for endeavouring to stem the tide of intemperance in his own country, and of the profession at large, for the masterly report here presented of the diseases of Sweden.

Art. VII.

On the Diseases of the Kidney, their Pathology, Diagnosis, and Treatment; with an Introductory Chapter on the Anatomy and Physiology of the Kidney. By George Johnson, M.D. Lond., F.R.C.P., Assistant Physician to King's College Hospital.—*London,* 1852.   8vo, pp. 517.

Dr. Johnson has from time to time, during the last six years, communicated to the profession several able papers on the diseases of the kidney.   In 1846, he presented to the Royal Medical and Chirurgical Society a paper on the Minute Anatomy and Pathology of Bright's Disease of the Kidney; the Transactions of the same Society for 1847, contain a contribution from Dr. Johnson, on the Inflammatory Diseases of the Kidney; and in 1850, the Society published a third paper by the same author, On the Proximate Cause of Albuminous Urine and Dropsy; and On the Pathology of the Renal Bloodvessels in Bright's Disease.   The article, Ren, in Dr. Todd's ' Cyclopædia of Anatomy and Physiology' is by Dr. Johnson; it appeared in 1848; and in the early part of the same year, being appointed to deliver the Gulstonian Lectures at the Royal College of Physicians, Dr. Johnson chose, for his subject, the pathology and diagnosis of renal diseases.   In the work before us, he has presented to the profession what may be considered as his matured opinions on this subject.—We lately* gave an analysis of the most recent and able work on ' Diseases of the Kidney,' published on the Continent, and then adverted, *en passant,* to some of Dr. Johnson's views; we propose now to give a full and connected statement of that physician's opinions on the class of diseases in question; and, at the same time, to offer such remarks on those opinions as they seem to us imperatively to demand.   Dr. Johnson's views are not those of a tyro; and if allowed to pass unquestioned, they will be regarded as the expression of the opinions of British pathologists.   In proposing to remodel *in toto* our views of the pathology of renal diseases, Dr. Johnson undertook no mean task; and his facts and his theories must be sifted and weighed, with all the care required by the importance of the object at which he aims.

Dr. Johnson divides his work into eleven chapters.   The first forty pages of the First chapter are occupied with an account of the anatomy of the kidney.   Having so recently discussed this subject, we may pass it by; reserving the few remarks we have to make on it to a subsequent part of our analysis.   The remaining twenty pages of this chapter are devoted to a consideration of the urine, its sources, and physical and chemical characters, and the function of the kidney considered as a whole, and of its special parts.

Dr. Johnson, resting on the observations of Drs. Prout, Golding Bird, and Routh, adopts as the standard of the specific gravity of healthy urine

1021; the quantity passed in twenty-four hours, he judges from Dr. Prout's estimate, to be from thirty ounces in the summer to forty ounces in the winter. For determining the amount of solids contained in a given quantity of urine, Dr. Johnson avails himself of the table calculated by Dr. Golding Bird from Dr. Christison's formula.

Nine pages are given to a description of the chemical characters and composition of the urine. Drs. Bence Jones, Becquerel, Prout, Garrod, Liebig, and Golding Bird, are the chief authorities, whose results are given. With reference to the physiological action of the kidneys, Dr. Johnson holds the doctrine now generally received—namely, that the office of the kidneys is to separate, and not to form, the constituents of the urine; and he adopts Mr. Bowman's opinion in full, that " while the convoluted tubes, with their lining of glandular epithelium, are the agents by which the solid constituents of the urine (the urea, uric acid, &c.) are secreted, the watery portion of the secretion is chiefly discharged through the Malpighian bodies."

Healthy urine contains no renal epithelium-scales, nor any even of their *débris.* To account for this, Dr. Johnson remarks, that

" It is probable that the gland-cells of the kidney are in a continual state of transition, that the old cells are liquefied and pass away with the urine, while new cells take their place, and these, in their turn, pass off in a liquid form." (p. 58.)

We need scarcely observe that this is a pure hypothesis.

This, Dr. Johnson's first chapter, consists of sixty pages; it contains, with one or two exceptions, a fair summary of the generally received opinions on the subjects of which it treats.

The Second chapter is devoted to a consideration of the *Causes of Renal Disease.*

Excluding from consideration cancerous disease of the kidney, and those affections which have their origin in purely local causes, as calculi, impediments to the escape of the urine from the pelvis of the kidney, injuries inflicted from without, &c., Dr. Johnson says—

" It will be found that all the causes of renal disease have this common feature—that *they tend to produce a morbid condition of the blood,* either by introducing some poison from without, or by interfering with the elimination of certain noxious matters developed within the body, by so far reducing the quantity of nutritive food that it is insufficient for supplying the waste of the tissues, and for enabling the blood to maintain its healthy composition, or, lastly, by some exhausting and depressing agency which lowers the vital energy, and diminishes the power which the healthy body possesses of resisting and overcoming injurious influences, whether originating within or without." (pp. 61, 62.)

We may remark, that this is true of diseases of the kidney, so far as it is true of diseases of the lungs, liver, brain, or any other organ, but no further. Dr. Johnson's opinion on this subject, however, will be better understood, if we pass in review the particular causes of diseases of the kidneys he enumerates, and the explanation he offers of their *modus operandi.*

The first of the particular causes of renal disease he mentions, is exposure to cold and wet. When the body is suddenly exposed to these agents, the cutaneous excretion is diminished; in consequence of which, Dr. Johnson says, the elements of that excretion must either accumulate in the blood, or be eliminated by some other channel. And this is true,

Dr. Johnson remarks, of the constituents of all the secretions. When an organ is double, and the secretion from one is suppressed, the second may perform the function that in health was performed by both. But even here, Dr. Johnson says, the demand on the second organ cannot be suddenly made, " without endangering, and often destroying, the life of the patient." These cases clearly differ from those in which " an excretory organ is called on to eliminate from the blood some materials which do not form a part of its own proper secretion." The result of exposure to cold and wet is to throw, according to Dr. Johnson, the constituents of the cutaneous excretion on to the kidney; and the effort to eliminate these constituents, by the cells of the latter organ, leads to disease of those cells.

Disease of the heart and lungs is given by Dr. Johnson as the second of the particular causes of disease of the kidneys. With reference to the influence of a mechanical impediment to the return of blood from the kidneys, in producing disease, our author remarks, that the structural changes which the kidney undergoes in these cases " can no more be explained by a mere mechanical impediment to the circulation, than the products of a spinning or a weaving machine can be accounted for by a reference solely to the action of the piston which sets the machinery in motion." As a consequence of the disease of the heart and lungs, the respiratory function is imperfectly performed, and the blood becomes diseased, " first, by containing less than the natural quantity of oxygen; and, secondly, by containing an excess of carbonic acid, and, probably, of other excrementitious matters." And he adds, that the disease of the blood under these circumstances is very imperfectly expressed, when we say of it, that it contains an excess of carbonic acid, &c.; for " it is *very probable* it may undergo other changes as a consequence of the retained carbonic acid, and that these secondary changes may have produced the renal disease."

On the whole, it seems to us that Dr. Johnson attaches too little importance to the direct influence of a mechanical impediment to the return of blood from the kidneys; at the same time that he shows, pretty clearly, that others have placed too high a value on it as a direct causative agent in the production of renal disease. Dr. Johnson observes, that the secondary influences of the retained constituents of the cutaneous secretion on the blood, should also be taken into account in estimating their effects on the kidneys. In calling attention strongly to this subject, our author has, we believe, rendered a service to renal pathology.

When the functions of the liver are impaired, Dr. Johnson affirms that bile is secreted by the renal epithelium; and proof of this is afforded, he thinks, by the fact, that the renal cells, in cases of jaundice, are seen to be deeply tinged with bile. For our part, we had always accounted for this by supposing that the fluid part of the urine, like the fluid in the pericardium and peritoneum, held some of the colouring matter of the bile in solution, and that this solution of biliary colouring matter entered the renal epithelium by imbibition. It has seemed to us that the cells are merely dyed in the same way as the other tissues of the body—the bones, fibrous membranes, &c. Dr. Johnson, however, thinks that the renal cells " strive (*sic*) to eliminate" the bile from the system.

We now come to one of the most important of the particular causes of disease in the kidneys—viz., the fever poisons, and especially the poison of

scarlatina.   Dr. Johnson regards the opinion of those who maintain that the poison of scarlet fever exerts a necessary influence on the kidney, as erroneous.*   "Under favourable circumstances," he says, " the poison is eliminated entirely by the skin, and it is only when its natural course has been interfered with by some disturbing influence, that it is diverted into other channels."   As to the period of the disease at which the poison acts, and its *modus operandi* in inducing the kidney affection, Dr. Johnson remarks :

" The renal disease may come on at any stage of the fever, and its greater frequency during the period of convalescence, may probably be explained by the fact, that at that period the patient is more frequently exposed to cold, by which the process of desquamation and of elimination from the skin is checked, and the poison being thus driven inwards, is excreted, as we shall hereafter see, by a desquamative process in the kidneys." (p. 70.)

In these sentences are contained several hypotheses.   The first, the basis of the others, is, that a specific poison having once entered the system, has to be excreted.   Dr. Johnson has, so far as we can discover, no evidence to offer in support of this opinion, and it is quite as probable that the poison is decomposed in the system, and resolved into other compounds, as that it is excreted in its original form ; at any rate, there is no adequate evidence that desquamation of the cutaneous or any other epithelium, is a process for the elimination of the poison of scarlatina or of any other specific fever.

If, then, this be an hypothesis, and it can claim no higher position, it is most illogical to build on it other hypotheses ; to talk of cold checking the elimination of, and driving inwards, a poison, the existence of which, in the form presumed, is hypothetical, and the elimination of which by the skin is yet more hypothetical.   But our author does not stop here.   He affirms that the disease of the kidneys following on the exposure to cold, is due to an effort of the cells of the kidneys to eliminate the hypothetical poison, hypothetically checked in its hypothetical elimination by the skin, and hypothetically driven inwards.

Dr. Johnson admits the fact, however, that persons suffer from disease of the kidneys, after scarlet fever, who have never been exposed to cold ; and he has the following apposite remarks on the subject :

" As every error has its opposite, so it is in the management of scarlatina, and, indeed, of all febrile diseases.   While some attendants appear to imagine that patients cannot have too much air, there are others who, acting upon the notion that warmth is essential, raise the temperature by carefully closed doors and windows, large fires, and heaps of bedclothes.   Next to the opposite plan of early exposure to cold, I believe that this irrational mode of treatment is most likely to excite disease of the kidneys in the course of scarlatina ; and that it is a very frequent cause of the disease amongst the poor, who live in small, dirty, and ill-ventilated rooms." (pp. 71, 72.)

Next in importance to temperature and ventilation, is the influence of diet and medicine, in either producing or preventing disease of the kidneys during the progress of scarlet fever.   Alcoholic stimulants act, Dr. Johnson states, by increasing the febrile disturbance, and diminishing nearly all the secretions, particularly that of the skin ; the only secretion which is usually increased by them, being that of the kidneys ;—

* See British and Foreign Medico-Chirurgical Review, vol. vii., p. 505.

" So that the natural process of elimination by the skin is checked, at the same time that the diuretic action of the alcohol tends to divert the poison to the kidneys, and there to excite disease. . . . . .

" What has been said (our author adds) of the diuretic action of alcohol, is equally applicable to all diuretic medicines ; the tendency of which is to stimulate the kidneys, and so to excite disease in them by directing the poison towards them ; whereas it is of the utmost importance to promote the action of the skin and bowels, and to permit the kidneys, as much as possible, to rest until the fever poison has been entirely eliminated." (pp. 72, 73.)

Food in large quantities, or of an indigestible nature, is mentioned by Dr. Johnson as another source of renal disease during the convalescence from scarlet fever.   Still,

" In a certain number of cases of scarlatina, renal disease will occur, when it cannot be attributed to any of the disturbing influences which have been mentioned ; but these cases are rare, and particularly so, if we exclude those in which there has been some previous disease or cachexia." (p. 73.)

Measles, erysipelas, typhus, and rheumatic fever, are all enumerated as occasional causes of renal disease. During the progress of chronic diseases, also, the sudden occurrence of renal disease is not an unfrequent accident. The Cholera-poison, according to Dr. Johnson, excites in the kidneys the same kind of disease as is produced by the poison of scarlet fever.   The cholera-poison, however, differs from that of scarlatina in being very quickly eliminated.

Disease of the kidney, with albuminous urine, is very common in purpura, and some of the general affections of the skin attended with eruption on its surface ; Dr. Johnson, in these cases, regards the renal disease as a secondary affection.

The intemperate use of alcoholic drinks, especially when combined with a deficiency of nutritive food, is to be classed among the most frequent causes of renal disease.   In these cases, not only are the general nutritive powers impaired, but " the blood is contaminated by a mixture of alcohol and imperfectly-digested food."

Another fruitful source of renal disease is, Dr. Johnson observes, the poison of Gout.   The renal disease thus produced differs in no respect from that unconnected with gout.   Dyspepsia, Dr. Johnson says, deserves mention among the probable causes of disease of kidney.   " We know," he adds, " that the products of imperfectly digested food are often eliminated by the kidneys in the form of lithate of ammonia"—soda we believe is the more usual base ; but this and other lithates which are found in normal urine, our author thinks pass through the kidney without exciting disease. In Scrofula, the kidney suffers, in common with other organs, from the general morbid condition of the blood.   The diseases so induced are among the most unmanageable of renal affections.

After having thus considered the causes of renal diseases, Dr. Johnson devotes a chapter to each of those affections of the kidney which he considers to be pathologically distinct.   These affections, or distinct and definite diseases of the kidney, are—*Desquamative Nephritis, acute and chronic; Waxy Degeneration of the Kidney; Non-Desquamative Disease of the Kidney; Fatty Degeneration of the Kidney; Suppurative Nephritis;*

Tubercular or Scrofulous Disease of the Kidney; and *Cancer of the Kidney.* The subject of *Hæmaturia* is considered by itself in the concluding chapter.

Acute Desquamative Nephritis.—This is a name framed by Dr. Johnson to express a form of acute renal disease, the type of which is afforded by that affection of the kidneys which occurs in the course of, or immediately after, an attack of scarlet fever; to it belong all those cases which, a few years since, were known as acute inflammatory dropsy, and the common exciting cause of which is exposure to cold and wet. The cases collected together by Dr. Johnson, under this head, would be regarded by Frerichs as examples of the first stage of Bright's disease. We shall follow Dr. Johnson through his account of acute desquamative nephritis.

An attack of acute desquamative nephritis is ushered in by more or less of rigors and chilliness, followed by feverish reaction, pain in the back and limbs, headache, and restlessness. In most instances, dropsy is a very early symptom. The swelling commences in the face, and extends soon to every part of the subcutaneous cellular tissue; effusion frequently occurs into the serous cavities. The urine is scanty, loaded with albumen, and dark-coloured from admixture with blood. The specific gravity of the urine varies. There is usually more or less pain and tenderness in the loins, and frequent desire to pass urine. Nausea and vomiting after eating are of common occurrence. In some cases, inflammation of one or more serous membranes supervenes; or œdema or inflammation of the lungs or bronchial tubes; or severe headache, followed by convulsions, coma, and death.

One of the earliest signs of amendment is an increase in the quantity of the urine, and a diminution in the amount of albumen it contains.

"In many instances, the pallor of the skin and lips, which usually appears simultaneously with the occurrence of the dropsy, remains for a considerable time after the disease has entirely ceased, showing the destructive influence which has been exerted upon the colouring matter of the blood." (p. 88.)

The sediment from the urine, in the earlier stages of the disease, is seen, on microscopical examination, to be composed of coagulated fibrin, blood-corpuscles, cells having for the most part the characters of renal epithelium, and occasionally crystals of uric acid. Some of the fibrin is in irregularly-shaped masses; but some has the form of solid cylindrical moulds, of an average diameter of about $\frac{1}{700}$ in., in which are entangled blood-corpuscles and epithelial cells. "Besides," Dr. Johnson says, "the blood-corpuscles and the epithelial cells entangled in the casts, there are many of these bodies scattered about the field." Some casts are composed entirely of blood, and some, of larger size, have a wax-like appearance, and contain no epithelium-scales.

After the disease has continued for two or three weeks, a variable number of oil-globules is seen in the interior of some of the renal epithelial cells. This is less common in children than in adults.

The deposit of uric acid usually occurs after the extreme congestion of the kidney has been relieved, and when the urinary secretion is becoming copious.

"In this abundant deposit of uric acid, the urine of acute nephritis differs remarkably from that of some other forms of disease, and particularly from that of fatty degeneration." (p. 93.)

Morbid Anatomy.—Both kidneys are usually diseased in an equal degree. Their size and weight are increased; the capsule peels off readily; their surface is smooth, at places pale, and at others vascular. The relative proportion of the congested and exsanguine portions varies much in different cases. On the surface are some round and irregularly-shaped crimson spots, due to hemorrhage into the convoluted tubes. The line of demarcation between the cortical and the medullary portions is well marked. In adults and young persons, the consistence of the glands is rather firmer than natural; in aged persons they are often soft and yielding.

"The medullary cones are usually of a dark colour, from venous congestion; the bodies of the cones appear compressed by the swollen portions of the cortical substance which passes between them, while the bases are expanded and spread out into the cortical portions, thus, as suggested by Rayer, having the form of a wheat-sheaf." (p. 95.)

The mucous membrane of the pelvis of the kidney, of the ureter, and bladder, is generally abnormally vascular.

Besides the lesions of the urinary organs, there is more or less serosity in the areolar tissue generally, and frequently, also, in the serous cavities; the special products of inflammation—lymph and pus—are also often present in the latter. The liver is sometimes enlarged, as if the seat of recent interstitial deposit; the lungs gorged with frothy serosity, and often " more or less extensively carnified, or hepatized." No lesions are, in a vast majority of cases, even when cerebral symptoms have been present, to be found within the cranium.

Changes in the Blood.—We shall pass by these, because Dr. Johnson merely quotes the well-known observations of Christison.

Necroscopic appearances in the Kidneys.—The disease chiefly affects the cortical portion of the gland.

"Most of these tubes are found to be unnaturally opaque, in consequence of being filled by epithelial cells, which have been formed within them, and thrown into their cavity. The tubes are crowded with these cells in different degrees; some being fully distended, while in others there is little evidence of the desquamative process having occurred; there being only a single layer of epithelium on their walls, and this, either differing little from the normal appearance, or, perhaps, being unnaturally opaque and granular in texture. The most crowded tubes will usually be found in those parts which, to the naked eye, appear pale and anæmic." (pp. 98, 99.)

Numerous epithelial cells are scattered over the field when a portion of the kidney has been scraped or torn with needles, and " frequently, in the examination of the tubes, a portion of their contents being squeezed out, presents exactly the appearance of the epithelial casts which have been described as existing in the urine." Coagula of blood may be found in some of the tubes, and these coagula may be either bright red, or more or less disintegrated and granular, and of a yellowish or brownish hue.

With reference to the epithelial lining of the tubes of the medullary cones, Dr. Johnson says that it

"Is generally found to present a natural appearance, or to be only so far changed, as would be accounted for by the passage through the tubes of an abnormal and irritating secretion." (p. 100.)

Dr. Johnson fails, however, to tell us what the physical changes of the

epithelium-scales of these tubes are, which, he says, may be accounted for by the passage through the tubes of an abnormal and irritating secretion.

The *Malpighian bodies* are, at first, in a state of simple engorgement, and appear to the naked eye like minute bright red grains.   Subsequently they are distinguished, by the unassisted eye, from the surrounding tissues, by having a lighter colour and a less opaque appearance.   Under the microscope—

"The capillaries, at the first view, seem to be bloodless, and to have small cells on their surface; but on a careful examination these are found to be blood-corpuscles, modified in appearance by being seen through capillary walls, which have been rendered thick and opaque by the transudation through them of an albuminous, or fibrinous effusion.   The surface of the capillaries often appears rough and very finely granular, as if from the coagulation upon them of the materials which have escaped through their walls, but I have rarely seen any organized effusion upon them, or within the Malpighian capsule.   The corpuscles usually form a single row in the canal of the capillary; they often present a dot in the centre, and they differ from the ordinary appearance of the blood-corpuscles chiefly in being somewhat larger and of a lighter colour." (pp. 101, 102.)

In a note to this passage, Dr. Johnson says:

"The addition of acetic acid to a healthy Malpighian body, produces the same change in the appearance of the vessels and the blood-corpuscles, as that which results from acute congestion and the consequent transudation of serum through the coats of the vessel." (p. 102.)

Dr. Johnson gives a drawing of a Malpighian body, with the altered blood-discs in the interior of its vessels.   There are one or two points about this description and figure, which puzzle us a good deal.   We cannot comprehend why the blood-discs are not dissolved, or at least reduced to such a state as to lose their definite outline, by the action of the acetic acid. Again, what is the dot in the centre of these so-called blood-discs?   No alteration in the coats of the vessels could produce such an appearance in blood-discs in their interior.   Again, in the figure, these altered blood-discs are represented of an oval form; while human blood-discs are circular. And finally, we find no mention made by Dr. Johnson of the nuclei of the capillaries of the Malpighian glomeruli; nor do we see, in the figure annexed, more than one kind of body represented on or in the vessels. Had not Dr. Johnson told us that these bodies were blood-corpuscles, "modified in appearance," we should have supposed his description to have applied to, and his figure to have represented, the nuclei seen on the walls of the normal capillaries of the Malpighian bodies.   We should have supposed that the walls of the vessels, having been thickened by disease, had obscured the nuclei; and then, that the acetic acid, by removing the opacity of the former, had brought out the latter; and that, in the case of the healthy kidney, the acetic acid, by dissolving the blood-discs, had made the outline of the nuclei manifest.   We own that we think this matter wants putting in a little clearer light.

Can Dr. Johnson have fallen into the error he charges on Rokitansky, Henle, Frerichs, and others, of having had "an insufficient regard to the normal structure of the kidney"?   If Dr. Johnson had not said thus much of some of the first European anatomists, we should have hesitated to ask the question regarding himself.

All the vessels of the kidney are, Dr. Johnson says, pervious.

Pathology of the Disease.—Dr. Johnson informs us that he adopts the following proposition as a central truth, in respect of diseases of the kidney:

"*All changes of structure commence in the secreting cells of the gland, and are the result of an effort made by the cells to eliminate from the blood some abnormal products—some materials which do not naturally enter into the composition of the renal secretion.*" (p. 105.)

And in illustration of this proposition he remarks,—1st. That the cells of different glands have characteristic appearances; 2ndly. That the cells of certain glands have the power of separating from the blood materials which do not form a part of their normal secretion; 3rdly. That in some cases these materials exert no injurious modification on the secreting cells; 4thly. That in some cases, where these foreign materials are secreted for a length of time, they may produce very decided changes of structure: and in proof of the latter, Dr. Johnson says, that in two cases of diabetes, he found the cells opaque; but then, in one of these two cases, the urine was albuminous, and contained granular casts of tubes;—he adds, " we have another illustration of the effect upon the secreting cells of an effort to eliminate new materials, in cases of jaundice." To this hypothesis we have already adverted.

In many cases, the evidence that there is some *materies morbi* which excites the renal disease is derived — 1st. From a consideration of the circumstances under which the disease occurs; all the cases of renal disease having as a common feature, Dr. Johnson affirms, that they tend to produce a morbid condition of the blood.

" 2ndly. We gain additional evidence upon the point in question from analogy [analogy, by the way, is very poor evidence]. We know that during the process of normal secretion, there is no visible shedding of secreting cells; but we have seen [i.e., in the two cases of diabetes, and in jaundice] that certain materials, foreign to the renal secretion, while they are passing through the gland-cells, so modify the process of secretion and cell-growth, as to give rise to a desquamation of cells from the renal tubes. And this observation [? hypothesis] *will suffice to show the probability,* that in other cases a like process of desquamation results from a similar cause—viz., an elimination of some morbid material by the renal cells. When, therefore, a patient has been exposed to one or more of the influences alluded to in the preceding chapter [i.e. the chapter on Causes], as tending to produce a morbid condition of the blood, and when the urine presents the signs of the desquamative process occurring in the kidneys, there can be little doubt that this desquamation is the result of an effort to eliminate some of the abnormal materials, which have either been introduced into, or developed within the blood." (p. 110).

And again:

" It is assumed, that the blood being in a morbid state, whether from the poison of scarlatina, or from any other of the various causes alluded to in the preceding chapter, an effort is made to eliminate the noxious matters by the secreting cells of the kidney. The cells, in striving to separate the strange materials, become modified in their action and nutrition, and being rapidly thrown off into the tubes, are thence removed by the current of liquid, and appear in an entire form in the urine." (pp. 110, 111.)

So that the only real evidence Dr. Johnson adduces of his "great central truth," the key-stone of the arch he has erected, are two cases of diabetes, the before-referred-to cases of yellow renal cells in jaundice, a little reasoning from analogy, and an assumption or two. Having dis-

covered this great central truth with reference to renal disease by a process of what?—not certainly induction,—he proceeds to reason downwards from it; to deduce, that is to say, the particular from the general.

In order to account for the appearance of albumen in the urine, and the general dropsy, Dr. Johnson, after remarking, that as a consequence of the loss of the renal cells and engorgement of the tubes with their accumulated contents, the process of secretion is " greatly retarded, if not entirely arrested," advances the following hypothesis.   Dr. John Reid, he says, showed " that when the blood in the systemic circulation becomes decidedly venous, and consequently unfit for carrying on the process of nutrition, it passes less freely through the capillaries into the veins."   Supported, then, by analogy, our author assumes that " the renal circulation is affected by an imperfect elimination of the urinary constituents in a manner analogous to that in which the pulmonary circulation is influenced by the retention of carbonic acid in the blood ;" and this assumption being granted, it follows, he says, that the retardation of the circulation must first occur in the inter-tubular capillary vessels; as a consequence of this obstruction to the onward current of the blood, the Malpighian capillaries and the arteries which supply them become gorged with blood; " this engorgement being exactly analogous to that of the right side of the heart, and of the venous system, in animals after death from asphyxia."   Ultimately, there is retardation of the circulation in the systemic capillaries.

As a consequence of the retention of the constituents of the urine in the blood, Dr. Johnson says, the capillary circulation throughout the body is impeded; and " hence," he adds, " the quick and throbbing pulse, with general febrile excitement, dropsical effusion, and, in some cases, inflammation of the serous membranes or other parts, or disorder of the cerebro-spinal functions." (p. 113.)

It must be understood that Dr. Johnson does not mean, when using the expression, morbid condition of the blood, that any form of renal disease is a blood-disease, in the sense in which Drs. Walshe and Finger affirm that Bright's disease is a blood-disease.   These pathologists agree in maintaining, that Bright's disease is a peculiar, a definite disease of the blood ; a disease of the blood, from which, as Finger expresses it, the patient may die before the organ which ordinarily makes the disease manifest by its lesion, has become affected.   Dr. Johnson intends simply, that some poison is contained in the blood, that poison being alcohol, or the poison of scarlet fever, and that the renal cells make an effort to eject this poison from the system, and so become diseased, and pass off from the lining membrane.   We know that some forms of albumen more readily pass through animal membranes, than do other forms of the same substance. Dr. Parkes* has shown, that during digestion in some cases of Bright's disease, the albumen of the blood is in a condition thus favourable for transit through the renal capillaries.   Now this would be a real blood-disease, an abnormal condition of one of the blood elements, by which its escape through the walls of the capillaries is favoured; here the renal in common with all the other tissues of the body must suffer, and albuminous urine be its consequence.   The first departure from health would be either in the primary or secondary assimilative processes, the blood-disease would

* See Medical Times, April 10, 1852.

be the consequence of the mal-assimilation of its albuminous element, the dropsy and the renal affection the necessary sequence of the blood-disease. Here, it will be observed, there would be no poison to be eliminated by the renal cells from the blood ; and no more effort to eliminate the abnormal albumen by the renal epithelium, than by the epithelium of any other organ, or than by the subcutaneous cellular tissue.

We now come to the question placed by Dr. Johnson himself at the head of the section which concludes his chapter on acute desquamative nephritis. Is there a disease to which the term desquamative nephritis is applicable? Frerichs, as Dr. Johnson observes, answers this question in the negative ; and we must, after a most careful perusal of Dr. Johnson's work, come to the conclusion, that the latter has advanced no evidence of sufficient weight to satisfy us of its existence.   We do not deny that in some cases of renal-disease large quantities of epithelium are to be found in the urine, any more than we deny that in bronchitis large quantities of epithelium are to be found in the sputa ; but what we do deny is, that Dr. Johnson has offered any proof that there is a disease, the substantive feature of which is a process of desquamation of the renal epithelium. That the disease known as acute inflammatory dropsy, or as acute Bright's disease,— that the disease described by Frerichs as the first stage of Bright's disease,—that the disease of which Dr. Johnson has quoted the analysis of the blood by Dr. Christison,—that this disease consists essentially in an effort of the epithelium-cells to eliminate from the blood some poison, some specific *materies morbi*, we deny that Dr. Johnson has offered any substantial evidence. Of assertion we grant he has been prodigal ; of hypothesis he has been profuse ; but of evidence, of facts, we feel bound to aver that his book, on this point, contains far too little for our satisfaction.   He tells us in every page what is likely, what is probable, what may be supposed ; but in no page does he demonstrate what is. In vain do we look for sound induction or logical reasoning. Hypothesis is built on hypothesis ; the first rule of logic is violated in every chapter.

We say, then, that in the work before us there is no evidence of the existence of a substantive disease, to which the term desquamative should be applied in the sense in which it is used by Dr. Johnson ; and secondly, that there is no evidence that the cases Dr. Johnson has grouped under the head of acute desquamative *nephritis*, are examples, in the largest sense of the word, of inflammation of the kidney.

Our readers must understand, that while we affirm that Dr. Johnson has not succeeded in proving the existence of acute desquamative nephritis as a substantive disease,—has failed in his attempt to erect this new pathological genus,—we admit that he has done good service to the science of medicine, in forcibly directing the attention of observers to the fact, that in many cases of disease of the kidneys, large quantities of renal epithelium are detached from the basement-membrane of the tubes, and that from the loss of this epithelium, evils of a serious nature result.   Dr. Johnson has failed to show that this desquamation is pathologically anything more than is the detachment of epithelium from other parts.   We know that epithelium may be detached from other organs by fluid thrown out by the vessels subjacent to the basement-membrane, which fluid, transuding through the basement-membrane, loosens the adhesion between the membrane and its epithelial scales.   We see this taking place constantly on the

bronchial surfaces, and in cases in which large quantities of fluid are quickly effused from the intestinal canal. We know that in acute Bright's disease there is congestion of the organ, and effusion of serosity, and escape of fibrin ; and we have therefore a ready explanation of all the phenomena we witness, without resorting to the hypothesis of the elimination of a poison by the renal epithelium.

The cure of acute desquamative nephritis is effected, our author considers, when all the poison which excited the renal disease has been eliminated. Either of two hypotheses may be adopted to explain the large quantity of urine passed during the period of convalescence : one being that the epithelium which has accumulated in the tubes stimulates the Malpighian bodies " to profuse secretion of water ;" the other, that " the abundant secretion of urine is a consequence of the urea and the other urinary constituents which have accumulated in the blood during the desquamative stage of the disease, and subsequently finding a free outlet, exert their natural diuretic influence."

Here, as elsewhere, hypotheses fall from Dr. Johnson's pen—

> " Thick as autumnal leaves that strow the brooks
> In Vallombrosa"—

and seeing how many of those which he propounded but the other day, have been since retracted by him, we may add, they fade as fast.

We find nothing in the treatment of acute desquamative nephritis differing from the treatment of acute Bright's disease, as ordinarily laid down by writers on that affection. Cupping on the loins in severe cases, purgatives, especially sulphate and carbonate of magnesia, bitartrate of potash, with jalap and compound extract of colocynth, and antimonial diaphoretics, and the hot-air bath, are the remedial agents chiefly relied on by Dr. Johnson, as by other judicious practitioners ; and like them, too, Dr. Johnson abstains from diuretics. Now in all this we certainly fail to see how Dr. Johnson has been aided by his pathological opinions ; what his pathological hypothesis has discovered for him. Experience had taught us long ago to act on the bowels and skin, and to deplete from the loins, as well as to abstain from irritating the kidneys by the employment of diuretics. Dr. Johnson, by the aid of his hypotheses, has learned to do no more. Nay, we believe that his sound sense and his experience have prevented him from reducing his hypotheses to their legitimate conclusion. For if the desquamation of the renal epithelium be, in a given case, but an effort of nature to eliminate the poison of scarlet fever, then it seems that we ought to aid nature in her efforts to determine as much blood as possible to the organs, and so to get rid, as soon as convenient, of the poison ; but experience has shown that whatever determines a flow of blood to the kidneys inflicts an injury on the patient, and so we rejoice that Dr. Johnson refuses to allow his hypothesis to carry him to the end of its malevolent career.

We pass by the brief sections on cause and diagnosis. With reference to prognosis, two questions, Dr. Johnson says, will arise. First, Has any permanent injury been inflicted upon the kidney ? Secondly, Is it probable that the patient will have a return of the disease ? If the urine returns to its natural condition, the patient, Dr. Johnson says, is completely cured, although it is possible that here and there a tube or a Malpighian body has been spoiled.

In answer to the second question, Dr. Johnson remarks:

"When the renal disease has originated in any non-specific cause—such as fatigue, anxiety, intemperance or irregularity in eating and drinking, or exposure to wet and cold—there is, I believe, greater risk of a second attack than in those instances of the disease which are traceable to the influence of a specific morbid poison, such as that of scarlatina. This opinion is based partly upon the actual observation of cases, and partly upon the consideration that the non-specific influences are much more likely than those which we consider specific, to be perpetual or frequently repeated." (p. 122.)

And again:

"It is not improbable that a patient, who has had an attack of renal disease excited by *any one* of the before-mentioned causes, may be more than usually susceptible of all the influences which are known to produce the disease. For instance, that when scarlatina has been complicated with acute renal disease, any subsequent exposure to cold is more likely to injure the kidney than when there has been no previous attack of disease. I repeat, that such a result is not improbable, but I believe that the risk is very slight when the original disease has been entirely removed; a condition which has been assumed throughout." (p. 124.)

Now we think that these questions were such as Dr. Johnson might, by a careful analysis of his notes of cases, have settled definitely; by a little trouble he might have been able to speak positively on the matter in question, and would not have had to talk of a probability, partly founded on observation, and partly on *à priori* reasoning.

In the Fourth chapter, Dr. Johnson describes the affection which he terms *Chronic Desquamative Nephritis.* In a large proportion of cases, it is associated, Dr. Johnson thinks, with the gouty diathesis.

"In general terms, it may be stated that intemperance in drinking is the origin of nearly all the cases of chronic desquamative nephritis; that in a few cases it may be traced to a previous attack of acute disease, and there will still remain a comparatively small number of cases which are connected with a derangement of the general health, the result either of an original weakness or unsoundness of constitution, or produced by the slow and long-continued operation of such depressing influences as bodily or mental fatigue and anxiety, deficiency of nutritious food, lengthened confinement to bed by disease or accidental injury, or unwholesome occupations which involve a neglect of exercise and the breathing of impure air." (pp. 172, 173.)

Chronic desquamative nephritis may reach an advanced stage, without arresting the attention of the patient or his friends. The symptoms—some of which, however, are often absent—are thus detailed by our author:

"A gradual loss of strength, with emaciation to a variable extent, in some cases being very great, while in others it is inconsiderable, or it is concealed by the anasarcous swelling of the body; defective perspiration, with a dry and harsh state of the skin, a peculiar pallid or sallow colour of the skin and lips, or a blending of pallor with a dusky hue, depending on the original dark complexion of the patient. The tongue is sometimes dry, at other times moist and pale; there is commonly thirst, with loss of appetite. The last symptom, however, is variable, the appetite being sometimes voracious; there is almost constantly flatulence, and sometimes gastric pain: not unfrequently, too, the patient is troubled with water-brash, or with retching and vomiting, especially on first awaking in the morning; pain or a sense of weight in the head, and sometimes a tendency to drowsiness, are complained of. I have noticed one symptom so frequently in connexion with renal disease, that I consider it worthy of mention, and that is, bleeding from the nose." (pp. 174, 175.)

Dropsy is by no means invariably present; and in very many cases, slight puffing about the face is all that is observed. Pain in the back is usually trivial in degree. Increased frequency in micturition is a much more constant symptom. However, as Dr. Johnson judiciously remarks—

" It is not to be supposed that, because a patient passes his urine with increased frequency, and is disturbed in the night by a desire to micturate, he is therefore suffering from disease of the kidney, since the same irritability of bladder will often be found to depend on acidity, or other abnormal conditions of the urine, resulting from dyspepsia, and entirely unconnected with renal disease." (p. 177.)

The least fallacious signs of the actual existence of the renal disease, are those derived from the condition of the urine. In the earliest stage of the disease, the signs of which may often be detected, when inflammation of the joints is subsiding after an attack of gout, the urine, nearly normal so far as relates to quantity, specific gravity, and colour, " is perhaps more acid than usual, but contains not a trace of albumen." It is clear when first passed, but after standing a few hours, deposits a little fine dust-like sediment. This, examined by the microscope, " is found to consist partly of a scattered amorphous granular material, and partly of the same material in the form of cylinders," which Dr. Johnson terms " granular casts." These granular cylinders, Dr. Johnson says, are evidently disintegrated epithelium. Of this we may remark, however, he only offers as proof, that one or more epithelium-scales are now and then found entangled in them. We advise Dr. Johnson to try the effect of reagents on these granular casts. Most certainly they are not always composed of disintegrated epithelium, as their ready solubility proves. Our experience leads us to conclude that they are formed, in many cases, of granules of lithate of soda, held together by a protein-compound, and in others by granular fibrin. They are found in the urine only during the gouty paroxysms, and disappear after the inflammation of the joints has subsided.

In the next stage, the following are, according to Dr. Johnson, the characters of the urine:

" During the attack of gouty inflammation in the joints, the secretion is scanty, and its specific gravity rather high; it is also albuminous, and continues so for a variable period after the gouty paroxysm is over." (p. 180.)

The deposit of granular casts is now pretty abundant. In the later stages of the disease, the urine becomes permanently albuminous. The quantity of the albumen is in some cases abundant to the last, while in other cases it becomes very scanty. The quantity of the urine continues greatly to exceed the healthy standard, till near death; its specific gravity is low. The daily discharge of solids, Dr. Johnson states, on the authority of Dr. Christison, is deficient. With reference to the microscopical characters of the urine in this third stage, Dr. Johnson describes three varieties of deposits.

1st. The sediment is rather copious, and is composed of granular casts, such as we have described above, in larger number than in the earlier stages. 2nd. The sediment is copious, and consists of casts of a peculiar whitish, waxy appearance, with a well-defined sharp outline. These, he considers, are " moulded in tubes, which have been deprived of their epithelial lining by the desquamative process." These waxy casts are mixed with granular casts. 3rd. In other instances, " the sediment is very

scanty. It contains the large waxy casts in small numbers, and occasion-
ally a few granular casts, or some disintegrated epithelium." With regard
to the appearances in the urine, in those cases of acute desquamative
nephritis which are unconnected with gout, they are "so far as I [Dr.
Johnson] am acquainted with them," essentially the same as those described
above as occurring in connexion with the gout.

In describing the chemical constitution of the blood, Dr. Johnson quotes
Dr. Christison's account of that fluid in the advanced stages of granular
kidney.

Secondary Diseases.—The whole of this section is very far inferior to
that of Frerichs on the same subject.* The only points worthy of notice
in it are, that, according to Dr. Johnson's observations, in the class of cases
he knows by the name of chronic desquamative nephritis, tubercular
disease of the lung is so rare, that there is no reason for supposing any
special relationship between them; and he continues :

"It is not *improbable* that if this chronic renal disease occurred in a person
predisposed to phthisis, the morbid tendency might be favoured and the disease
developed ; nor is it *unlikely* that chronic inflammation of the kidney may be an
occasional consequence of pulmonary consumption; but this kind of connexion
between the two diseases is not of frequent occurrence." (pp. 195, 196.)

And with reference to the relation between hepatic disease and chronic
desquamative nephritis, he observes:

"The form of hepatic disease which is most frequently associated with this
chronic nephritis, is that which is commonly called cirrhosis, or the gin-drinker's,
tuberculated or hob-nailed liver. And it seems *probable* that the two diseases are
as closely allied as is consistent with the difference in the structure and functions
of the organs concerned." (p. 207.)

We would again ask Dr. Johnson, if this kind of information is such as
we had a right to look for at his hands? Surely, had he analyzed his own
cases, he must have been able to say how often desquamative nephritis
and phthisis pulmonalis had been combined in his experience; how often
cirrhosis of the liver and chronic desquamative nephritis had been found
in the same subject. These would have been facts, and from them con-
clusions might have been legitimately drawn. For our part, we are at a
loss to conceive why cirrhosis of the liver, a disease consisting in the
exudation of lymph—common induration-matter—into the areolar tissue
of the capsule of Glisson, should *probably* " be closely allied" to chronic
desquamative nephritis; that is, to a disease, the pathology of which
is, according to Dr. Johnson, an effort to eliminate by the renal epithe-
lium some poison from the blood. " I must here repeat what has so often
been *asserted* before, that the central starting-point of all renal disease is a
morbid condition of the blood; the secreting-cells of the kidney *strive* (*sic*)
to remove from the blood some of the poisonous materials with which it is
charged, and in doing so, they become changed in appearance and struc-
ture." They may be closely allied; but certainly we see no reason, *à priori*,
on Dr. Johnson's hypothesis, why they should be so.

Morbid Anatomy and Pathology.—Early in the disease, the kidneys are
of the natural size and weight, and they present to the naked eye no
appearance of disease. On a microscopical examination, the epithelial

* See British and Foreign Medico-Chirurgical Review, vol. ix. p. 285.

cells of the convoluted tubes have an "unusual, finely granular appearance; in some tubes, there is an appearance of entire cells having been
shed, so as to fill the tubes, and render them opaque." After the desquamative process has continued some time, new cells are no longer formed on
the basement-membrane, which is left quite denuded.   "The most common
result of the destruction of the epithelium, is the gradual wasting of the
tube," and "it is easy to trace them dwindling down from their normal
size to an almost invisible degree of minuteness."*   In consequence of the
wasting of the tubes, and the collapse (Dr. Johnson says) of the surrounding
tissue, the Malpighian bodies are brought down together, and appear relatively more numerous.   There are found in the degenerated tubes; 1st. An
unorganized fibrinous or albuminous material; 2ndly. Oil; and 3rdly,
Serum.   The fibrinous material is that which forms the "large waxy
casts," and is secreted, Dr. Johnson concludes, by the basement-membrane,
because the larger casts formed by it are often hollow.   The smaller solid
casts, Dr. Johnson thinks, are derived from the Malpighian bodies.

A deposit of oil is observed now and then, but only in a few tubes.   The
oil is sometimes contained in cells, but more commonly is in clustered masses.
In Dr. Johnson's paper on the Inflammatory Diseases of the Kidney,
contained in the 30th vol. of the 'Medico-Chirurgical Transactions,' we
find the following statement:

"In a case of simple acute or chronic nephritis, the quantity of oil in the
secreting-cells is very small, sometimes indeed none can be detected.   But it
frequently happens, that after a tube has been stripped of its secreting cells,
in the manner before mentioned, an accumulation of fatty matter occurs in its
interior, the denuded basement-membrane becomes scattered over with separate
oil-globules, and these increase in size, till they form masses of fatty matter,
having much the appearance of adipose tissue; and such a mass is *frequently*
washed out from the tube, and may be detected in the urine." And again, "*The
detection in the urine of such a mass as is represented in Plate* 6, *fig.* 8, *would
of itself justify the inference, that the kidney from which it had come was in a
state of chronic inflammation,* rather than of fatty degeneration."

He now says,

"Since the publication of that paper, I have frequently observed bodies similar
to the one there figured (Plate 6, fig. 8), and I HAVE DISCOVERED THAT THEY ARE
CLUSTERS OF STARCH, *the result of flour or crumbs of bread having accidentally
fallen into the urine.*" (p. 216, note.)

This ought to teach young microscopists to look twice before they publish once.   Had Dr. Johnson used common prudence, he could not have made
so terrible a blunder as that of describing and figuring, and then supporting
a theory by, a few crumbs of bread that had fallen into the patient's chamber
utensil.   Why this beats the antiquary hollow.   He only bored his friends.
Dr. Johnson discourses *de micâ panis* to the St. Stephen's of Medical
Societies.

A third condition, Dr. Johnson says, in which the tubes that have
lost their epithelium are often found, is that of dilatation.   Serum is
effused into these dilated tubes, and ultimately from them cysts are formed,
visible to the naked eye.   The normal epithelium is sometimes replaced by
"a layer of delicate nucleated cells."

* This, as well as other points with reference to the anatomical structure of the diseased and
healthy kidney, is illustrated by well-executed woodcuts.

" I do not *remember* to have seen these cells in any tubes below the natural size, and which I *suppose* to be diminishing from atrophy. It appears, therefore, a reasonable inference, that the cellular lining in question is an evidence of life and growth in the tubes which possess it. I am not *certain*," he adds, "whether these cells are necessary for the secretion of serum into the tubes, or whether the denuded basement-membrane alone has the power of secretion, but I *think* it *probable* that the cells are always present in the early stages, and that, subsequently, when the serum accumulates and dilates the tubes, their cellular lining becomes flattened and inconspicuous." (pp. 217, 218.)

Again we ask, is this guess upon guess legitimate in a work professing to re-arrange all diseases of the kidney on a new scheme? Is such writing and reasoning well placed in a work professing to found new pathological genera? If any one else had written such — what shall we term it? —we know no one whose judgment on its worth would have been more correct than that of Dr. Johnson himself.

Two hypotheses are offered by Dr. Johnson to account for the accumulation of liquid in the tubes, so as to dilate them into cysts. The first is the loss, from the interior of the tubes, of the ciliated epithelium, which ciliated epithelium Dr. Johnson has never seen in the human kidney, but which, from analogy, he fancies exists there. This loss,—i. e. the loss of the hypothetical ciliated epithelium,—he says, " will sufficiently account for the uncertain escape and occasional accumulation of the contents of such tubes as still retain any secretory power." The second hypothesis is, that the tubes near the medullary cones become impacted with detached epithelium, and then " it follows that whatever materials are secreted within them must accumulate and distend them."

Varieties in the External Appearance of the Kidney.—After the disease has lasted some time, a diminution in the weight and bulk of the organ is the most constant result. At first, the wasting is limited to the cortical substance, which becomes thinner as the disease progresses; if there has been no deposit in the organ, it " continues to waste, but retains in a great degree, until an advanced stage, its smoothness of surface;" there is a gradual loss of vascularity, and in some cases extreme atrophy. " The surface is whitish, firm, and corrugated, with here and there patches of vascular engorgement. The medullary cones have now shared in the general atrophy."

When the unorganized waxy material has been deposited in the tubes at an early stage, and in large quantities, then—

" The size and weight may be very little below the average; and in proportion to the quantity of the secondary deposit will be the yellowish-white, firm, and wax-like appearance of the cortical substance. Where the deposit is most abundant, the vascularity is least so; when it is recent, the surface remains smooth, but after a time the deposit contracts, and forms the firm, white granulations, varying in size from a pin-head to a pea, by which the capsular surface of the kidney is often roughened. In cases where the deposit is comparatively scanty, these granulations are the only outward indications of its existence. In these cases, too, the wasting of the cortical substance and the secondary deposition may proceed slowly and simultaneously, so that the firm, white, coarse granulations may be found in kidneys reduced to one-half of the normal size and weight." (pp. 226, 227.)

The Condition of the Bloodvessels of the Kidney.—Dr. Johnson states that in chronic desquamative nephritis, the walls, supposed to be muscular,

of the minute arteries of the kidney, are greatly thickened, and, at the same time, the arteries themselves are tortuous. At a very advanced period of the disease, these arteries " may often be observed to contain oil-globules here and there within their canals. The walls of the capillaries of the Malpighian corpuscles are opaque and thickened, and sometimes roughened on the outside by a deposit of fibrin." Finally, there is " cessation of the circulation, with atrophy and shrinking of the vessels." The inter-tubular capillaries and the veins appear unchanged in structure.

The Treatment of chronic desquamative nephritis is discussed at considerable length.

" The general principles by which the treatment of chronic nephritis should be regulated, are essentially the same with those which are available in the treatment of the acute form of the disease; but the particular remedies, and their mode of administration, will, of course, be modified according to the stage of the disease and the character of the symptoms." (p. 263.)

The causes of the disease are to be avoided; if this be impossible, then the patient " should be fortified against them." Then " the morbid state of the blood, and of the constitution generally, of which the renal disease is only a secondary consequence and a manifestation," are to be removed.

The diet must be carefully regulated; animal food may be taken daily, and vegetable in moderation; pastry, as difficult of digestion, must be avoided. As a rule, the less fermented liquors taken, the better. In gouty cases, " malt liquors and most kinds of wine are absolutely poisonous." The patient must avoid exposure to the cold and vicissitudes of the weather. " Flannel should be worn next the skin throughout the year." Moderate exercise in the open air is desirable. " Great and extraordinary benefit" is sometimes derived from a sea voyage. A temporary residence on the sea-coast, or at some inland watering-place, may be in some cases beneficial. The action of the skin may be promoted by an occasional use of the warm bath, and diaphoretic medicines. Moderate doses of purgative medicines are to be given, for the purpose of regulating the bowels; and when the local disease is connected with gout, small doses of colchicum. Local bloodletting is only to be resorted to, when the call for its employment is urgent. The more advanced the disease, the less able is the patient to tolerate loss of blood. Dry cupping may sometimes be substituted for it. If counter-irritation be employed, it is to be effected by ammonia, mustard, or tartar-emetic. Cantharides and turpentine are to be carefully avoided. Iron is often a valuable remedial agent in the treatment " of chronic renal disease." The tincture of the sesquichloride is Dr. Johnson's favourite preparation.

Mercury is admissible as an occasional aperient; but then, the practitioner must bear in mind the readiness with which the subjects of renal disease are often salivated. Mercury may also be given to promote the action of diuretics. This latter class of remedies, however, are themselves only to be employed for the purpose of removing excessive dropsical effusions, and " after other medicines have failed to give relief."

Having thus considered the treatment of the renal disease, Dr. Johnson discusses the treatment of those complications which frequently arise in its course. With reference to the dropsical effusion, our author remarks:

" The purgative method of removing dropsical effusions is that which is, perhaps, the most efficient, and at the same time attended with fewer risks and dis-

advantages. Those purgatives must be selected which produce copious watery stools. Elaterium, gamboge, and jalap, either with or without the addition of a saline, are the medicines experience proves to be most useful." (p. 279.)

When spontaneous diarrhœa is present, Dr. Johnson says " it is doubtful whether it can, with perfect safety, be suddenly or completely checked." In this case, he recommends measures to be adopted that tend to increase the cutaneous transpiration. " Diuretics are only to be resorted to under the circumstances before mentioned. When all other means have failed to remove the dropsy, a few punctures must be made in the tense integuments."

The treatment of the dyspeptic symptoms, diarrhœa, pulmonary complications, inflammations of serous membranes, chronic rheumatism, cerebral disorders, disease of the heart, and cirrhosis—are all briefly described.

We shall pass by the section headed, " Additional Observations on the Origin of Cysts;" merely observing, that Dr. Johnson is inclined to doubt the accuracy of the opinions, which were first advanced by Mr. Simon, and which have been most ably supported by Rokitansky and Paget, respecting the origin of cysts in the kidney. With reference to the former, Dr. Johnson remarks—" Rokitansky has fallen into the very grave error of representing the normal fibrous matrix as a product of disease." And again :

" There are two serious defects in the history which Rokitansky has given of the development of renal cysts ; the one arising from an insufficient regard to the normal structure of the kidney, and the other from inattention to the peculiar appearances assumed by the tubes, partly resulting from their relations with the other tissues, and partly as a consequence of changes in the tubes themselves." (p. 322.)

Henle, Dr. Johnson says, has fallen into the same mistake as Rokitansky, with reference to the existence of the fibrous matrix. Frerichs, as we mentioned in our number for April, p. 287, doubts the existence of this fibrous matrix,—" a piece of scepticism which I [Dr. Johnson] am at a loss to account for."

It is clear that there is an error of observation somewhere. Can it be that Dr. Johnson and Mr. Goodsir have mistaken the basement-membrane of the tubes, after the detachment of the epithelium, thrown into folds by compression, for a fibrous matrix, as suggested in Quain and Sharpey's ' Anatomy'?—

" Mr. Goodsir, and more lately, Dr. Johnson, have described a fibro-cellular framework, or matrix, as pervading every part of the renal substance, and supporting the uriniferous tubes and bloodvessels. We must confess, that although small bundles of fibrous or cellular tissue are met with here and there, accompanying vessels in the substance of the kidney, we have not been able to observe any continuous pervading structure of fibro-cellular tissue. The apparently reticular framework which is displayed by washing a thin slice of the kidney" [that is the mode Dr. Johnson adopts for demonstrating the fibro-cellular matrix] " has seemed to us to be formed by the basement-membrane of the transversely, or obliquely-cut tubes, deprived of epithelium, and connected by a transparent tissue, which is doubtless made up principally of vessels (although in the uninjected state they do not definitely appear), and has granular corpuscles dispersed in it." [*]

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[*] Quain's Anatomy. Edited by R. Quain, F.R.S., and Prof. Sharpey, F.R.S., p. 1198.

Does Dr. Johnson think, then, that when he macerates a slice of kidney in water, he washes away the basement-membrane? and if not, where is it figured in his plate? Certainly, if in the healthy kidney any distinct structure, such as is figured in Dr. Johnson's book, page 17, exists, the German and English observers whom Dr. Johnson criticises, are less clear-sighted than we have been accustomed to regard them. The fibrous matrix between the tubes is represented by Dr. Johnson to be as broad as the diameter of some of the tubes. If Johnson is right, then we advise Henle, Rokitansky, Frerichs, and Sharpey, forthwith to do with their microscopes what Prospero did with his books; for it will be clear that they are of little use to them.

In his Fifth chapter, Dr. Johnson describes *Waxy Degeneration of the Kidney.* In the acute form, the symptoms are the same as those of acute desquamative nephritis, only the renal casts are of the large waxy variety. Dr. Johnson has never examined a fatal case of acute waxy degeneration of the kidney.

"A chronic disease, characterized by the gradual degeneration of the epithelium of the kidney into the waxy material in question, may occur, without being preceded by, or associated with, the true desquamation, either in an acute or a chronic form, or certainly when the desquamative process forms a very small part of the disease." (pp. 329, 330.)

Two fatal cases are given in illustration of this chronic form of the affection. In both, the kidneys were large and pale, and had a wax-like appearance; and in the majority of the tubes were found large waxy casts, but no epithelium. A similar deposit existed, in one case, in the liver. The disease considered by Dr. Johnson to be waxy degeneration of the liver, is evidently that described by Rokitansky as lardaceous.

His Sixth chapter is headed by Dr. Johnson, *Non-desquamative Disease of the Kidney.* Non-desquamative disease of the kidney is attended by the same general symptoms as desquamative nephritis, but in the urine no trace of epithelium can be detected. It may occur in an acute and a chronic form. By an examination of the kidney alone, the observer may pronounce, Dr. Johnson says, on the presence or absence of desquamation. We may observe, that neither here nor elsewhere does Dr. Johnson allude to the fact, that the renal epithelium, like the intestinal epithelium, separates very readily from the basement-membrane after death. This, certainly, is a source of fallacy, which ought to be taken into account in determining, from an examination of the kidney, on the presence or absence of desquamation of the epithelium during life. We have repeatedly examined the kidneys of those whose urine contained during life no trace of epithelium, and found the tubes filled with detached scales; and we have examined kidneys which, when first removed from the body, had the epithelium attached to the basement-membrane, but which, on a second examination some hours subsequently, had the tubes filled with epithelium detached from the basement-membrane. Or, as Dr. Johnson describes the appearances found in a kidney sent to him for examination—"The convoluted tubes were uniformly filled with desquamated epithelium." We could find, we say, no better terms than these to apply to a condition generated after death under our own eye.

Dr. Johnson's work contains the histories of twenty fatal cases of renal

disease; but in eight cases only are we informed how long after death the body was inspected, and in three of these eight cases the kidneys were sent to Dr. Johnson after the examination of the body, and the date of their examination is not stated; and with reference to the other five cases, one was examined four days after death, putrefaction having commenced, and one only of the remaining four within twenty-four hours after death.

Although generally free from epithelium and casts of tubes, the urine, in non-desquamative disease of the kidney, now and then contains "small waxy casts" without epithelium. In commenting on a case of this kind in the 'London Journal of Medicine,' February, 1851, Dr. Johnson wrote:

"The almost entire absence of epithelium from the urine showed that the secreting structures were but little involved; *since any morbid matter in the blood, injuriously affecting the secreting cells, would have led to a desquamation of these structures.*"

In June, 1852, Dr. Johnson discovers this *à priori* conclusion to be all a mistake, and so he is now led to another *à priori* conclusion—viz., that "the free shedding of the epithelium is an efficient means of eliminating abnormal and noxious products;" and consequently, that in some cases free shedding of the epithelium is a favourable sign.

Kidneys affected with non-desquamative disease usually present an increase of size and weight. The cortical substance is of a wax-like pallor, while the medullary cones have a pinkish colour. The surface is smooth; the vessels are impervious. This is the waxy kidney of Dr. Gairdner. Dr. Johnson thinks that the increase in size and weight of the kidney are due to hypertrophy of the gland-tissue. In criticising Dr. Gairdner's work[*] we expressed our own opinion on this matter.

Fatty degeneration of the Kidney occupies the Seventh chapter.

Dr. Johnson distinguishes two forms of this disease:—1st, The granular; and 2nd, the mottled fatty degeneration. The granular form is preceded by the non-desquamative disease,—

"This being indicated by the condition of the urine, which is highly albuminous, clear, and without sediment; whereas the second form of fatty degeneration comes on very gradually and insidiously, and frequently makes great progress before the urine affords any indications of the existence of renal disease." (p. 392.)

Dr. Johnson retracts the opinion he formerly expressed—viz., "that it was rare to find an increase of oil in the kidney, without a similar increase in the cells of the liver." Nay, the reverse he now finds to be very often true. Oh! how we sigh for numbers, when we read these assertions and counter-assertions of our author. How many cases of fatty kidney has Dr. Johnson notes of, and in how many of these was an excess of fat found in the liver? The question is definite, and the answer might be as definite.

The Treatment of waxy degeneration, non-desquamative disease, and fatty degeneration, is disposed of by Dr. Johnson in two pages. "It must be regulated," he says, "in accordance with the general principles which have already been fully explained in connexion with the acute and chronic forms of the desquamative disease."

Suppurative Nephritis, which occupies his Eighth chapter, is divided by Dr. Johnson into suppurative nephritis from morbid conditions of the

[*] See British and Foreign Medico-Chirurgical Review, vol. ix. p. 285.

blood, nephritis from external violence, nephritis from retention of urine, nephritis from calculi in kidney.   The pus-corpuscles generated in the first form, Dr. Johnson thinks, serve the purpose of eliminating some material from the blood, differing greatly from the normal constituents of the secretion.   Purulent casts of the tubes of the kidney are found in the urine in suppurative nephritis.   Both desquamative and suppurative nephritis, Dr. Johnson says, " originate in a *beneficial* effort to eliminate noxious matters from the blood," only in the latter the noxious material differs more from the natural secretion than in the former.—The other sections of this chapter are exceedingly brief.

Tubercular Disease of the Kidney and *Cancer* of that organ occupy the Ninth and Tenth chapters of the work before us.   The account of the former is chiefly drawn from Rayer; that of the latter, from Rayer and Walshe. A remarkable illustration, we may observe, of the difficulty attending the diagnosis of cancer of the kidney, occurred lately in the person of one of the most able and philosophical physicians this country ever produced—a man in whom great power of observation and sound judgment were combined in a manner rarely witnessed.   He suffered from cancer of the kidney; and although he had watched his own case carefully, and had consulted some of the most accomplished physicians in the metropolis, it was only a few days before his death that the nature of the affection under which he laboured was suspected.   The disease was supposed to be renal calculus.

The last chapter of Dr. Johnson's work is devoted to the consideration of *Hæmaturia;* the various lesions that give rise to that symptom being shortly passed in review, and a few cases illustrating them given.

Had this work been by a man of less ability and promise than Dr. Johnson, we should not have devoted so much space to its contents ; but he has already added important facts to our store of knowledge ; he will, we trust, add many more.   He has gained, as we remarked when noticing the work of Frerichs on Bright's disease, an honourable place for himself among British pathologists ; and we looked with interest to see his matured views on a subject to which he had devoted so much thought and attention.   We trust that our criticisms will be taken as they are meant—as those of a friend.   We hope we have said enough to induce him to remodel his work when it passes—as it doubtless will, and as it deserves to do—into a second edition.   If we could persuade him to weed it of its host of hypotheses ; to analyze the notes of his cases, and then to fill the space now occupied by unprofitable discussions and hypothetical explanations, with the valuable results he could not fail to obtain, we should have done him, and even the science of medicine itself, good service.

" Je sais que la verité est dans les choses et non dans mon esprit qui les juge ; et que moins je mets du mien dans les jugements que j'en porte, plus je suis sûr d'approcher la verité."

This was the idea impressed on Louis' mind, when he produced his greatest work ; it was its spirit that animated Andral, when he gathered and used the materials of his clinique ; and if Dr. Johnson will produce a work on Diseases of the Kidneys to rank with those of the great masters of our science, he too must, like them, be content to put more of nature and less of himself into its pages.

ART. VIII.

Mémoires de la Société de Chirurgie de Paris. Tome Troisième.
Fascicule I. 4to, pp. 120.—*Paris*, 1852.

THE Surgical Society of Paris, although little, if at all, encouraged by the
seniors of the profession in that city, has made good its ground as one of
the most useful of the medical associations. The discussions which take
place at its meetings are usually spirited and scientific, and most of the
Transactions it has published are well worthy of attention. In noticing
them, we have usually preferred an analytical to a critical review, both for
the sake of economy of space, and in order to enable us more exactly to
exhibit the procedures and claims of " Young Surgery" on the other side
of the Channel. If any of these may seem objectionable, numerous other
opportunities of adverting to them offer themselves in our pages.

I. *Plastic Operations on the Face.* By M. Chassaignac.

The author observes that although facial autoplasty may seem, during
the quarter of a century since it has been revived, to have wellnigh reached
perfection, there may be still combinations which remain untried ; and his
object, in this paper, is to detail the successful results of some of the
modifications he has resorted to.

1. *Plastic Operations on the Eyelid (Blepharoplasty)*.—This is a case of
considerable interest, as showing that the surgeon may sometimes usefully
interfere in relapse from cancer, in cases in which prudence might seem
to justify him in refusing to do so.

Henry Vandeleur, æt. 61, had suffered, several years since, from hemi-
plegia, and he continues lame, his intellect being also somewhat impaired.
He was admitted in August, 1849, in an advanced state of cachexia,
having a cancerous ulceration of the lower eyelid. About nine years pre-
viously, he first perceived a pimple there, which continued stationary for
some years, until he tried escharotics for its destruction. On admission, the
ulceration occupied the whole of the lower eyelid, extending to, but not im-
plicating, the lachrymal sac, and reaching to one or two centimetres beyond
the external commissure. Above, it involved the conjunctiva, and extended
below to the naso-labial furrow. The operation was performed on the 30th
of August. The diseased parts were circumscribed by a semicircular incision
having its convexity downwards, and all those of a suspicious character
were removed, the sclerotica in the lower hemisphere of the globe being
thus laid bare, and the loss of substance altogether being considerable. A
curved incision was next carried from the temporal region towards the lower
maxilla, and then swept upwards into the naso-labial furrow to the lower and
inner margin of the loss of substance. A flap, with a broad pedicle, was thus
isolated, having an upper concave border formed by the external hemisphere
of the solution of continuity, and a lower convex one reaching to near the
edge of the lower jaw. This flap was dissected away, several vessels being
divided, and the facial requiring ligature. When it was thus rendered free,
" a movement of conversion" was imparted to it, by which, without laying
much stress on the pedicle, it was brought from an obliquely vertical into
a horizontal direction. In this way the most free and convex portion of the

flap was brought into juxtaposition with the skin of the lateral part of the nose, and there retained by sutures. The concave edge of the flap could now be brought towards the orbit, so as to form the free edge of a new lower eyelid, while the convex edge left a large hiatus in the cheek, which was concealed by the integuments drawn from the neighbouring parts by twisted sutures. The whole was dressed by occlusion.

On the 5th of September the parts were examined, and the flap was found swollen and puffed up, but not threatening to sphacelate. The sutures were removed, and the edges of the autoplastic union were touched with a solution of nitrate of silver. By the 10th, union was complete. At first, the thickness of the upper part of the flap formed by the parts brought from the cheek, was unsightly, but this appearance soon wore off. The patient was kept in the hospital, in order that the soundness of the cicatrization might be judged of; and on the 15th of October he became the subject of a severe attack of erysipelas of the face. The flap was invaded by this, but no ill consequences followed, and the patient was discharged well on the 10th of November; the flap, however, though fully maintaining its vitality, being quite insensible, even to the prick of a pin, &c.

Nearly seven months afterwards (May 23rd, 1850), he came to the hospital again in a cachectic state, and suffering from a relapse of the cancer at the inner angle of the eye, where a fungous growth, the size of a nut, had appeared, surrounding the upper and inner part of the flap, but in nowise involving it. On this account, and as the lymphatics remained unaffected, the tumour was thoroughly extirpated on the 30th of May. No plastic operation followed, the irregular wound left was dressed by occlusion, and cicatrization rapidly followed. On the 19th of June, an attack of facial erysipelas implicated all the parts, but was soon dispersed. The patient was discharged on the 1st of September, and could close his eye, notwithstanding the inertia of the new lower lid.

2. *Obliteration of the Orbital Cavity by Plastic Operation.*—This operation is undertaken for the important purpose of concealing, as far as possible, the hideous deformity resulting from the extirpation of the eye. In these cases, the artificial eye, requiring a stump of more or less size, to which the muscles remain attached, is not applicable; and when muscles, as well as globe, are all gone, it is to autoplasty alone that we can look for a diminution of the deformity. By its aid, too, we can prevent the incessant irritation which various bodies produce, when the hiatus left by extirpation remains.

In the case of this patient, a country labourer, æt. 50, in whom cancer of the globe rendered the extirpation of the eye peremptory, it was resolved to alleviate the resulting deformity at the same time. All the portions of the eyelids which were not involved in the disease were saved, in order to form the flaps, and their external zone was detached from the circumference of the orbit, towards the temple. After the globe was removed, the orbital plate of the ethmoid being found also in a diseased state, as much of it as appeared so was cut away with a strong scalpel, thus leaving a free communication with the nasal fossæ, and allowing air freely to enter the orbital excavation. The raw edges of the palpebral flaps were brought together, and the wound dressed. Owing to the liberation of the eyelids

by the preliminary dissection at the external border of the orbit, they could be drawn considerably from within outwards; but as yet they were far from being able to close the large aperture formed by the orbit and the inner part of the root of the nose. Stimulating injections were thrown into the cavity after the 3rd of July, and by the 25th it was much diminished in size, the eyelids, too, having become united in two-thirds of their extent. By the 6th of August, a rounded orifice, a centimetre in diameter, alone existed at the inner part, leading to a cavity filled with healthy granulations, on the inner side of which a motion produced by the air escaping from the nasal fossæ could be perceived. Since his dismissal from the hospital, the patient has returned at intervals, and the orifice can now be only perceived with great difficulty. His aspect is healthy, and the perfect regularity of the obliteration very much diminishes the appearance of the deformity.

M. Chassaignac, while attributing some of his success in this case to his having detached the palpebræ from the outer portion of the orbit, considers much of it to be probably due to the fact of the intercommunication between the orbital cavity and the nasal fossæ. The free issue which the secretions from the suppurating surface obtained in this direction, favoured the anterior occlusion; while the admission of the air prevented the falling-in or collapse of the eyelids during the early stage of the cure. So important does he regard this intercommunication, that where not rendered necessary, as in this case, he considers that it should still be produced.

3. *Plastic Operation on the Lachrymal Sac (Dacro-cystoplasty).*—It is an error that cannot be too strongly opposed, to believe, that when in fistula lachrymalis we have succeeded in re-establishing the course of the tears, we shall always be able, either by compression, scarification, or cauterization, finally to close the fistula itself. There are cases in which autoplasty is our sole resource; for the above-named means, so far from repairing the breach of surface that exists, only serve to increase it. When, besides loss of substance, there is excessive thinning of the integuments which form the margin of the fistula, and, worse still, when these are firmly adherent to the bone, autoplasty becomes the sole resource.

F. Leclair, a footman, æt. 30, finding the black patch he was obliged to wear, to conceal the fistulous opening at the inner angle of the eye, very detrimental to his pursuits, requested that an operation might be performed to rid him of his infirmity. Being a man of remarkable intelligence, his statements could be relied upon, and these would tend to show that his affection might be congenital, no other authentic example of such, however, being on record. At all events, an abscess formed at the angle of the eye two or three weeks after birth, and then bursting, has never closed since. Eight years previously, he was most carefully treated by A. Bérard, who, being unable to trace the lachrymal passages, perforated the unguis, and passed-in a canula, which has continued there ever since. Most of the tears then flowed away, but no means that had ever been tried was able to heal the fistulous opening. The rounded orifice of the fistula was of a bluish colour, and closely adhered to the bone. The canula could only be removed by cutting a little flap in the cheek, opposite its lower part. A few days were required to unite this, during which the patient's state, owing to the increased lachrymation, became temporarily aggravated by the removal of

the canula. The next step was to obtain a permanent dilatation, by means of bougies, of the accidental passage which Bérard had made, to secure a free passage for the tears. The autoplastic operation was then performed as follows:—A triangular flap was formed at the inner angle of the eye, having its base turned outwards, embracing the internal extremity of the two eyelids, and the apex terminating towards the root of the nose. The flap enclosed in its centre the fistulous opening, which was detached from its adhesions to the bony orifice. It was then completely detached in the shape of a V, with clean and regular edges. Had it, however, been attempted to bring them together without displacement (*glissement*), the lips of the V might have become torn by the sutures. Two flaps were therefore separated, one above and the other below; and by sliding them towards each other, they were united by some points of suture. This separation and sliding of the flaps prevented any tension of the integuments. At the end of the second day, the sutures were removed, adhesion being complete, as it has ever since continued. The epiphora was entirely cured; and beyond a somewhat greater breadth of the palpebral opening at the inner angle than ordinary, all traces of deformity were removed.

4. *Plastic Operation in the Parotid Region.*— Great difficulties attend plastic operations in the parotid region, especially when the object is to fill up excavations produced by the removal of tumours, &c. We may, on the one side, borrow flaps from the posterior part of the cheek, and on the other from the skin covering the upper part of the sterno-mastoid; and by careful dissection in these two regions, we may obtain laxity sufficient to conceal the loss of continuity. The integuments, however, in place of being applied accurately to the depths of the depression, stretch over it like a bridge; nor can their adhesion to these deep-seated parts be obtained even by the employment of compression—a proceeding always to be avoided, when possible, in plastic operations, from its tendency to induce mortification in the flaps. In place of taking his flaps in part from before, and in part from behind, M. Chassaignac takes the whole from the submaxillary region, separating a band of integument two or three fingers' broad, which, if carried in the imagination from ear to ear under the chin, would resemble a *cravat*, which is the appellation M. Chassaignac bestows on the operation. The flap thus detached, without any tension or twisting, and by mere displacement (*glissement*), can be brought over and adapted to the parts below with remarkable facility, and there retained by sutures. A case is related, in which this cravat operation was performed, after the extirpation of a tumour situated on the parotid gland, with complete success.

5. *Plastic Operation on the Lips* (*Labial Autoplasty*).—M. Chassaignac is not certain that the modification he proposes of this operation is new, or materially different from that employed by Velpeau; but at all events, he considers it a very preferable procedure to that ordinarily in use. The lip is forcibly everted, and the mucous membrane is transfixed by a scalpel held flatwise, at a distance from the free edge of the lip proportionately great to the thickness of the organ—the mucous membrane being more extensible as it recedes from the free edge. It is always sufficiently so to cover any section of the parts without stretching, so as to bring the mucous and cutaneous surfaces in contact. The skin near the edge of the lips, on the other hand, is not easily displaced, and it contributes much

less than the mucous membrane to the covering of the solution of continuity.

The mucous membrane, thus transfixed, is completely detached above, and turned backwards, retaining its adhesion at the base. All the diseased parts are now removed, either by the bistoury or the scissors, and the vessels tied. The mucous flap is brought forward, and exactly applied to the skin by means of four or five, or even six or seven points of suture—the adaptation to the cutaneous edge being adjusted with great nicety. Those who perform the operation for the first time, must be on their guard against an illusory appearance of relapse of cancer *in situ*. During the first week after cicatrization, the two extremities of the curvature described by the operation somet.mes become raised and indurated; but in the three patients in whom M. Chassaignac has observed these unfavourable appearances, they have disappeared again of their own accord.

6. *Observations.*—M. Chassaignac makes several interesting observations in the course of the paper, which will be better considered apart from the descriptions of the operations.

(1.) He seems to have considerable faith in the *tincture of aconite* as a preventive of *purulent infection*, a disorder which is either more frequently met with, or more dreaded, by the French surgeons than by our own. He administers the aconite for a few days prior and subsequent to all operations. In the case of *cancer*, some days after the operation, he substitutes *iodide of potassium* (*thirty grains per diem*) for the aconite. He employs the iodide, indeed, constantly in cancerous affections, finding that even when the cancer itself undergoes no appreciable modification, still the patient's general state is always ameliorated. M. Gosselin, from whom he learned the practice, has published the successful results of this kind which he has met with. The cachectic condition, and yellow aspect of the patient, undergo great modifications.

(2.) The occurrence of *erysipelas of the face* in the first of the cases narrated, leads M. Chassaignac to make some observations upon the prevalent opinion, that this is sometimes induced by the application of empl. resinæ, or diachylon. In the case in question, it is remarkable that the attack on both occasions did not come on immediately after the operations, or while the dressings were still applied to the wound; but after cicatrization was complete. Indeed, numerous facts have induced M. Chassaignac to advance what has been considered a paradoxical opinion, that adhesive plaster, so far from inducing erysipelas, sometimes acts as a *preventive*. In September and October, 1850, a kind of epidemic of erysipelas of the face and scalp prevailed in Paris. During this period he had ten or twelve persons in his wards labouring under it, none of whom had plaster-dressings; while among from twenty to twenty-five of his patients, who were under treatment for compound fracture, contused wounds, amputations, resections, cancer operations, &c., and in all of whom his plan of dressing by occlusion (which consists in the very prolonged employment of adhesive plaster) was adopted, not one acquired erysipelas.

One reason of the belief in this cause of erysipelas, is the confounding with it the local erythema which is so frequently produced by the plaster. True erysipelas is not, in fact, produced by mere local causes, but by some general or diathetic cause; and when we do see it following the applica-

tion of plaster, we cannot conclude that it would not have appeared had no plaster been used. The disease, too, is eminently capricious in its progress, and would never follow the geometrical lines which local erythema supervening on the application of plaster pursues. When we reflect upon the immense frequency with which plaster is applied, the occurrence of erysipelas is far too rare to be due to such a cause.

"For my part I declare that I have a great respect for the opinions of the masters of our art. Not that blind respect for authority, which is the faith of many men incapable of originating anything, and who follow routine to avoid the trouble of possessing and maintaining an opinion—but simply a respect founded upon this reason, that when men of eminent capacity have observed a series of facts for a long period, it is ten to one that, if our opinion does not agree with theirs, the truth is on their side, and the error on our own. In a word, before we attack any general opinion, we should be triply sure of what we advance.

"Still, how can we determine upon stifling a conviction that has only been formed after the observation of a considerable number of facts, and when we have been in no haste to draw our conclusions? This is just the position I find myself in with respect to this statement, that sticking-plaster does not produce erysipelas. For more than ten years have I now observed, day by day, with continued attention, all cases in which sticking-plaster has been applied, that have come under my notice—cases, I may say, that I have induced, inasmuch as during this period I have applied cuirasses of adhesive plaster in all kinds of traumatic lesions. At the commencement of my researches, I still acted, impressed with the fear of what I had been taught, concerning the danger of erysipelas from this cause. Now, my practical experience has constantly contradicted the received opinion. I have seen the development of erysipelas coincidently with the application of cataplasms, ointments, and charpie; but I have hardly ever seen it during the employment of my dressing by occlusion. Must I not, then, at my own risk and peril, make known that I differ from the generally received opinion? As to my proposition, that adhesive plaster preserves from erysipelas, let those who like treat it as a surgical heresy. It may be said that the absence of erysipelas in the different wards I have had under my charge, is rather due to the rarity of my renewal of the dressings than to the nature of the substances employed. Nevertheless, this paradoxical opinion will still have its utility, if it only exhibit this truth as demonstrated to myself: that adhesive plaster is not a cause of erysipelas." (p. 10.)

(3.) Another point adverted to by M. Chassaignac, is the *period of time at which sutures should be removed.* There seems to be no principle exactly established upon this point, one surgeon removing his sutures in forty-eight hours, while another allows them to remain three or four days. It is to be remembered, however, that every suture is a foreign and irritating body, and a cause of suppuration. Suppuration along so limited a track would be of itself of little consequence; and it derives its importance from its coincidence with, and vicinity to, a solution of continuity wherein immediate reunion is desired. M. Chassaignac entertains no doubt that suppuration at the points of suture is provocative of suppuration in solutions of continuity which they are employed to hold together; and the conclusion to be thence drawn is, that they should not be continued a moment longer than is indispensably necessary.

"In this respect, I divide the wounds which result from plastic operations into two classes. First, those which exhibit an active tendency to separation; and, secondly, those which exhibit only a passive tendency. Thus, take, for example, an upper lip that has been vertically divided. It is quite evident that in such a

wound an active tendency to separation exists, inasmuch as the muscular action of the dilators as well as the constrictors become, strangely enough, congeners in producing separation. A permanent muscular action is being exerted, and it can only be combated by a means whose durability of action is sufficiently prolonged to enable the cicatrization to become energetically resistant at the period when the sutures are removed. Here, in spite of the inconvenience there is in prolonging the application of this means, we must wait the time necessary for the production of a true cicatrization, and not a mere adhesion that may be easily destroyed.

"On the other hand, when we bring a flap of integument from the parotid region towards the cheek, there is no muscular action incessantly opposing the new situation imparted to the flap. If it become displaced, it will not be so by its own spontaneous action, but by a motion communicated to it, by some action which is not properly its own. There is only a passive tendency to displacement, and the sutures may be removed at an early period without the cicatrization being endangered. When a simple adhesion has been effected, we may remove the agents that have accomplished it. In this case, a duration of the application for twenty-four hours will accomplish all that the sutures can usefully do, and it is then we may remove them." (p. 35.)

(4.) In regard to the *nature of the suture employed*, M. Chassaignac usually employs separate points of suture where most surgeons use the twisted suture. He thinks this last has been much abused in plastic operations. Where energetic muscular action has to be opposed, as when the entire substance of the lip is concerned, it may be useful; but where the contraction is slight and insignificant, it is wrong to use a means that multiplies foreign bodies and consequent irritation, and is often difficult of removal.

(5.) One of the cases related in this paper, touches upon an interesting point—viz., the *influence of plastic operations in preventing the relapse of cancer*—an influence affirmatively maintained by M. Martinet de la Creuse, and supported by various authorities. The laws of relapse in cancer, however, are not sufficiently well known to enable us to state with any precision what influences may retard or prevent it, inasmuch as it is impossible for us to declare that relapse would certainly have occurred unless autoplasty were had recourse to. Still, the case in question lends a strong support to the opinion; for relapse of the cancer did take place, and the autoplastic flap escaped its influence.

II. *Dissection of a Hydrocele of the Neck, with Observations on the presumed Seat of the Affection.* By M. Richard. *With a Report on the Paper, by* M. Lebert.

M. Richard observes that although the researches of various pathologists have established the fact, that cysts may form in the neck independently of the thyroid gland, yet the pathological anatomy of these tumours, owing in part to the large size they sometimes have reached, or the surgical operations they have undergone, has remained obscure. For this reason, in the present paper, he furnishes an account of a cyst he recently met with while dissecting the neck of a well-developed female subject, about fifty years of age. The cyst, projecting about three centimetres, and filled with fluid, was found, on dividing the sub-hyoidean muscles, on the left side. It was separated from the trachea and the vessels of the neck by a thickening of the middle cervical fascia, within which it was ensheathed. Below, it terminated in a point opposite the sterno-clavicular articulation, and above

it extended into the middle of the space formed between the lower border of the thyroid body and the sternal fourchette. The tumour, bridled down in front by the sterno- and thyro-hyoid muscles, and behind by the middle cervical fascia, which adheres below to the clavicle, would have had, on its farther increase, a tendency upwards towards the ear, taking the same direction as the cases related by M. Laugier. Or again, fixed in its place by the aponeurosis in which it was encased, it might have exerted injurious pressure upon the parts placed behind it, and given rise to those disturbances in respiration, circulation, and deglutition, which have been noticed in certain cases by Fleury and Marchessaux.*

On the opposite side of the neck, and occupying an exactly corresponding position, an elongated, parenchymatous, lymphatic gland (about a sixth of the size of the cyst), was observed—no similar gland being discoverable posterior to the cyst. Higher up in the neck, too, near the lower border of the thyroid body, and attached to the external thyroid vein, an unenlarged lymphatic gland was found on each side, forming a symmetrical arrangement with the parts seated below, and at once conveying the idea that the cyst on the left side was originally one of these lymphatic glands. Lymphatic glands, placed over the trachea, between the thyroid body and the sternal fourchette, have been only very vaguely described by authors. Still, Mascagni has represented them in his magnificent Atlas ; and in five necks, examined for the purpose, M. Richard has found an identical disposition of four or six glands—often small ones, especially the upper— connected together, as well as to the other glands of the neck, by very visible vessels. M. Richard refers to an autopsy related by Mr. Cæsar Hawkins,† in which a chain of cysts replaced the cervical glands. He also refers to a case under M. Velpeau's care, in which, after the discharge of some fluid from a cyst, the remaining solid portion of the tumour proved, on extirpation, to be an enlarged and indurated lymphatic gland.

M. Lebert, reporting upon this case, alludes to the incorrectness of the term *hydrocele,* used to designate this class of tumours of the neck ; it being too indefinite when employed to designate a mere collection of fluid, and likely to convey an erroneous idea when suggesting any analogy between this affection and the hydrocele of the *tunica vaginalis.* He prefers the term *thyroidean cysts* for those of them which arise in the thyroid gland, and *extra-thyroidean cysts* for those that have no connexion with it. In respect to the transformation of a lymphatic gland into a cyst, he believes it may occur.

" The structure of lymphatic glands still offers many obscure points for consideration, and in order thoroughly to comprehend it, I have found it necessary to examine them from the rudimentary condition to the most advanced hypertrophy. I may here observe, that the hypertrophy of glandular organs often enables us to obtain a better idea of the structure and functions of these organs, just as comparative anatomy alone can enable us to determine the value of different anatomical elements of an organ, the incomplete development of which, in a particular group of the animal series, is compensated for by its more perfect evolution in other groups of animals.

" In very hypertrophied lymphatic glands, I have observed a disposition, which I had long suspected, but have always searched for in vain in normal glands. It is

* Archives Générales, 2 Sér. vi. 415 ; 3 Sér. vi. 269, 427.
† Medico-Chirurgical Transactions, vol. xxi.

the disposition in glandular granules, in vesicles having very thin walls, and lined internally with a layer of epithelium. Such an arrangement must remarkably facilitate the formation of cysts." (p. 49.)

M. Lebert has met with two examples, which proved in the most undoubted manner that true cysts may be found in lymphatic glands. In one of these, an enlarged and cancerous cervical gland contained in its interior a distinct cyst, filled with a yellow and transparent fluid. In the other, cavities containing liquid were found in several enlarged glands, which were extirpated in a case of cancroid of the lower jaw. Indeed, the transformation is probably far more frequent than is usually supposed : but M. Lebert is not of opinion that it has occurred in the instance now brought forward by M. Richard. Analogy is against it ; for we have no example on record of a gland being entirely transformed into a mere shell or envelope, as would be the case here. Unfortunately, the preparation was presented too late to allow the cyst and its contents to be microscopically examined, which, before a decision could be come to, would be indispensable. At present, the case presents a mere probability, from which nothing can be concluded. Indeed, from an examination of the histories of several of these extra-thyroidean cysts, M. Lebert can find nothing favouring the hypothesis of *their* glandular origin. On the contrary, the early detection of their fluctuation, the enormous size they may reach, and the fibro-vascular structure of their walls, all oppose the idea. He refers, in conclusion, to a case which occurred to himself, in which the cyst, occurring in the neck of a child, æt. 5, had reached the size of a walnut. As the child was very fat, and the tumour was deeply-seated, it had all the characters of solidity. On cutting down upon it, a transparent cyst projected through the wound, and a minute examination after extirpation proved that there was not a trace of glandular element present.

III. *On the Treatment of Aneurism by Galvano-Puncture.* By M. Boinet.

Several Italian and some French provincial surgeons have taken great pains of late to bring this operation into vogue, and its tardy adoption, or rather rejection, by the Paris and London surgeons, has been made a matter of reproach ; unjustly so, however, as will be seen from our account of M. Boinet's paper, which fully exposes the utter insufficiency of the evidence brought forward by the advocates of the operation. The occasion was a case forwarded to the Society by M. Vial ; and M. Boinet having to report upon this, examined into all other hitherto published results.

M. Vial believes that if the operation has not met with the success it deserves, this arises from the pain, inflammation, suppuration, gangrene, &c., which are due to a faulty mode of manipulation. He proposes that the galvanism should be applied for a much shorter time, and at a greater number of sittings, than is customary at present. The case he relates is certainly an extraordinary one, when we consider that it is brought forward as a successful one. It was an example of aneurism in the bend of the arm, which supervened upon a bleeding performed three months before, and had attained the size of an egg. Application of two needles connected with Wollaston's pile was made for six or seven minutes on the 8th, 13th, 18th, and 20th August, 1846 ; but so intense was the suffering caused, that the youth (æt. 20) refused to submit to it any more. The tumour continued

of the same size, but had become more firm. Permanent compression was applied to the brachial artery, and by the 18th September (twenty-eight days after the last application of galvanism), all pulsation had ceased in the tumour; this, too, having diminished to the size of a walnut. It now, however, became again the seat of a slight *bruissement*, which gradually increased, and by the 6th October, a marked pulsation was observed. By the 22nd December, this had again so diminished as hardly to be felt. The patient died of chest-disease, January, 1847. On examination, the aneurismal tumour was found to have walls two millimetres thick, and to be filled with half-solid coagula. The brachial artery communicated with its posterior part by means of a minute oval opening, whence proceeded two filaments of coagulated blood, one extending upwards in the brachial, and the other downwards into the radial artery. The arterial calibre and lining membrane were in their normal state. That this would have proved a radical cure, is thus highly doubtful; and even supposing it might have done so, the time required for its production (122 days) is infinitely longer than would be necessary if the case were treated by ligature or compression. Moreover, compression of the brachial, and the application of ice, long persevered in, had probably much to do with the production of the coagulum.

We will not follow M. Boinet in his criticism of all the other cases that have been published; but as M. Petrequin, of Lyons, a very able though rather too enthusiastic surgeon, is one of the warmest advocates of this operation, we cannot exhibit its baselessness better than by presenting abstracts of his own cases. The *first* was an example of aneurism of the ophthalmic artery, for which the carotid had been tied in vain. On the 19th July, 1845, galvanism was applied for a quarter of an hour by three needles. Severe pain was produced, and the subcutaneous tissue was ecchymosed. Ice was applied, the pulsation and *bruit* continued, when on the 14th August a violent fever carried the patient off. No autopsy. Here the operation was at all events useless, even supposing it had nothing to do with exciting the febrile action which proved fatal. In the *second* case, galvanism was applied by two needles for ten or twelve minutes on the 10th September, for the cure of a small aneurism of the temporal artery, occurring in a youth of 19. The pulsation completely ceased, and the tumour became hard. By the 12th September, all swelling had disappeared, and the temporal artery was obliterated above. The patient left the hospital on the 20th, and was last seen a week afterwards. As compression and cold were also applied above the sac, it is not certain that they may not have contributed to the cure, while, the patient being watched for so short a time afterwards, there is no proof that such cure continued permanent. In the *third* case, the points of four needles were intercrossed within the sac of an aneurism following bleeding. The tumour became hard, and the pulsation ceased in a quarter of an hour. Compression was maintained on the artery, and ice applied to the tumour. The sac became inflamed, and abundance of fetid pus was discharged by the apertures left by the falling of the eschars which had succeeded the acupunctures. The galvanism was applied on the 5th June, and by the 26th, all traces of the tumour had disappeared, and the brachial artery was pulsating naturally through its whole course. The patient was discharged twenty-nine days after the

operation; but as he was not seen afterwards, we are not able to say whether the cicatrix in the artery continued able to resist the impulse of the circulation in the vessel, so soon re-established. Five years have elapsed since the operation, and an account of the present state of the patient would be far more conclusive than the above early statement. Here again, ice and compression were also employed for fifteen days; while the violent inflammation set up in the sac may have had something to do with the result. In the *fourth* case, the galvanism was applied for a very large spontaneous popliteal aneurism, in a man æt. 65. The size and pulsation of the tumour had already been diminished by cold; and the galvanism applied by four needles, rendered the tumour quite dense and pulseless. Ice was applied, and improvement continued. The patient left for the country twenty-five days after the operation, having still a tumour, as large as a small egg, in the ham. Here coagulation seems to have commenced prior to the use of the galvanism; while of the ultimate result of the case nothing is known. In the *fifth* case, galvano-puncture was employed for aneurism after bleeding, with the effect of rendering it firm and destroying the pulsation. Compression was then applied, and the patient seemed in a fair way of doing well: but the last report only comes down to eight days after the operation. The *sixth* was also a case of aneurism of the size of an egg, occurring after bleeding. Compression had been applied to the brachial artery and to the tumour; and the skin over the latter had become thin, red, and adherent. On the 10th March, currents were directed through four needles for fourteen minutes, the tumour becoming hard, and the pulsation ceasing. The brachial was then compressed, and bladders of cold water were applied to the swelling. The cure was found to be complete, 20th April, forty days after the operation, the pulsation being normal throughout the course of the brachial, which at first had been obstructed over several fingers' breadth. No report of the patient's condition since he left the hospital being furnished, the durability of the cure is involved in doubt. Moreover, compression so long and forcibly maintained, probably influenced the production of the coagulation.

The most conclusive case on record is that related by M. Abeille. It occurred in the person of a woman about sixty-five years of age, who exhibited a pulsating tumour of the subclavian region, about the size of an egg. Four needles were kept in for thirty-seven minutes. The pulsation ceased, and the tumour became hard. Compression was maintained on the subclavian for ten hours afterwards. By the eighteenth day, the tumour was diminished by more than a half; and by the thirty-seventh, all projection had disappeared, a flat, deep-seated, oval body being alone perceptible to the touch. This patient was seen two years and a half after the operation, when the artery was found to be rather larger, and a dense, resisting body occupied the place of the tumour.

The following are some of M. Boinet's conclusions, after reviewing the published accounts of *thirty-two* cases:

" We have found 21 of these to have been unsuccessful; 10 successful; and as the result of the other is not indicated, it was probably not successful. Of these 32 aneurisms, 14 were due to traumatic causes, and the others were developed without appreciable cause. Of these aneurisms, 9 affected the brachial artery; 8 of these arising from venesection, and the other from contusion. If we examine the

results that have been obtained by the new method, we find that in 5 out of the 9 brachial aneurisms it is said to have been successful. But is such success real? The patients have left the hands of the surgeon too soon, and before he could decide, after a sufficiently long lapse of time, whether the cure would be maintained, and no relapse take place. We may also add, that in almost all these cases, success has been obtained as a consequence of inflammation and suppuration of the sac and the artery—an inflammation developed under the influence of galvano-puncture; and that in all of them ice and compression have been employed, for more or less time, concurrently with the galvanism. . . . .

" In the presence of these facts, can we or ought we to partake of the enthusiasm of some of our colleagues? Whoever will examine into them without prejudice, must perceive that the successes reported are cures which may be considered as real at the periods when the patients left the hospitals, but which require to be re-examined several years after, in order to determine how far they are permanent. The doubt which seems to prevail in the minds of all surgeons upon this subject, has arisen from these cases having been too prematurely published. On analyzing the cases and the observations of these authors, we have remarked that they have recorded what they wished to obtain, rather than what they have obtained; and that they write without having any proof, and under the guidance of mere inference. This is especially the case when they have to refer to any accident, which could never happen if such or such a condition were fulfilled—a condition which at a later period becomes realized, without leading to the promised and hoped-for results. The rules which should serve as guides are as yet not distinctly traced; and the surgeons who have tried this method are not at all agreed in their mode of employing it. . . . . .

" The most constant result that has been obtained, both by observations on man and experiments on animals, is the coagulation of blood in the aneurismal sac, and in the arteries submitted to galvano-puncture. This advantage would be invaluable if proved by numerous facts, and, above all, if we could procure it without inducing the numerous accidents we have enumerated. If this were so, it would remain to be inquired, whether for the radical and permanent cure of an aneurism, the blood must become coagulated at the same time in the sac and in the artery. It is at present a received principle, that the primary condition for obtaining a radical cure of an aneurism is an obliteration of the artery, to a certain extent, above and below the opening in its coats. But in the treatment by galvano-puncture, the result chiefly sought for is the restoration of the permeability of the artery. But is not this permeability a danger for the future? and does it not compromise rather than favour the success of the new operation? Is it not to be feared that the re-establishment of the circulation in an aneurismal artery, which has an ill-cicatrized wound, or still is in communication with a sac quite or partly filled with coagula, may not prove a remote or predisposing cause of the return or the maintenance of an aneurism, when on the road to recovery, or even cured? Such a result is much more to be feared than desired. If the obliteration of the artery above and below the sac is necessary and indispensable for the cure of an aneurism, and always takes place after ligature or compression, or a spontaneous cure; if we may always without any danger, determine coagulation in an artery, whatever be the calibre of the vessel, or the age and condition of the subject,— would it not be more rational to pass the needles into the artery itself, above and below the sac, rather than into the sac, which would then become obliterated, as after ligature or compression? Another fact, too important to be lost sight of, is, that in experiments upon animals, needles plunged into the arteries always promptly induce coagulation of the blood, without the production of inflammation or gangrene of the walls of the vessel. In aneurisms, however, the coagulation of the blood in the sac is often obtainable only with difficulty, and is perhaps only due to the inflammation excited by the electricity,—so that we may even inquire if such coagulation is not rather the result of inflammation than of galvano-puncture,

properly speaking; and whether the cures which have been obtained would have been procured without the secondary or indirect coagulation.

" It results from these facts, and the conclusions that may be deduced from them, that the important question of the treatment of aneurisms by galvano-puncture is as yet far from being cleared up, and can only be resolved by clinical experience, conducted within the limits of prudence. In the mean time, surgeons will consider this method as less certain than the employment of the ligature, and cannot propose it as deserving of equal confidence. It is only in certain desperate cases—in those in which the ligature, on account of the seat of the aneurism, is not practicable—that galvano-puncture may sometimes offer certain advantages. We can predict nothing as to the future of this procedure, since the published facts are neither numerous nor precise enough to enable us to form a complete estimate respecting it." (pp. 106—113.)

Since M. Boinet's Report has been read, three new cases of aneurism treated by galvano-puncture have been published. In one of these a cure seems to have resulted, but was accompanied by serious symptoms; while, in the two others, so alarming were the accidents produced, that the ligature was resorted to in all haste.

IV. *On a New Mode of Reducing Dislocation of the Thumb Backwards.* By M Demarquay.

In spite of the numerous investigations that have been made into this subject, the procedures for the reduction of the dislocation of the thumb, recommended even by high authorities, not unfrequently fail on trial. M. Demarquay having recently met with a case in which these difficulties were overcome by a very simple manœuvre on the part of M. Roux, he presents in this paper an account of the investigations which this led him to undertake.

A lady, æt. 30, while hastily descending from a carriage, fell upon the pavement with all the weight of her body resting on the extended right hand. A dislocation of the thumb backwards ensued, for which M. Demarquay was consulted about fifteen hours after the accident had occurred. The right thumb formed an obtuse angle with the corresponding metacarpus, the anterior extremity of which sensibly projected into the palm. All flexion and extension were impossible; the hand was swollen and violaceous, and the least touch caused great suffering. The patient having been brought into a state of anæsthesia, traction was made by Charrière's forceps, in the direction of the axis of the first metacarpal bone; and when the posterior portion of the phalanx was brought to the level of the meta-carpal surface, flexion was employed. This procedure, tried again and again, however, failed to accomplish reduction. After various other efforts had been made, M. Roux was called in, and he employed the same manœuvre as just stated; but prior to flexing the thumb, he imparted to it a *rotatory movement inwards*, and reduction was easily accomplished.

To obtain an explanation of this result, M. Demarquay repeatedly inves-tigated the subject experimentally on the dead body. When a luxation of the thumb backwards is produced, the terminal extremity of the first meta-carpal bone passes between the two bundles of the *flexor brevis pollicis*, a portion of the external border of which it often tears, and is to be felt just under the skin. It is now enclosed in a muscular " buttonhole" aperture, formed outwardly by the *abductor pollicis* and the external part of the

flexor brevis, and inwardly by the inner portion of the *flexor brevis*, the transverse *adductor*, and the very strong tendon of the *flexor longus pollicis*. The anterior ligament always breaks near the metacarpus; so that in from fifteen to twenty experiments, it has never been once found interposed between the articular surfaces. In many of the experiments, both the lateral ligaments, and, in nearly all, the external, have been broken. The difficulty in reduction does not arise from the interposition of the anterior ligament between the articular surfaces, as supposed by some, or from the persistence of the lateral ligament, as conjectured by others; but from the surrounding of the metacarpus by the fleshy " buttonhole" already described. The remedy is not the incision of this, as recommended by some authors, but the extricating the head from its grasp by the procedure adopted by M. Roux. The hand and forearm are to be placed between pronation and supination. The surgeon seizes the terminal portion of the phalanx of the thumb with the forceps held in his right hand, and exerts traction on the part, while with his left thumb and index finger he pushes the projecting metacarpus firmly backwards, taking care to prevent it from advancing during the subsequent flexion. When the extension has been for some time made in the direction of the axis of the thumb, a marked rotatory movement inwards is to be effected, which will disengage the metacarpus from the external band of muscles that surround it, and enable them to slide in front of its head. This done, the tractions are to be continued until the posterior part of the phalanx is brought to a level with the head of the metacarpus. Flexion of the thumb is then to be performed, at the same time that with the thumb of the left hand the displaced metacarpal bone is forced backwards.

Besides the papers we have noticed, this part of the ' Mémoires' contains a brief eulogium on Marjolin, by M. Monod ; and an essay on " Aneurismal Varix," by M. Monneret. As this last paper is entirely occupied in the physical diagnosis of the affection, as deduced from certain doctrines upon the production of abnormal sounds held by M. Monneret. but not elucidated here, we pass it over; and the more readily, as M. Marjolin, jun., who reports upon it, is completely at issue with the essayist.

Art. IX.

Lectures on the Principles and Practice of Midwifery. By Edward W. Murphy, A.M., M.D.—*London*, 1852. 8vo, pp. 606. Illustrated by Lithograph Plates and Woodcuts.

Dr. Murphy informs us, in his preface, that he has published this work in answer to a demand made by his pupils for a book to read with his lectures ; not being able, in the present controversial condition of obstetric literature, to recommend for that purpose any existing treatise, in which principles are expounded with sufficient clearness, and the rules of practice laid down with decision. There is some exaggeration in this statement ; for, with the exception of a few controverted points, several of which have been raised only of late, and await the accumulation of facts and observations for their decision, the conduct to be pursued in the various emergencies of obstetric practice is stated with sufficient unanimity in the various standard works.

to enable the student to determine upon his line of procedure without much difficulty. The errors which are usually committed, are rather due to inattention to all authorities, than to hesitation in deciding between contending ones. Moreover, although we freely admit the ability and impartiality with which Dr. Murphy has discussed these controverted points, and agree, in the main, in the conclusions he has arrived at, we are far from thinking that he has set them at rest. In point of fact, he required no apology for presenting the medical public with a new work. A distinguished disciple of the Dublin school, a practitioner of considerable eminence in the metropolis, and a teacher in a school of medicine second to none in importance, his opportunities have given him a claim to attention, such as is always most willingly accorded by the profession.

We must caution our readers, however, that if they expect to find in the present work a complete Treatise on Midwifery, they will be much disappointed ; the subjects of parturition and puerperal diseases being alone embraced within its scope. Why the author only publishes a portion of his lectures, and whether he intends hereafter to put forth the remainder, he furnishes no information ; but when it is stated that such important subjects as the signs and diseases of pregnancy, the development of the uterus and of the fœtus, abortion, and extra-uterine fœtation, are entirely excluded, it will be at once seen, that as a guide to the student and young practitioner the present work is very defective. Even some of the subjects adverted to,— as, for example, the induction of premature labour and obstetric auscultation,—are treated in a most superficial and parenthetical manner, quite incommensurate with their importance. We hope, indeed, when the present impression is exhausted, that Dr. Murphy, in place of republishing literal reports of these or any other of his lectures, will present us with a complete and substantive work on midwifery. This is, no doubt, what his students wished for ; and to publish for them copies of the lectures to read *with* these same lectures, was at all events a singular procedure. The consequence will frequently be, that they will neglect to attend lectures which they think they can read at any time, this ' any time' often proving no time at all. A work of this kind, too, would enable Dr. Murphy to render his lectures more graphic and demonstrative, and less discursive : for certainly some of the historical details given in the present volume—in regard to puerperal fever, for example—must prove wearisome to hear, however profitable to read, and occupy time that might be more advantageously disposed of. We feel convinced that the system of lecturing is overdone at the present day, and that the student has not always gained by the substitution of the encyclopædiacal style of the present race of teachers for the more dogmatical and demonstrative procedures of their predecessors. What with the multiplicity of subjects, and the inordinate length at which these are often treated of, the student's entire time and attention are absorbed in the lecture-room ; and the labours of the closet, the cultivation of his power of thought by communing with the master-spirits of all periods, are too much neglected.

To return from this digression, we may observe, that we have been much gratified by the tone and temper with which Dr. Murphy discusses the various topics embraced by his lectures. There is an impartiality in weighing evidence, and a candour towards opponents, to which we have

unfortunately been too little accustomed in the metropolis of late.   Never-
theless, it will be found, that he does not hesitate to bestow approval upon
some innovations of practice that have been stigmatized in terms which we
care not to repeat, and the employment of which in discussions that should
have the advancement of science for their object, we hope the improved sense
of the profession will not much longer tolerate.   Autocracies in science,
founded upon the mere *prestige* of the past, will soon become impossibilities ;
and although progress may be opposed and temporarily delayed by appeals
to ignorance and prejudice, it is not the less inevitable.

It is not our intention to follow Dr. Murphy in detail through the thirty-
three lectures now published ; but we shall confine our attention to the
points in which his views differ from those generally entertained, or are
exhibited in a manner somewhat dissimilar to that found in other authors.

The Masculine Pelvis in Women.—After describing the distinctions
between the male and female pelvis, Dr. Murphy goes on to explain the
consequences which may ensue, when the habits of life of the woman confer
on her pelvis a more or less masculine configuration.

" It is true that the uterus is here contained, and therefore we cannot explain
by its absence any narrowness of the pelvic cavity which may exist ; nevertheless,
if we suppose the muscles connected with the pelvis to be large, strong, and con-
stantly exerted, the effect would be nearly the same ; the acetabula would be
drawn closer to the centre ; the planes of the ischia would converge more,—not,
as in the diseased pelvis, protruding into the cavity, but by the bone in its growth
adapting itself to the diminished distance.   In a similar manner, the ilia would be
more upright, and the pelvis of the female would gradually assume many of the
characters of the male.   Such is frequently the case with women in the rural dis-
tricts, who are strong, healthy, and constantly employed from early youth in
carrying weights, and in other active muscular exertions.   The difficulties offered
by such a pelvis are altogether different from those of a diseased pelvis, which, if
you study what is written on the subject, would seem to be the only source of all
the embarrassments that are met with.   The triangular shape of the brim is not
generally an impediment, because, although the transverse measurement is dimi-
nished, the oblique is sufficiently wide, and the head will usually pass into the
cavity.   But here all the difficulties seem to centre.   Anteriorly, the symphysis
pubis is narrower and more unyielding ; even a deposition of bone is sometimes
found behind it, which may be extremely dangerous if the intervening soft parts
are pressed against it by the head.   Posteriorly, the promontory of the sacrum
offers no opposition ; but the sacrum itself being straighter, there is less facility in
the head performing the lateral rotation ; and this difficulty is still more increased
by the convergence of the ischio-pubic rami : the head is obliged to descend much
lower in the pelvic cavity before it can escape under the arch of the pubis, and is
prevented from doing so in consequence of the space being so much lessened by its
funnel-shape.   In addition to this, the tubers and spines of the ischia are more
ossified,—the one thicker and rougher, the other larger and more projecting ; thus,
as the head advances, its passage becomes more and more impeded, until it is
ultimately arrested, perhaps close to the outlet.   In women of this description, it
is possible, also, that the head of the child may be more than usually ossified, and
the action of the uterus is always strong ; so that a most disadvantageous com-
bination of circumstances may take place in a healthy pelvis of this kind." (p. 28.)

And again :

" You perceive, therefore, that two cases which present precisely the same
apparent amount of resistance to the passage of the head, may be perfectly opposed
in every other respect, and consequently that the same treatment, if applied to

both, might be mischievous to either. It is essential to point this out to you, because rules of practice are too often laid down by authors as being applicable indifferently to all cases which present the same amount of disproportion; and hence a great deal of useless controversy has sprung up as to those rules, in consequence of the opposite experience of the disputants: for example, if the question be as to the rule when instrumental aid is called for. The practitioner who meets with the diseased pelvis as his chief cause of difficulty, and living perhaps in a manufacturing town, or other unhealthy district, such a practitioner would find that he could not suffer his patients to continue in labour beyond a certain period, without great hazard to her life and to his own reputation. He would also have it in his power to deliver his patient with the forceps successfully, where the disproportion was such, that had it been the masculine pelvis he would have done irreparable mischief. It is not surprising, therefore, that his opponent, accustomed to a healthy district, and meeting with cases where the chief difficulty arises from a pelvis of the latter kind, should wonder at and protest against such temerity." (p. 128.)

Positions of the Head.—In order to exhibit the number of these, and the order of their occurrence, as adopted by various authors, Dr. Murphy furnishes the following tabular view of them:

Left occipito-cotyloid.	Right occipito-cotyloid.	Occipito-pubic.	Left fronto-cotyloid.	Right fronto-cotyloid.	Fronto-pubic.	Left occipito-iliac.	Right occipito-iliac.	
1	2	3	4	5	6	...	...	{ Baudelocque, Dewees, &c.
1	2	...	3	4	...	5	6	Lachapelle.
1	3	...	2	4	...	...	...	Naegele.
3	4	7	5	6	8	1	2	Ramsbotham.

Nearly all authors are agreed that the most usual position for the head to pass, is with the occiput corresponding to the left plane of the ischium. Dr. Ramsbotham gives precedence to the transverse position of the head, believing the occiput to be more frequently opposite the ilium than the plane of the ischium. In this case, the ear should be felt directly behind the pubis, in place of the right or left groin. According to Dr. Murphy's experience, the head is most frequently placed in the left oblique occipito-cotyloid position, but not so much so as to remove the ear from the neighbourhood of the pubis. The result of his observations in the Dublin Lying-in Hospital is also confirmatory of the general accuracy of Naegele's statement, that the head does not enter the brim in Baudelocque's second position, but, having first passed down in the third position, becomes gradually rotated as it descends into the second. In nearly an equal number of the cases examined, the head entered the brim in the third as in the second, the majority of the former passing without difficulty into the second, and being so expelled, while a very few remained in their original position. The fourth position also passes into the first, although in very rare instances it preserves its original direction. In seventy-four cases, still more recently examined, the following results were noted:

First position.	Fourth into first.	Second position.	Third into second.	Third position.	Irregular.	Face.
43	3	11	9	2	4	2

Obstetric Duties of the Practitioner.—Dr. Murphy's chapters on this part of his subject contain much sound advice. We agree with him in the necessity of promptitude of attendance; but we must not forget how often the conduct of the patient renders this impossible. Neglecting all premonitory signs, the practitioner is often not summoned until the last moment, when he may be elsewhere engaged; or, on the other hand, he has been so needlessly hastened on former occasions, in certain cases, as to render some delay excusable hereafter. In the majority of instances, it will be found that the fault of the practitioner not being in attendance rests with the patient herself, or her attendants; for he is too well aware of the exaggerated importance that is attached to his presence during the instant of birth, and the danger of being superseded by another, not to do his best to be at hand at the time. If the services of men whose time is at all engaged are to be secured, the patient must be prepared to take a little trouble to this end; and the plan we have now adopted for a great number of years, has been found to work very well. On engaging to attend a patient, we supply her with two cards having her address written on the back, the one being headed, " First Message," and the other, " Immediate." Her instructions are, to forward the first of these as soon as preliminary symptoms manifest themselves, or are even suspected, employing the other in case the labour sets in urgently without such symptoms, or if she thinks her first message is not attended to with sufficient promptitude. In this way the practitioner may learn what the state of his patient really is, far better than he can do from any verbal message; and may either repair to her instantly, or after a period of time, as the case may be.

In one point we are entirely at issue with Dr. Murphy—viz., the necessity of making an *early* vaginal examination He objects to it, on the ground that any irregularity of presentation that may be, or supposed to be, then detected, can only admit of rectification at a later period. Admitting this willingly, we still think that the examination should be instituted at as early a period as possible, in order to ascertain the exact stage of the labour. We have met with cases, in which, judging from the urgency of the pains, and the woman's feelings, delivery seemed imminent, and yet has been postponed (under the beneficial employment of an anodyne, which no one would have administered until assured by an examination of its necessity) for a fortnight or a month; and others, in far greater numbers, in which an examination revealed a state of forwardness of the labour which the insignificant pains that had occurred were far from indicating, and thus prevented a departure that would have been willingly allowed by the patient. So often has this been the case, that we now make it a rule never to leave a patient without insisting upon this point; and very few have been the instances in which she has declined to comply with a request made with delicacy, and so obviously for her own interest. We think the practitioner should always thus assure himself of the necessity of his presence; and having done so, however long this may be required, he must resign himself to it, not only with patience, but with cheerfulness; for to let any sign

of impatience, regret, or weariness, escape him, is not only a great act of cruelty towards the unfortunate cause of his detention, but militates against his own interests, inasmuch as nothing tends to aggravate delays depending upon other than physical causes, so much as the mental irritation engendered in the parturient female by the impatience of her attendant. For this reason, he should always be provided with materials for reading or writing, so as to have some other occupation than the constant watching and examining of the patient. She feels herself more at ease, and the labour often makes more rapid progress, than if she were more exclusively the object of his attention.

The following are Dr. Murphy's directions for *supporting the perineum.* We have always found it more convenient to employ the left hand for this purpose, as recommended by Ramsbotham and Rigby; but we must confess ourselves to be somewhat sceptical of the power of this or any other procedure, to prevent rupture of the perineum in certain cases, however carefully it may be employed.

" The plan which I have found the most useful and convenient to adopt at this period of labour is the following :—To sit behind the patient as she lies on her left side, the back of the chair being towards the head of the bed, and while the child is passing through the pelvic cavity, to press moderately with the left hand over the hip of the patient. Counter-pressure in this way employed is generally grateful to her, and seems to give some relief : it assists also in keeping the pelvis fixed while the head is passing the perineum, the most important part of this process. Having the left hand so employed, the right can be used to support the perineum. A single fold of a fine napkin should be placed along the edge of the perineum, and the right hand so applied, that the fold of skin between the forefinger and thumb should correspond to this, the forefinger and thumb passing on either side of the vulva, and the palm of the hand resting against a thicker fold of the napkin, applied to the posterior part of the perineum. By this means you have full power to make any counter-pressure with the palm of the hand which may be necessary, and the fingers being quite close to the edge of the perineum and vulva, you can easily trace the margin of the perineum, and feel the head, if necessary. Thus one hand fulfils the office generally assigned to two, and enables you to grasp with the left hand the pelvis, to prevent the patient moving away too suddenly when severer pains come on. If, the head being expelled, this be no longer necessary, you can employ the same hand to support the uterus during its contraction in expelling the body of the child. Besides these advantages, it is certainly less fatiguing. The only inconvenience of this method is, that when the funis is coiled round the neck of the child, so as to make it necessary to remove it, or that the delivery of the shoulders should be assisted, the hands must be changed, that the left may support the perineum, and the right make the required correction. But this is a temporary disadvantage, and only arises occasionally." (p. 95.)

We are glad to find Dr. Murphy treating the *application of the bandage* after delivery as a matter of much greater importance than Dr. Ramsbotham considers it. The latter practitioner recommends this being left to the nurse, on account both of the little importance he attaches to its use, and of the indelicate character of the operation when performed by the accoucheur. We will venture to say that no one who has examined a bandage a quarter of an hour after it has been applied by an ordinary nurse, will feel satisfied with its adjustment; and all appearance of indelicacy is obviated by applying it over the body-linen of the patient. Dr. Murphy regards it of especial use in those subjects in whom the weakened condition of the abdominal muscles prevents their giving the uterus sufficient support

as it becomes emptied of its contents, and exposes the patient to the risk of the organ relaxing again, and pouring out blood to a dangerous extent. Another object to be gained by the bandage, is to support the pelvic articulations which have been exposed to extension during the labour; and the sense of comfort and strength thus derived, so strongly expressed by most women, is a much more reasonable explanation of their desire for the application, than the anxiety for the preservation of the symmetry of their form, attributed to them by Dr. Ramsbotham. Dr. Murphy directs that the bandage should embrace the entire abdomen, from the pelvis to the diaphragm, pinning the pelvic portion much more closely than the upper portions; but we believe it will be rarely found that the bandage will remain *in situ,* unless the interval between the two ilia be filled up by two or three napkins folded into a broad pad.

Compression of the Anterior Lip of the Uterus against the Pubis by the Head of the Child.—Dr. Murphy makes the following observations upon the management of this cause of delay in the progress of labour:

" A band is thus formed before the head, which, when long pressed upon, is swollen, tender, and rigid. The treatment of the cervix, when in this state, has become a kind of *vexata quæstio* in obstetric practice. Some practitioners of station and experience have advised that the anterior lip of the os uteri be pushed up by the fingers above the head, in the interval of the pain, and there maintained until the returning contractions of the uterus drive the head below it; while others of equal reputation deem such practice to be objectionable, and calculated to increase rather than diminish the difficulty, by exciting more inflammation. I may mention the late Dr. Hamilton as holding the former opinion; Dr. Collins, the latter; as a proof how men of very extensive experience often arrive at opposite conclusions on what would seem to be a simple practical point. I must assume, on the evidence of Drs. Hamilton, Burns, and Breen, that this kind of artificial dilatation may be accomplished in some instances with safety and success. My own experience, however, confirms that of Dr. Collins, and is opposed to this practice; the opportunities I have had of putting it to the test have taught me that success is by no means so easy as it is described to be; that the anterior lip may be pressed back again and again, and yet return to the same situation as before; that it is difficult to get the head to pass the introduced finger; and that these attempts, when unsuccessful, only increase the swelling and inflammation of the soft parts. I have also met with instances, in which, after a failure of this kind, the anterior lip was retracted without any assistance, when the contractions of the uterus succeeded in at length altering the direction of the head. It appears to me, therefore, that this kind of manipulation may be employed, and would be serviceable, if the fingers were placed against the head of the child, in order to relieve the constriction of the anterior lip, and to direct the head more towards the pelvic cavity. I am still, however, disposed to object to the practice of artificial dilatation of the mouth of the uterus for the former purpose. While on this part of our subject, I must apprize you of a deception which may be produced by certain states of the os uteri. The anterior lip is sometimes hypertrophied, and projects so much before the head, that it seems to be the cause of difficulty. It is scarcely necessary to say, that you will employ your time very fruitlessly if you attempt to push back this hypertrophied portion of the os uteri." (p. 117.)

Delay in Labour from the Head of the Child being too large and too much ossified.—Dr. Murphy's experience at the Dublin Hospital enables him to confirm the truth of the statement of Dr. Simpson, that the danger from parturition is greater, both for mother and child, in male than in female births. Among 5699 patients there delivered, there were 213 cases

in which labour extended to or beyond twenty-four hours. Of 218 children born, 126 were boys, and only 92 girls, or in the proportion of 3 to 2. Of the boys, 46 died; of the girls, 35. Professor Simpson attributes the disparity to the greater size of the head at birth in male infants, Dr. Joseph Clarke of Dublin having already made the same remark. Dr. Murphy believes that not only is the size of the head larger in these, but that the ossification of the bones is more advanced, and their power of yielding less. The head of the male child is rounder, and the fontanelles are smaller, the head of the female being more oblong, and the fontanelles more distinct. Such a head will usually pass slowly and with difficulty, even through an average pelvis; but when the cavity becomes diminished in size, or altered in its normal character, the difficulties become dangerously increased. In Dr. Murphy's experience, this state of the fœtal head is often met with, when the pelvis itself is also too much ossified and assumes the masculine characteristics already adverted to.

Question of Instrumental Interference in Laborious Labours.—Dr. Murphy endeavours to deduce from the examination of statistical facts, the rules that should guide our practice in those cases in which the head, without being actually arrested, yet advances so slowly as to seem to be so. In 75,911 cases of midwifery occurring in British practice, there were 138 forceps-cases, and in 35 of these, or 1 in every 4, the child was still-born. In France, 173 forceps-cases furnished 41 still-born children; and in Germany, 2808 furnished 650—so that it may be laid down as a rule, that nearly one-fourth of the children delivered by the forceps are lost. In order to determine how these protracted cases fare when left to themselves, Dr. Murphy refers to Dr. Collins's tables, as the only data which from their accuracy and minuteness afford the elements of calculation—these exhibiting the duration of the labour in all the cases reported. From these tables, it appears, that of 430 cases in which labour lasted or exceeded twenty-four hours, 324 were delivered without assistance, the children being lost in 61 cases, or about 1 in 5. Among the 5699 cases that fell under Dr. Murphy's own care, 218 were similarly protracted, and 175 of the number were delivered naturally, 41 children, or 1 in 4 nearly, being still-born. Thus, so far as the children are concerned, the proportion still-born is very much the same, whether the forceps be employed or not, the difference, if any, being in favour of leaving these cases to nature. With respect to the mother, the experience of Drs. Collins and Murphy leads to the conclusion, that a mortality not greater, but probably considerably less, attends the non-interference practice, as compared with the results of forceps-cases that have been recorded. If these conclusions are sound, it is obvious that the use of instruments is to be discountenanced in all but exceptional cases of this kind, in which the habit of the patient is too feeble to admit of her enduring a protracted labour without risk of exhaustion. The discrepancy between English and foreign practice may be judged of by the fact, that while Ramsbotham employed the forceps once in 729 cases, Joseph Clarke once in 742, and Collins once in 684, Kilian resorted to them once in 78, Carus once in 14, and Siebold once in 9 cases. Dr. Murphy's recommendation is to employ the forceps only in cases of positive arrest, refraining if any advance, however slow, is made, unless dangerous constitutional symptoms are present. When the arrest amounts

to positive impaction, perforation is to be preferred; but when there is sufficient room to admit of the ear being felt, or for the fingers to be passed easily between the head and the pelvis, and the head has made no advance whatever for *four hours*, he considers the forceps should be resorted to.   In the event of pain, swelling, or heat of the passages manifesting themselves, further delay becomes unjustifiable.   He criticises Dr. Ramsbotham's indications for interference, founded, as they are, upon the actual presence of inflammatory action with irritative fever and impending exhaustion; and considers that the practitioner would not be justified in allowing the case to proceed to this dangerous extent, and that if only called in when it had done so, the perforator, not the forceps, is the instrument that should be resorted to.   In respect to the choice to be made among the numerous varieties of forceps, Dr. Murphy makes the following judicious observations:

"But, in commencing your career, we must advise you to avoid all complications in instruments—the simpler they are, the safer will they be in your hands.   For this reason we prefer forceps of the latter class, such as have been used by Smellie, Denman, and others, who employ the straight forceps, with the blades narrow, and the fenestræ pyriform.   These can always be easily introduced or withdrawn: the handle is a perfect guide to the direction in which the blade is passing, and the only point which it is necessary to secure, is, that the blades do not slip when extraction is made.   This cannot take place if the blades be properly curved; their curvature is one of the most essential points in the structure of these forceps.   Their extremities should be sufficiently close to retain their position on the head without bruising the face, and, at the same time, the space between the blades should be sufficiently wide to prevent much compression of the cranium.

"Experience can alone decide between the merits of similar instruments on a point of this kind; and having used several of these forceps, that which I have found to be the best, and one which I am disposed to recommend to you, is one used by Dr. Beatty, Professor of Midwifery to the College of Surgeons in Ireland, and described by him in the 'Dublin Journal,' vol. xxi. . . . . . These measurements are those given by Dr. Beatty, but the instrument which is in my possession, and which I have found so useful, varies a little from this.   The entire length is $11\frac{1}{3}$ inches; the handle $4\frac{1}{2}$ inches; blade 7; greatest breadth between the blades 3 inches; between extreme points $1\frac{1}{3}$ inch.   To this instrument I have added a shank about 1 inch in length, when it is necessary to introduce them, high within the pelvic cavity.   It is right to state, that the length is taken in a direct line from the extremity of the handle to the extremity of the curved blade.   This instrument is well calculated for that operation, which I have described as being intermediate between the operations with the long and short forceps—viz., when the head is in the cavity of the pelvis, without touching the perineum.   When the head is resting on the perineum, a shorter forceps would be more convenient; for instance, that of Dr. Collins, which is about 10 inches in length.   But when the head is fixed in the brim, the forceps of Dr. Radford, having unequal blades, appears to be the most suitable.   In giving you this account of an instrument of so much importance as the forceps, we wish to avoid leading you into what seems to us to be a great error—viz., a belief that by mechanical skill in the construction of the instrument, a great deal more can be accomplished by it than what is really the case.   Hence, every practitioner has his favourite forceps, and no little boast is sometimes made of the deliveries accomplished by it.   But we would beg of you to remember that Smellie and Denman, with their smaller forceps, had as much success as Davis and Hamilton with their more ingenious instruments, and that the success of an operation depends much more upon the hands that use the forceps than on the instrument itself." (p. 222.)

Spontaneous Evolution.—While agreeing with Gooch, Ramsbotham, and most other writers, in preferring, as a general rule, Douglas's to Denman's explanation of this occurrence, regarding the action that takes place rather as a spontaneous *expulsion* than *evolution*, Dr. Murphy, from his faith in the fidelity of Denman's observations, and from what he has himself witnessed, feels persuaded that this evolution does sometimes occur:

" I have met with cases where the arm presented, and occupied the os uteri completely, but afterwards it retreated, and the breech descended in its place. . . . . I think that it is very likely, when the child is full-grown and living, that the shoulder, in the intervals of the pains, might gradually leave the pelvis, if the body was forced down into it by the action of the uterus." (p. 292.)

As the rule for interference in these rare cases, he recommends, when the action of the uterus is powerful and the body is advancing, that the case should be left to nature, taking great care to support the distended perineum. When the process is retarded, he would pass a blunt hook over the body of the child above the pelvis; but no attempt should be made to push back the child in order to get at the feet.

Uterine Hæmorrhage.—This part of his subject is treated with great ability by Dr. Murphy. He prefaces its consideration by a description, illustrated by some good lithographs, of the modern researches into the uterine and placental circulation; and indeed, the present is the only work on midwifery in which these are completely set forth. He says:

" On a question of so much difficulty, and one which has been so much misunderstood, I have preferred giving you the description of the best and most careful observers, rather than my own—to adopt their language, as being least likely to be disputed. These quotations are sufficient to prove that there is a portion of the placenta in direct communication with the uterine vessels, which has been described by Weber as a rete of colossal capillaries; by John Hunter, as a cavernous structure; and by Goodsir, as a great cavity everywhere traversed and intersected by filamentous prolongations of the lining membranes of the uterine veins; that the maternal blood is impelled through innumerable uterine arteries into the great cavernous cavity of the placenta, and, having supplied the necessary nutriment to the fœtal blood, flows back through the large oblique canals that communicate with, or are part of, the uterine veins; that these venous canals and the cavernous structure are composed of a tissue of extreme delicacy; and lastly, that there is no direct communication between this maternal circulation of the placenta and that going forward in the fœtus. What, then, would be the effect if this vascular connexion between the placenta and the uterus were broken through ?—if the placenta were partially separated from the uterus ? From the nature of this injury the torn curling arteries might not pour out much blood. Any hæmorrhage must arise chiefly from the broken veins, and not, recollect, from one, but *from both* of the divided extremities. There are thus two sources from which blood escapes—1st. From the openings that communicate with the rete of colossal capillaries, by which the cavernous structure is emptied of maternal blood, to be again filled by the uterine arteries. This may therefore be considered as arterial hæmorrhage of the uterus through the placenta. 2nd. From the venous orifices on the surface of the uterus. The maternal blood flows from both extremities of the divided veins : in the former instance, in a direct current from the uterine arteries through the cavernous structure; in the latter, by regurgitation from the veins of the uterus. You can understand, therefore, not only the possibility that the certainty of the fact which the late Dr. Hamilton stated many years ago— which Professor Simpson has since, with so much ability, confirmed, but which still remains an enigma to perplex some writers on the subject—you can under-

stand why, when the placenta is partially detached from the uterus, blood flows from its denuded surface, and that the exposed uterine veins (or sinuses, as they are called) are not the only sources of uterine hæmorrhage. When that viscus is completely separated from the uterus, and its connexion with these arteries broken off, you can also perceive that blood will no longer flow from the surface, and the only blood that can then be expressed from it is the residue lodged in the cavernous structure." (p. 305.)

In cases of hæmorrhage from *partial* detachment of the placenta, its arrest is to be looked for from the coagulation of blood in the cells of the cavernous structure, and the closure of the venous orifices by the agency of uterine contraction. When hæmorrhage supervenes upon the *complete* detachment of the organ, its chief source is the venous openings of the uterus, and our object is to induce uterine contraction. By the aid of this, the semilunar or falciform projections, formed by the lining membrane of meeting veins, "are converted into valves, and where numerous short trunks intersect each other, the decussation of these valves completely closes the communication between the vessels." To this explanation of the arrest of hæmorrhage it is objected that there may be relaxation of the uterus without hæmorrhage, and hæmorrhage during firm contraction. Dr. Murphy observes, in reply, that while hæmorrhage *must* be the consequence of *complete* relaxation, it need not occur in the very common case of *partial* relaxation. This arises from (1), a slight degree of contraction sufficing to raise the falciform valves and partially to close the communication between the different trunks, and at least to retard the regurgitation of blood. (2.) "The current of the uterine circulation is altered in direction and in force. The arterial blood is no longer drawn towards the cavernous structure of the placenta, but flows into the intercommunicating branches in the parietes of the uterus. The current of the venous blood is directed much more rapidly towards the great central trunks of the abdomen, because these vessels are now relieved from the pressure of the gravid uterus, and by their expansion the venous blood is drawn more strongly from the terminal branches towards the central canals." (3.) The venous openings are frequently filled-up by fragments of decidua, by broken tufts of fœtal vessels or small coagula, which act with sufficient power as plugs when the circulation is not directed strongly towards the uterine surface. As to the cases of hæmorrhage occurring in spite of firm contraction, these may arise from retention of a portion of placenta, slight laceration of the os uteri, and analogous circumstances—cases which are clearly exceptional, and in nowise contradictory of the all-important axiom, that *contraction of the uterine fibres is the essential means of arresting uterine hæmorrhage.*

Dr. Murphy's general remarks on the treatment of hæmorrhage need not detain us; but his observations upon the employment of opium and ergot are interesting. In respect to *opium,* he employs it largely—and we think wisely so— in midwifery practice; and he remarks that the hesitation which some practitioners manifest in resorting to it in hæmorrhage, under the idea that its sedative action may prevent uterine contraction, arises from their overlooking the condition of the nervous system. When the nervous irritability is unimpaired or increased, a very small dose of opium would stimulate, and a larger one would exhibit its sedative effects; while, when the irritability is destroyed, and the uterus suffers from atony,

the large dose only acts as a stimulus, and the sedative power is only manifested when the nervous energy is restored. Thus, in extreme flooding, half a drachm of laudanum may be given again and again without producing any effect; but in proportion as the nervous influence becomes re-established, the sedative effect of the drug is observed. When the hæmorrhage is not sufficient to impair the tone of the uterus, a large dose may, by the exertion of sedative influence, prove dangerous; but when the loss is great, the very same dose will act as a stimulant, and induce contraction of the organ. The cause of the failure of *ergot* in extreme flooding also may arise from its misapplication; since, in order that it may act on the uterus, the nervous system must retain a certain amount of irritability. When this is exhausted, opium is the most powerful stimulant, and ergot is inefficient; while the specific action of the latter is at once manifested, if employed before exhaustion is produced or after it is removed. " Opium is therefore of the highest value in saving a patient from the consequences of extreme flooding; ergot, in preventing such flooding from taking place. Both remedies may be used in the same case, but one can never supply the place of the other."

In treating *unavoidable hæmorrhage,* attended with exhaustion, Dr. Murphy very much prefers having recourse to Dr. Simpson's plan of artificial separation of the placenta, to turning; and thinks this last operation is quite unjustifiable, on Smellie's plea that the woman must not die undelivered—the very attempt to prevent her doing so being, in fact, sometimes the cause of her death. His rules are thus summed up—1. In cases where no exhaustion has taken place, or where this is only commencing, the child should be turned and delivered the moment the os is sufficiently dilated or dilatable. When this is not so, the placenta should be compressed by the plug and by the discharge of the liquor amnii, and other means employed to prevent exhaustion, until delivery is practicable. 2. In extreme exhaustion, turning should not be performed, but the placenta should be separated, and the child left undisturbed until decided reaction takes place. 3. When the os is rigid, means should be employed to compress the placenta and increase the action of the uterus, so as to give time for the dilatation and turning. Should exhausting hæmorrhage, however, come on in the meantime, the placenta should be removed, rather than the hand be forced into the uterus. Adverting to some additional examples of spontaneous expulsion of the placenta taking place with advantage to the mother, Dr. Murphy observes:

" These cases are only small additions to a large number already recorded by Dr. Simpson, and several other practitioners, all proving that Nature, of her own accord, sometimes succeeds in separating the placenta from the cervix, the effect of which is, the cessation of hæmorrhage, and generally the recovery of the patient. We have seen, also, that even the child may escape under such unfavourable circumstances. If, then, it be true that the natural separation of the placenta arrests hæmorrhage,—if it be consistent with our knowledge of the structure of the placenta and the utero-placental circulation that such should be the case,—and if turning the child is proved to be a most dangerous operation in cases attended with extreme exhaustion, is it not reasonable to think that the *artificial separation* of the placenta in these cases—a much less serious operation, a mere imitation of nature—would be a justifiable practice, and one that hardly merits the very severe castigations it has received? The objection may perhaps occur to you, if

separation of the placenta be safer than turning, why not always do so ?   We would reply, that although safer for the mother, it is destructive to the child ; and our practice must be guided by the same principles in this as in other obstetric operations : if it be possible to save both mother and child by turning, to do so ; but if we have any doubt about the mother's safety, not to hesitate one moment because of the child.   Some practitioners will not scruple to destroy the child with the perforator, when there is a doubt about the mother's safety.   Why, then, should we hesitate, in the present instance, to sacrifice the child. if we are satisfied that the separation of the placenta will arrest the hæmorrhage and save the mother, especially if by so doing we avoid exposing her to the shock of so dangerous an operation as turning in extreme exhaustion ?   It is no reply to this argument to say, that some women have been thus delivered in the last stage of exhaustion, and have escaped.   I only ask you to examine, as I have done, the records of midwifery practice, to find the number who have not escaped, but who have lost their lives through this operation." (p. 345.)

Puerperal Convulsions.—Dr. Murphy treats of this class of affections under the subdivisions of Sthenic or Hyperæmic, Asthenic or Anæmic, and Hysterical Convulsions—a very practical arrangement, as indicating the opposite conditions of the system in which convulsive phenomena may be exhibited, and the very different treatment which these require.   He exhibits at some length the characters which distinguish these convulsions from epileptic, with which they yet have so striking a resemblance ; and his experience accords with that of most other practitioners, in establishing the rarity of puerperal convulsions in persons liable to epilepsy.   He also believes Dr. Ramsbotham to be in error in describing the hyperæmic convulsions as a form of apoplexy.   They may occur without any apoplectic symptoms taking place, and may terminate fatally without exhibiting any post-mortem signs of its occurrence.   It is true that in certain cases apoplexy may supervene and complicate the case, and render it far more dangerous ; but the two affections are essentially distinct.   Dr. Murphy states that he is disposed to doubt whether the frequency of albuminuria in this affection is so great as is stated by Lever and Simpson ; but if he has no more certain evidence of its absence than is conveyed in this passage—" In the cases of puerperal convulsions that I have met with, the majority were strong, healthy young women, without any œdema, who did not impress me with the idea that they were labouring under a chronic disease,"—it is obviously of little worth, being purely conjectural.

The question of the *propriety of immediate delivery* in hyperæmic convulsions is fully entered into by Dr. Murphy, and decided in the negative. He does not object to the application of the forceps, if the head be within reach, and room sufficient ; nor to that of the perforator, if the head be impacted and the child dead ; but he regards perforating the head or turning the child, on the principle that it must at once under all circumstances be removed, as an error.   Turning is especially objectionable, inducing, as it does, far more irritation than is caused by the presence of the child,—a vaginal examination alone, indeed, frequently sufficing to bring on a paroxysm.   The history of cases where this has been resorted to, shows that the fits have continued afterwards, and that fatal results have been frequent.   Examined statistically, it is found that in 200 cases of puerperal convulsions occurring in British practice, 100 were delivered by the natural efforts, the deaths being 22, or about 1 in $4\frac{1}{2}$.   Of 35 delivered by

the forceps, 13, or 1 in 3, died; and of 43 delivered by the crotchet, 12, or 1 in 4, died; but of the 14 delivered by turning, 7, or 1 in 2, died. The experience of the Dublin Hospital strongly corroborates the same views.

By the term *anœmic* or *asthenic* convulsions, Dr. Murphy does not wish to indicate the convulsions met with as a consequence of extreme exhaustion from hæmorrhage; but those due to a deficiency of the quality as much as of the quantity of the blood, occurring in persons debilitated by poverty and privations, in whom convulsions may occur after very slight, or even without any, loss of blood. He observes that it may be stated as a rule, that convulsions occurring prior to parturition are more dangerous than those which accompany or follow it. " The former depend upon extraneous causes; the latter upon the uterus : the one may arise with women who have had many children; the other occur almost invariably with the first child. In the latter, hyperæmia is the most prominent constitutional symptom; in the latter, there is every evidence that the constitution is suffering from debilitating causes." He illustrates these observations by referring to the success of Dr. Collins' practice, he having to do with robust primiparæ, as contrasted with that of the late Dr. Ramsbotham amongst the poor of London. It is obvious that while depletion, antimony, and other antiphlogistics are demanded in the hyperæmic form of convulsions, the asthenic form calls for stimuli, good diet, warm purgatives, counter-irritation, and opiates.

Rupture of the Uterus.—Dr. Murphy makes ample reference to Dr. Trask's valuable monograph upon this subject,* and quite agrees with him in opinion, that the administration of the ergot in improper cases is a frequent cause of rupture of the uterus. He also dwells upon the *pathological conditions* of the uterus giving rise to the accident, believing their influence to be much greater than is usually supposed. While at the Dublin Hospital, he watched in vain for rupture of the uterus in cases of severe labour in contracted pelvis, and was surprised to find it in others in which no mechanical cause adequate to its explanation existed, in which the pains perhaps were feeble and inefficient, or in which there was no sign of anything abnormal, until some hours after delivery. In a paper, published in the seventh volume of the ' Dublin Journal,' he drew attention to the morbid conditions of the uterine tissue observed in these cases; and since that time Duparcque and various other writers have published illustrative observations. The changed condition consists in some cases of thinning or partial atrophy, and in others of various degrees of softening of the tissues of the organ, with the exception of the peritoneal, which usually escapes. This may go on until the portion affected becomes a mere putrilage; and such cases are especially met with when puerperal fever prevails.

When the child escapes into the abdomen, Dr. Murphy is strongly of opinion, that the mother's safety would be more consulted by resorting to the Cæsarean section for its removal, than by attempting to bring it back through the aperture in the uterus. He believes, however, that popular prejudice would condemn, and probably prevent, resort being had to this means, under the erroneous idea that it was employed for the purpose of endeavouring to save the child—for the mere chance of doing which Dr. Murphy would not inculcate its performance.

* American Journal of Medical Sciences, New Series, vol. xii.

Two chapters of the work are devoted to an account of the history and application of anæsthetic agents; and although, of course, no work on midwifery will be considered complete in future without the question of their employment being fully considered, we scarcely think that there is any necessity for the repetition at such length of the now often-told tale of the history of their discovery. We need not here advert to Dr. Murphy's enlightened and favourable opinions respecting the employment of this valuable means of abridging suffering and facilitating obstetric manipulation, as he has already made these well known to the profession in separate publications. We may state, that in London we have by no means found that willingness or desire for the inhalation of chloroform on the part of parturient women, which seems to prevail in the northern parts of the island; and have been often struck with the indifference and decided refusals with which the proposition of employing it has been met—the objection being based, in many instances, upon what we must regard as a narrow religious view of the question. It seems to us, that while in the case of obstetric operations, or of suffering beyond ordinary intensity, the practitioner should urge its employment, in all other cases, the patient having been made aware of the existence of such an agent, no persuasion should be used, it being a matter purely for her own consideration.

The portion of Dr. Murphy's work which relates to the puerperal state and puerperal diseases, does not call for any notice on our part; and, in our estimation, it does not equal the former part in clearness and practical utility. The description given of the different forms of fever and inflammation, affords a faithful enough reflex of what is known upon the subject, but is diffuse, and wanting in graphic power, contrasting in this respect disadvantageously with other works with which the profession is familiar. We think that the class of pseudo-inflammatory and mimetic affections, so ably described by Marshall Hall, is insufficiently characterized; the whole picture, in fact, conveying rather an idea of what is met with in hospitals during the prevalence of epidemics, than in ordinary bedside practice.

We hope, however, that we have succeeded in showing that Dr. Murphy's work is a valuable contribution to obstetric literature; and with his habits of patient investigation, calm inquiry, and determination to submit to the dictates of authority only when these are based upon a sufficiency of facts, we hope ere long to find him employing the opportunities which his position affords, for the production of a complete treatise on midwifery, such as shall at once take its place as a standard work.

Art. X.

1. *Sketches of Brazil, including New Views on Tropical and European Fever.* By Robert Dundas, M.D., Physician to the Northern Hospital, Liverpool, &c.—*London,* 1852. pp. 449.

2. *Report of the Cases of Fever treated in the Clinical Wards of the Royal Infirmary during the Winter Session of* 1851-2. By Professor Bennett. ('Monthly Journal of Medical Science,' April and June, 1852.)

We took occasion, in a former number of this Journal, to review at some length the most recent views which had been published on the important

subject of the continued fevers. We need scarcely remind our readers, that after an investigation as full and as impartial as we could make of the whole question, we came to the conclusion that the gradual progress of inquiry had tended to make it extremely probable, that as formerly in the case of scarlet fever and measles, so now in the case of these so-called continued fevers, diseases entirely distinct in their origin, progress, and sequelæ, and therefore arising from distinct causes, had been confounded together. This opinion, which had been maintained with greater or less force of argument by many writers, was especially worked out by Dr. Jenner, with a breadth of detail and accuracy of observation which seemed to our mind absolutely conclusive of the question. We may also say, that, as far as considerable opportunities permitted, we had ourselves tested at the bedside the accuracy of Dr. Jenner's observations, and had become convinced of their entire correctness and truth.

The work of Dr. Dundas has been partly published for the purpose of opposing this view, and of showing that " the fevers both of hot and cold climates are identical, and differ only in form and degree." Although the book is styled ' Sketches of Brazil,' it is only to a limited portion that the title is applicable; a great part of the book being occupied with a general discussion on the causes of intermittents, the identity or non-identity of fevers, and the uses of quinine in the treatment, not only of the fevers usually considered to be of malarious origin, but of those which are prevalent in this and other temperate climates.

The lectures by Dr. Bennett were delivered to his clinical class at Edinburgh, and are, like everything else from the same hand, noted for clearness, precision, and force. He, with Dr. Dundas, affirms the negative of the proposition we argued, and believes that continued fevers are one and indivisible.

The question is one of extreme importance as regards therapeutics, and we therefore return to it on the present occasion. Passing over Dr. Dundas's opinions about malarious fevers, with a simple statement that we do not agree with him, we shall notice what arguments our two writers have adduced in support of their position, and in opposition to ours.

In the first place we must observe, that the only method of deciding a question of this kind, is by noting as accurately as possible the symptoms of the diseases supposed to be dissimilar, and of observing whether in their kind, their order of succession, and their consequences, the varieties presented are merely deviations in one or another direction from a single standard, or whether they are so different as to point to two different causes. Symptoms are the indications of an agent or of agencies, destroying the health of some portion of the body; except from these symptoms, we know nothing, in many cases, about the agent; we can, in many cases, neither see it, nor weigh it, nor test it; we know it only by its results. When we find a certain number of symptoms succeeding each other in a given order, and observe the same succession constantly, we infer that a special cause produces them, as a constantly repeated order cannot be accidental. By symptoms, in fact, we reason back to causes, and the department of symptomatology is necessarily the foundation of practical medicine. We cannot understand, then, what Dr. Dundas wishes to imply by the following sentence: — " No practical physician will admit that symptoms alone are sufficient to justify a distinct classification of fevers." (p. 25.)

Why, it is by symptoms alone that we separate smallpox from scarlet-fever, or ague from plague. It is by symptoms, arranged according to their kind and the observed order of their manifestation, that what we term diseases are formed. If symptoms are not different, there cannot be two diseases; if symptoms are different, fundamentally different, there cannot be less than two diseases.

It was by a consideration of symptoms, coupled with the examination into the anatomical changes coincident with the manifestation of these symptoms, that we deemed it necessary to assume that three special causes are active in producing what used to be called the continued fever of this country; that each of these causes gives rise to symptoms entirely different from those produced by either of the other two causes; and that, as might have been implied, the post-mortem appearances are, like the symptoms, different in each of the three cases.

To answer the argument we brought forward, there are but two methods. It must be contended, either that the symptoms are not different, or that, being different, they are not yet so different as not to own a common cause. The latter argument is that usually adopted. The variations of continued fever are so notorious, that it is in vain to deny the differences in symptoms; it has been contended, however, by those who assert the unity of fever, that these differences are not greater than can be accounted for by taking into consideration the influence of age, sex, constitution, modes of life, and peculiarities of atmosphere and climate.

We, and those who think with us, affirm, on the other hand, that the differences in symptoms of continued fevers are so extreme, as to render it impossible to suppose that any modifications in the action of a single agent could produce them. In order to bring out this fact clearly, we described, in the article before referred to, the symptoms of a disease which has been observed with accuracy only within the last ten years, although it has been prevalent, from time to time, for many centuries. The kind and order of the symptoms of this disease are so remarkable, that no one who has ever observed it has doubted but that its cause must be essentially different from other morbific agents. The unanimous testimony of the Scotch physicians who have principally observed this disease, combined with the experience of other countries, appeared to us so conclusive, that we had no hesitation in separating it, and in believing that the so-called "relapsing fever" is a disease *sui generis*, and as different from the diseases with which it was formerly confounded, as small-pox is from measles. This conclusion we believe to be incontestably true; the amount of evidence in its support is overwhelming; and until a series of cases are recorded, sufficient to counterbalance the observations of Cormack, Wardell, Halliday, Douglas, and others, we must keep to our own opinion. The question as regards typhus and typhoid fevers is more difficult; but even here, the careful observations of Jenner, by far the most complete that have ever been made, coupled with the numerous more or less accurate researches which corroborate them, are, to our mind, decisive of the point.

Let us, however, inquire what Drs. Dundas and Bennett have to say on this subject. The following extracts express fully the opinion of the first author, and at the same time present all the evidence on the point which we find in his book:

" The greater number of the distinctions in the symptoms attempted to be esta-
blished between these fevers, are obviously either trivial or visionary, distinctions
without a difference; and however broadly they may be defined in the closet, I
have never yet met with a physician who could conclusively verify them at the
bedside of the patient." (!) (p. 23.)

This extraordinary statement needs no contradiction; but we may refer
Dr. Dundas to some future extracts from Dr. Bennett's lectures on this
point.   As Dr. Bennett agrees with him to some extent, he will be more
likely to accept his opinion than ours.

Dr. Dundas continues:

" As regards the character of the eruption, so constantly insisted on, nothing,
I believe, can be more fallacious; for I have myself more than once witnessed every
variety of eruption pretty distinctly marked in the same patient at one period or
other of his disease; and practitioners who have seen much of tropical fevers, must
have observed the occasional appearance of all the eruptions commonly deemed
pathognomonic of special forms of European fever." (p. 23.)

This statement is not supported by any evidence in the book, and we
can only say, that it is opposed to all the accurate observations we have
been able to gather from the best writers on tropical fevers.   Dr. Dundas
remarks, in a short paper published in the 'London Journal of Medicine,'
in answer to one of his reviewers, who stated that eruptions were not seen
in tropical fevers, that every practitioner must have seen mulberry-
coloured spots passing into the petechial."   This observation perhaps gives
us a clue to the difference of opinion.   Dr. Dundas considers petechiæ as
an eruption.   But of late years, petechiæ, that is, hæmorrhage into the
skin, have been separated from true eruptions,—that is to say, from pecu-
liar congestions of, and exudations into, the skin.   Hæmorrhages—i. e.,
petechiæ, may occur in all diseases; in relapsing typhoid or in typhus
fevers, in small-pox, in pneumonia, in yellow fever, in influenza, &c.; there
is nothing specific about petechiæ; they show only an hæmorrhagic ten-
dency, and point, not to a special affection, but to a particular condition
of the blood and solids which may come on in the course of any exhausting
disease.   If Dr. Dundas asserts simply that petechiæ will occur in the
marsh fevers of the tropics, every one will agree with him; but if he asserts
that the "rose-spots" of the Parisian fever, or the measly eruption of the
Edinburgh or Glasgow fever, will appear in a congestive remittent,
he must adduce his evidence at length.   No one can accept his simple
assertion.

At a later page, Dr. Dundas thus writes:

" It is furthermore obvious, on the most cursory observation, that the descrip-
tion of fever as a disease has, in the generality of instances, been drawn for certain
localities, and not from the whole group of febrile diseases, as witnessed in different
parts of the world.   The typhus fever of this country is superseded by the bilious
remittent and intermittent in southern climates, by the plague in the Levant, and
by the yellow fever in the tropics.   Each of these maladies, under the special
influence of climate, temperament, different modes of living, and numerous other
agencies, affects certain peculiarities in its progress; but they are all distinctly
impressed by the phenomena universally characteristic of fever as a genus of dis-
ease in every clime." (p. 269.)

We heartily wish that the term *fever* could be laid aside altogether as
applied to specific disease, and could be used only to express the presence

of certain symptoms, such as increased heat, quickened pulse, &c., which may occur in numerous morbid states. The term appears to be continually leading men astray. A disease which kills in three days, with black vomiting and yellow skin, and which cannot be seen with the thermometer below 70°, is to be confounded with a disease which runs on for thirty days, has only a few pale-red spots for eruption, and can prevail when the thermometer is at 30°,—simply because this term "fever" has been unfortunately applied to both of them. Against this doctrine of the unity of certain diseases, which are wide asunder as the equator from the poles, but yet happen to have, in common, not only with themselves, but with hundreds of other diseases, certain febrile symptoms, we have frequently protested. If such a doctrine be true—if a single agent can produce such different diseases as a congestive remittent and a Parisian typhoid fever,— farewell to all certainty in medicine. The observation of one hour is destroyed by the observation of the next; and it would be about as useful to register the symptoms of a disease, as to note the various ripples on the stream which from moment to moment the shifting wind produces.

Dr. Dundas's opinions go, then, to the length of affirming the identity of all those diseases, in hot and cold climates, which are called fevers. Into such a question as this we cannot be expected to enter. We might, with as much truth, argue that the coinage of all countries is the same, and that the cowry of the Hindoo is the same thing as the guinea of the Englishman, only changed a little by a rice diet and an abstemious beverage. The real question we would debate is, whether the so-called fevers of cold climates are identical or not, and not whether they are identical with those of warm countries. To answer this question, we find in Dr. Dundas's book nothing beyond the three sentences we have quoted above, and two or three others which have the same meaning. We are satisfied that these sentences should stand on their own merits, and make what converts they can; and we shall pass on to examine the views professed by Dr. Bennett on this important subject.

"I agree," says Dr. Bennett, "with those who consider continued fever as an essential disease, dependent on some unknown constitution of the blood, and occasionally accompanied or followed by various local lesions of the cranial, thoracic, or abdominal viscera, and with various eruptions of the skin." (p. 352.)

After remarking on the question of the identity or dissimilarity of continued fevers, Dr. Bennett continues:

"Any one who studies fever, first in this city and afterwards in Paris, will soon convince himself that there are at least two predominant kinds of fever—the one called by us typhus, the other called by the French typhoid—that is, resembling typhus. Again, those who have studied fever in Edinburgh for the last twelve years consecutively, are aware that every now and then a form of the disease is prevalent, which runs a short course, but has a tendency to relapse at pretty regular periods. Lastly, there is in fever, as in most other diseases, a kind which is very slight and soon ceases—a so-called febricula. Every practical physician is acquainted with these forms of fever." (p. 352.)

We commend this last sentence to Dr. Dundas's consideration; in Dr Bennett he will find a physician "who can conclusively verify these distinctions at the bedside of the patient."

Dr. Bennett, then, practically proves that he has made these distinctions,

by dividing his cases (19 in number) into 4 sections—viz, febricula, 4;
relapsing, 1; typhoid, 3; and typhus, 11.

So far Dr. Bennett's experience would seem to be altogether opposed to
the views of Dr. Dundas, and to be in favour of the non-identity of these
forms of fever. He not only separates them at the bedside, but he finds it
convenient to use different terms to apply to each form. He uses the words
typhus, typhoid, and relapsing fever, in the same sense that we do, with
the same precision, and the same certainty.

As regards, also, the experience of the Edinburgh wards for 1851, that
of two is in favour of the non-identity, since, in the nineteen cases under
Dr. Bennett's care, no difficulty has been found in assigning to each case its
proper position. Yet Dr. Bennett does not believe that the doctrine of
non-identity has yet been satisfactorily made out. How is this? Turning
to the June number of the 'Monthly Journal,' we find that Dr. Bennett's
argument for their identity assumes the following form. We give the
extract in full, as we are afraid lest by abridging it we might fail in giving
every point its due weight. After a brief history of each case, Dr. Bennett
proceeds:

" *Diagnosis of Continued Fevers.*—On reviewing the nineteen cases of con-
tinued fever embraced in this report, with a view of determining how far we are
enabled to distinguish its varieties at an early period, it will, I think, appear that
this is impossible. If there be any fact connected with the disease better esta-
blished than another, it is, that at the outset we are unable to determine whether
any given case will turn out to be a febricula or a typhus, a relapsing or a typhoid
fever. If you study carefully the symptoms presented by cases 3, 5, 6, and
11, you will be satisfied of this. We may, indeed, when acquainted with the
prevailing type of an epidemic, often be led to guess, with more or less correctness,
as to its probable cause, but exactitude is impossible. Should the fever cease on
the seventh day, then it may be febricula, or relapsing fever. The latter is deter-
mined by the return of the disease; but I know of no circumstance, beyond the
type of the epidemic, which can lead us to predict that event. On the other hand,
should the fever continue beyond the seventh day, then we have to do with typhus,
or the typhoid form. Notwithstanding all that has been said as to the means of
distinguishing these varieties by means of the eruption or of the abdominal symp-
toms, I believe that in practice it will be found to be impossible before the twenty-
first day. We have seen in the three cases of typhoid fever which have fallen
under our observation, that no eruption existed in any of them. With regard to
the ten (eleven?) cases of typhus fever, also, in five there was no eruption; in
three there were rose-spots; in one a mulberry or measly eruption; and in one
petechiæ. Then, with regard to diarrhœa, it is only symptomatic of typhoid fever
after the fourteenth day. Thus, in case 6 it first appeared on the twenty-eighth
day; and in case 7 on the fifteenth. In case 8, on the other hand, it is said to
have been present from the first. From all these considerations, the distinctions
which have been made out between the various forms of fever, are for the most
part retrospective, and can only be determined in the advanced stages.

" The same arguments which apply to the uncertainty of diagnosis, may be urged
against the general doctrine that these forms of fever are dependent upon separate
poisons, run a distinct course, and are governed by laws as distinct as those which
regulate the various kinds of eruptive fevers. Without denying the existence of
various kinds of continued fever, I am of opinion that this doctrine has not been
established." (pp. 564, 565.)

If we examine, one by one, the arguments advanced in this passage, we
find them to be as follow:

1. *The supposed dissimilar fevers cannot be distinguished in the early*

stages. This is no more than might be said of fifty severe diseases, were it true ; but we believe that in many cases they can be distinguished even within the first five days ; but whether they can or cannot, is of no real consequence as regards their identity.

2. *The Distinction between these Fevers must be Retrospective.*—We doubt this ; but admitting it, the distinction would not be the less real.

3. *The Eruptions are not Diagnostic.*—Now this would be a well-founded objection. In typhus fever, although the eruption is not present in every case, it was present in 89·5 per cent. of Dr. Jenner's patients, and is almost invariably present in persons over twenty-five. Its absence, therefore, in any particular case after the seventh day, is a strong argument against the diagnosis of typhus. Now, Dr. Bennett says it was absent in 5 cases out of 11—namely, cases 1, 2, 6, 7, and 11.* On reference to these cases, we find that case 2 was in a man aged 21 ; case 6 was a man aged 20 ; and case 11 was a girl aged 20. In these 3 cases, then, it is possible that the eruption may have been absent, simply on account of the youth of the patient. In case 1 the diagnosis of typhus fever may be questioned. It may have been a cerebral disease. Four days after admission, after vague symptoms of pyrexia, and after a good deal of vomiting, the patient had a convulsive attack ; on the next day there was confusion of intellect ; and two days afterwards, paralysis ; the patient died, and no autopsy was obtained. Dr. Bennett considered it typhus with cerebral complication, and we do not wish to assert that it was not so; but we hold that, as reported, we have some grounds for a contrary diagnosis. Case 7 was in a man aged 43. It is so briefly reported (eight lines), that we do not know what to say about it ; as the man had pneumonia of the left lung, is there not a possibility that it may not have been typhus, but merely pneumonic pyrexia?

We would unquestioningly receive Dr. Bennett's opinons as to these cases, did we not feel that in a matter of this kind every one must judge for himself, from the evidence that is put before him. In the present day, if Hippocrates were to announce some medical dogma, we should ask him for his proof; and none of his followers, however eminent and justly celebrated, can claim to be otherwise treated. We could have wished that Dr. Bennett had communicated his cases in full, as that would have removed all doubt.

We hold, then, that of these 5 cases, 3 were in persons under the age when eruption is always present, and in the other 2 the diagnosis may be questioned.

Dr. Bennett, then, states that in 3 other cases of typhus (cases 3, 5, and 9) there were rose-spots. In case 3, the only note of eruption is, that on the fourteenth day there was "marked eruption over chest and abdomen." Now, the rose-spots of typhoid fever are usually few and scattered, but the early typhus rash, which for a day or two often resembles the rose-spots, is frequently marked over the chest and abdomen. No further note is made on the eruption. In case 5 the rash is thus noticed. "Rose-coloured exanthematous spots appeared on the chest and arms." Now, the rose-spots of typhoid fever could never be called "exanthematous" in the sense in which it is used here. In the commentary on this case, we are told "the eruption here presented rose-coloured spots

* We take the numbers as they are given with the cases.

at the commencement, becoming darker afterwards." This is the ordinary course of things in typhus, but never occurs in typhoid fever; and this remark proves to us, that although called rose-spots, this was the true typhus exanthem. In the last case (9) the eruption is thus noted :—" Rose-coloured elliptical spots scattered over the abdomen and chest appeared this morning ; they are of mulberry colour on the arms." This, evidently from its profusion, and from the colour on the arms, was the typhus rash, light coloured, as it often is at the commencement.

We do not wish to be hypercritical on Dr. Bennett's evidence, but it is absolutely necessary to examine closely all the facts that are brought forward on this point ; and Dr. Bennett himself would, we are sure, be the first to wish his statements to be sifted and canvassed as closely as possible.

In the 3 cases of typhoid fever recorded, " no eruption," says Dr. Bennett, " existed in any." As the rose-spots only appear in eighty-five per cent., it is not impossible that they might have been absent in these 3 consecutively, and may have been present in the 15 next. It appears, however, that in one case—a fatal one—the patient came in with perforation at the end of the disease, and died in two days. Dr. Bennett says, in his account of the case, that " the captain of the vessel" (the boy was a sailor) " had observed no eruption on the skin ; but of course his information on such a point is of no great value." (p. 357.)

Therefore, in point of fact, there were only two cases of typhoid fever without eruption, as of course the negative evidence of a non-professional person in the case of rose-spots is inadmissible, and the patient may have had the spots before his admission on the twenty-fifth day of the disease.

To sum up, then, our conclusions, we may say, that in 2 cases of typhoid fever and in 3 of typhus the eruption was absent ; in 2 cases of presumed typhus the diagnosis may be questioned ; in 3 cases of typhus, said to have " rose-spots," it may be almost certainly concluded, that the spots were not the true " rose-spots" of typhoid fever, but the early rose-coloured stage of the " mulberry rash." So far, in fact, from Dr. Bennett's cases invalidating the diagnosis from the eruption when present, they may be held to confirm it.

The Diarrhœa is only symptomatic of Typhoid Fever after the Fourteenth day.—This statement is so opposed to all ordinary experience, that we are unable to receive it, especially as it would appear to be made merely from the observation of two cases. Of 40 cases mentioned by Louis,[*] 22 had diarrhœa from the first day ; 18 between the third and ninth days ; 6 between the eleventh and fourteenth days ; and 3 had no diarrhœa throughout. Dr. Jenner states that the diarrhœa, if it be not initiatory, commences on the fourth, fifth, or sixth days.[†] The experience of most practitioners in this metropolis would, we fancy, be in accordance with this.

The question of the identity or non-identity of the fevers of cold countries, appears to us to be in this position. Very strong evidence has been brought forward by the advocates of the non-identity, which must be met by evidence equally stringent on the other side. A series of observations, proving that the symptoms of typhoid fever can exist with intestinal lesion, and that the symptoms of typhus fever can exist with typhoid deposit in Peyer's patches, is the only plan by which the observations of Louis,

Valleix, Gerhard, Stewart, and Jenner, can be answered. Dr. Bennett has seen this, and has acted to a certain extent upon it; but his cases are reported so very briefly, that they are of little use. So far as they go, they do not appear to us to make out any case against the doctrines of Dr. Jenner.

The last argument which has been advanced in favour of the identity of all fevers, not only of cold, but of hot climates, is, that *they are all equally curable by quinine.* Dr. Dundas, who has brought forward and supported this doctrine, lays great stress upon it, and believes also that he has succeeded in introducing an important change of practice.

Experience only can decide a point of this kind; and if it should turn out that quinine will cure typhus and typhoid fever as readily as it will ague, then we admit that it would be to a certain extent an argument, though not a decisive one, for the identity of these diseases. The practical point that quinine would cure typhus and typhoid fevers would, however, apart from all question of identity or diversity, be a discovery of the greatest importance.

Dr. Dundas states, that if quinine be used at the commencement of continued fever, in doses of ten or twelve grains every two hours, the disease will be arrested " in the great majority of cases." Should the urgent symptoms, however, persist, the quinine must be suspended, and after seven or eight hours, small and repeated doses of tartar-emetic should be given to full vomiting. The patient should then be allowed to rest for twenty-four hours, and the quinine then re-administered. In support of this, Dr. Dundas relates various cases treated by himself, Dr. Scott, Dr. Goolden, Mr. Eddowes, Mr. Hine, &c. The improvement in some of these cases would appear to have been marvellous. Take the following as an illustration :

" Case 1.—Cornelius Vincent, aged 26, admitted under Dr. Dundas.

" Oct. 3rd.—Has been ill ten days. *Present state :* Severe headache, anxious countenance; slight delirium; skin hot and dry; tongue black, dry, and furred; teeth covered with sordes; thirst; urine scanty and high-coloured; bowels open; pain of abdomen on pressure; pulse, 100; respirations, 28. Ten grains of disulphate of quinine every second hour.

" Oct. 4th.—*Convalescent.* The pain in the head and the delirium have ceased; the abdomen is less tender; the heat of skin diminished; the tongue clean and moist; pulse, 90; respirations, 24. Infusion of quassia, ʒiij three times daily. No further treatment was resorted to, and from this date he rapidly gained strength, and was discharged well on the 11th October." (pp. 403, 404.)

If this were truly a case of typhus, the improvement was most remarkable; but we could have wished it had been detailed more at length. All Dr. Dundas's cases are so sketchy as really to be almost worthless. In the present day, medical men *will* judge for themselves, by demanding a proper detail of symptoms, as to the efficacy, or otherwise, of a particular mode of treatment. In every case the symptoms should be given so fully as to enable the reader to form his own diagnosis; without this, no impression will be produced on the profession.

Although, however, we consider that Dr. Dundas has not done justice to his brief, by the kind of evidence he has produced, there is no doubt that as several medical men besides himself have found benefit from quinine, in various febrile attacks (which, however, in no single case are properly

diagnosed, and among which an undoubted case of erysipelas of the head is put down as typhus), the plan demands a careful investigation.

Dr. Bennett has tried this method in the manner recommended by Dr. Dundas, in two cases of typhoid, and in four or five of typhus. " In none of the cases," says Dr. Bennett, " notwithstanding the physiological action of the drug was well marked, did it in any way cut short the disease, or produce on its progress, as far as I could ascertain, any amelioration whatever." (p. 564.)

Dr. Robertson, Dr. Bennett informs us, has tried it in eight cases, and Dr. Christison in one, with an equal want of success.

At present, then, the evidence for or against the quinine treatment may be said to be nearly balanced; except in so far as the experiments of Drs. Bennett and Robertson must be held to have been more carefully conducted, judging from the records, than those of Dr. Dundas.

The plan may be said to be still *sub judice,* and to require, both for and against, a greater amount of evidence than has as yet been brought forward. We recommend Dr. Dundas to pursue his investigations, and if he succeed in proving his case, he will have deserved well of his profession.

Art. XI.

Raccolta delle Opere Minori di Alessandro Riberi.
The Minor Works of Alexander Riberi : *with Notes and additional Essays.—Turin,* 1851. 2 vols. 8vo, pp. 1000.

This work consists in a republication of the various surgical essays, cases, and observations, which Professor Riberi has communicated to the Italian journals during the last thirty years. These have been collected by Dr. Marchiandi, an old pupil; but the professor has enriched them by additional notes, and has added many papers hitherto unpublished. The facts are principally derived from the large hospital at Turin, to which M. Riberi has for so long a period been chief surgeon, and which has afforded him a vast field of observation that he seems to have very diligently cultivated; his object in publication at various times being to communicate some of the practical results to the less favoured among his countrymen. We think much might have been omitted in this republication, especially the discussions of practices among his predecessors or cotemporaries, which the progress of science has now rendered of less interest than when they were first introduced. Almost all the authorities quoted by M. Riberi are French, and he seems to know little of English surgery save through that medium; but we fear that his countrymen may address the same reproach to ourselves respecting the surgery of Italy.

From amidst so miscellaneous a collection of facts and observations, we select a few to present to our readers.

Traumatic Tetanus cured by Inhalation of Ether.—Although there are already cases on record in which this fearful disease has yielded to the induction of anæsthesia, we think a notice of the two narrated by M. Riberi will prove of interest.

Valentino Corteri, æt. 26, a strong countryman, of the sanguineo-bilious

temperament, received a gun-shot wound in the sole of the foot, December 17, 1848, and was admitted into the hospital the same day. In order to subdue the subsequent traumatic fever and pain in the foot, he was repeatedly bled, and kept on low diet; and all seemed going on well until the twelfth day, when, the tendon of the *flexor longus pollicis* having come away by sloughing, severe pain and violent extension of the toe occurred, and continued day by day to increase. Violent tetanus soon manifested itself, and after the trial of large doses of laudanum, it was resolved to have recourse to etherization. The inspirations were commenced on the 5th of January, and repeated five or six times a day for a period of seven or eight minutes, the laudanum being still continued, and the acetate of morphia being applied to the wound, which had assumed quite an inactive appearance. The temporary relief obtained by the ether was very manifest and decided, so that the patient implored its use; but in the intervals of its employment the tetanus continued to implicate more and more of the muscles, and by the 12th of January the mouth could hardly be opened. The laudanum was increased to half an ounce per diem, and the limb was rubbed with a liniment containing morphia. The disease now showed signs of diminishing; and the inspirations, which were continued for ten days longer, were gradually lessened in frequency and duration, the quantity of laudanum being also diminished. The paroxysms were invariably relieved by the ether. About the 20th of February, cicatrization was completed. It may be objected that the large doses of opium given had something to do with the cure; and Dr. Riberi does not deny that they may have had their part; but he observes, that no one who witnessed the marked and complete temporary alleviation produced by each application of the ether, could deny its agency. Moreover, of the numerous cases of traumatic tetanus he has met with in the hospital, some far less severe than the present, and in which yet larger doses of opium have been administered, he has never known one to recover.

2. Major Morand, æt. 39, and of strong constitution, while at the head of his troops at the battle of Novara, 23rd March, 1849, was struck by a ball at the outer and upper part of the left hip, the ball being lost amidst the muscles. Five or six days afterwards he reached Turin, having suffered much on the journey, and feeling depressed at the disastrous issue of the battle. After exposure to the air on a cold and wet night, he was seized with the first symptoms of tetanus on the 3rd of April, and these rapidly increased, so that in a few hours the attack had become quite confirmed, and the patient's condition highly alarming. Contrary to what is usually seen in tetanus, the intellectual faculties were in a very clouded condition. He was bled twice, and ether was resorted to five times at intervals of an hour. Anodyne embrocations were applied to the spine and near the wound, and some laudanum was got within his spasmodically closed jaws with great trouble. Next day the tetanic spasms had quite disappeared, but were replaced by apoplectic congestion of the brain; and this resisted very active and repeated bleeding for four days. The tetanic symptoms never re-appeared, and by the 8th of May the patient was declared well.

Acute Inflammation of the Cotyloid Cavity.—Professor Riberi relates six cases of this affection. It is characterized by acute pain at the hip-joint,

aggravated by pressure, and especially by any attempt at moving the limb. There is a sense of deep-seated fluctuation conveyed to the hand, and the limb appears considerably elongated, but not changed in direction. The general febrile action is very considerable. Active antiphlogistic treatment usually soon subdues the disease, and the limb then appears of its normal length. Dr. Riberi observes—1. That all these patients were between 10 and 20 years of age, that five out of the six were males, and most of them of the lymphatic temperament and more or less strumous habit.— 2. Although the affection seems of a rheumatic character, and is brought on by exposure to cold, no other joint becomes affected. In three of the cases, incipient endocarditis or pericarditis was observed.—3. The antiphlogistic treatment was rigidly carried out, from three to eight general bleedings having been resorted to. The internal employment of nitre was also found useful. In some cases leeches were freely employed; and as a means of relieving the severe pains, an embrocation, composed of oil of stramonium and prussic acid, or an ointment containing extract of hyoscyamus and blue ointment, was found of service.—4. It has been much disputed, both in these cases and in hip-joint disease, whether the *elongation* of the limb is real or only apparent. Fricke even maintains that with apparent elongation there is actual shortening, the elongation really depending upon inclination of the pelvis on that side. In point of fact, however, accurate admeasurement proves that there is real elongation, but that this is by no means so considerable as it appears to be to the eye.

Observations upon Strangulated Hernia.—Under this heading, Dr. Riberi adverts to various questions of great practical interest. (1.) *Rules that should guide the application of the taxis.*—(a.) Resort to the taxis is contra-indicated when the strangulated parts or the adjoining viscera are in a state of acute inflammation, and especially when, from the duration or intensity of the strangulation, or other causes, there is reason to believe that some of the results of inflammation have been produced, or that this has extended from the strangulated part inwards. It is true that sometimes the signs of inflammation are more apparent than real, and that a bleeding or two, with the application of ice and of turpentine, serve to dissipate them, and to render a hernia reducible that seemed imperiously to call for operation. Such cases are, however, only rare exceptions.— (b.) In general, the taxis is inexpedient where the strangulation is produced by the neck of the sac, by the internal orifice of the inguinal canal, by the viscera themselves, or by a morbid communication with the tunica vaginalis, &c.—circumstances indicated by the history of the case, and by the introduction of the point of the finger within the canal.—(c.) The taxis is indicated (aided by baths, the application of ice, the use of clysters, the introduction of opium into the urethra, &c.) when the accompanying irritation or inflammation is slight, and entirely confined to the tumour.—(d.) It is especially so when the strangulation takes place at the external orifice of the canal or the crural arch, where the fixed condition of the parts, and the possibility of directing the pressure in the axis of the apertures, often enable us to succeed.—(e.) Much, however, depends upon the manner in which the taxis is applied. The pressure must be uniform, prolonged for half an hour, and graduated, with a slow, vermicular motion,

having in view the double object of emptying the vessels of the strangu-
lated parts, and of returning these after having thus diminished their
volume.   M. Riberi illustrates this procedure by that which is followed for
the reduction of a paraphimosis; having, in the cure of this disease, at all
stages of its progress, never had recourse to a cutting instrument.

(2.) *Attempt at immediate union of the Wound of the operation.* —
Attempts have been made to unite the wound made at the operation by
means of sutures; but if these have now and then proved successful, in
most cases purulent collections, erysipelas, &c., have resulted.   In two
cases the author obtained the union of the parts, but in both he had to
open several abscesses which formed at a later period.   In another case in
which he was consulted, indications of extra-peritoneal purulent effusion
were the consequence of such union having been obtained.

(3.) *Treatment of Omentum.*—When in the operation for hernia, omen-
tum has been found with the intestine in the sac, some surgeons advise
that the intestine should be reduced, and the omentum left untouched or
in part excised, believing that by its subsequent adhesion it would form
an obstacle to the future production of the hernia.   In three cases thus
treated by the author, the hernia was reproduced, and he has therefore
abandoned the practice, not only as being of no avail, but because the
retention of the omentum in the sac may give rise to disorders of the
digestive organs.

(4.) *Inflammation of the Intestine.*—It is not uncommon to find, on
opening the sac, that by reason of violent inflammation, the intestine has
become thickened in its parietes, unequal on its surface, and of a brown or
livid colour, but without, as yet, gangrene having manifested itself.   The
precept is to return the intestine, the natural tepidity of the abdominal
cavity being an excellent corrective to inflammation.   The propriety of the
precept is indubitable, but this protective power of the abdominal cavity
has been exaggerated.   In six of the cases so treated by the author, ulcera-
tion followed between the fifth and tenth days, with the issue of gas, fæcal
matters, or even worms; a more or less speedy spontaneous cure still
resulting.   M. Riberi considers that the natural fomentation which the parts
receive on their return, assists in, but does not suffice for, the resolution of
the inflammation.   In spite of its aid, ulceration frequently follows, although
the ultimate results of this are not so serious as might be expected.   He
advises, in such cases, that antiphlogistic measures should be employed
with energy and perseverance, so long as any sign of inflammation is
present.   It does not suffice to have combated the febrile reaction, but the
painful induration opposite the internal ring and adjacent parts should
also be entirely subdued.

(5). *Gangrene of the Intestine.*— Various are the precepts which have
been delivered, for the treatment of intestine which is found, upon opening
the sac, to have become attacked with gangrene over a greater or less
space.   The practice M. Riberi pursues is as follows.   He dilates the
strangulating part with a bistoury, as also the internal ring, if it offer any
resistance, though this may not amount to strangulation, draws out the
intestine, and cuts away the gangrened portion.   If an entire segment of
the intestine be not comprised in the gangrene, he unites the edges of the
aperture, by means of sutures, with the lowest portion of the wound, in

order to facilitate as much as possible the retraction of the intestine on the
re-establishment of the entirety of the canal.   When the entire segment
is affected, as a preliminary step he unites the two ends by means of sutures,
or he unites these two extremities of the intestine by their serous surface,
as is done by Jobert, and then passes a thread through the mesentery,
bringing it out at the lips of the wound, until the re-union of the intes-
tinal margins has taken place.   Three cases are related, in which by pur-
suing this plan of fixing the extremities of the intestine to the deep portion
of the wound, all effusion of the intestinal contents into the peritoneal
cavity was avoided.   In neither of them did any artificial anus form, while
in two complete cure was obtained, and in only one a very small intestinal
fistula remained.   In cases where the practitioner merely cuts away the
gangrened parts and liberates the stricture, an artificial anus is usually
the result.

Abnormal Secretion of Milk.—Boerhaave, Haller, and Osiander, refer
to examples of men, in whose breasts a secretion of milk took place in
consequence of their having applied infants to them in order to keep them
quiet.   Other examples of abundant secretion are recorded as occurring in
virgins, or in women who had long ceased child-bearing.   In one of the
four cases related by Fax, the woman was sixty years of age, and thirty
had passed away since she had had a child.   Richer relates a case in
which a young slave became an excellent nurse for a year.   Professor Riberi
has met with three examples of secretion of milk in non-puerperal
women.   As in some of their characteristics they bore great resemblance
to each other, we will not follow him in the separate narration of each
case, but notice some of their general features.   Two of them were un-
married, and respectively twenty-three and thirty-four years of age, and
the other was a widow.   In the first two the hymen was entire, and in
all, their conduct was such as to give no reason to believe the secretion
was connected with the puerperal state.   All suffered from disturbed
menstruation and uterine irritation, and exhibited a train of Protean symp-
toms, that we can regard as none other than aggravated hysteria, but which
were treated with the profuse bloodletting and debilitating treatment, that
are the reproach of Italian practice.   When at last sedatives and tonics were
resorted to, considerable improvement followed.   All these women were of
a lymphatic-sanguineous temperament, timid disposition, and somewhat
dull intellectual powers; and in all the breasts were preternaturally large.
The quantity of milk which flowed away from the enlarged and painful
breasts varied, but was usually estimated at two or three pounds per diem
in one case, and one or two pounds in another, and that for months and
years.   In one of the cases, the only relief that could be obtained was pro-
duced by applying puppies to the breast; and in the case of the widow, a
female attendant had to be employed to suck her breasts, which she did
three times a day for above five years.   The patient became passionately
attached to the woman who performed this office for her, and who, on her
part, acquired a great liking for her mistress' milk, and was able, from
change in its taste, to prognosticate approaching alteration in the health.
In one of the cases the milk was analyzed, and found to be normal, the
proportion of casein and cream being perhaps too small.   In the others,

the milk had all the appearance of being healthy.   The author adds, in a note, that two of these patients eventually quite recovered after marriage.

Dr. Riberi also relates the case of an infant two months and a half old, both of whose breasts secreted milk.   It was not an example of the hard, swollen state of the breast, so often seen in young infants.   Both breasts were as large as a good-sized apple, of their natural colour, and undulating to the touch.   Gentle compression immediately discharged all the milk through the nipple. and the swelling disappeared.   The breasts were thus pressed daily, the quantity of fluid gradually diminishing, and on the tenth day they assumed the normal appearance.   The milk on analysis was found to resemble ordinary human milk.

On the Employment of Acupuncture.—Acupuncture, Dr. Riberi observes, has undergone the same fate as so many other therapeutical procedures— that of falling from undue renown into undeserved oblivion.   As in its period of prosperity he was not one of those who exaggerated its powers, so now he does not feel disposed to abandon it, having, in the course of a long practice, found it very useful in a great number of diseases, differing much from each other.   He publishes a few examples of these, but we prefer noticing some of his general observations upon the classes of affec- tions to which it is most applicable, and upon its supposed *modus operandi* in these.

As acupuncture is employed in diseases of different natures, the question first arises, what is the dynamic condition against which it most avails? This is the *erectile or hypersthenic* condition, which is increased by stimuli and diminished by debilitants.   And thus many cases of gastralgia, rachi- algia, incomplete amaurosis, paraplegia, &c., dependent upon atony or anæmia, are found to improve upon good diet and stimuli, when acupunc- ture has been tried in vain.   The diseases to which it is pre-eminently applicable, are *neuralgiæ*, and the so-called *nervous pains*, of an *erectile* or hypersthenic character, but unaccompanied by inflammation.   It has also been found useful in the pains accompanying inflammatory affections that have not yet produced organic results ; but here the result is less striking and less durable, and especially if the local inflammation be accompanied by febrile action, when, indeed, the acupuncture may be mischievous.   It has been found useful in various kinds of chronic spasm, as also for the relief of the pains accompanying strumous affections.   In respect to the painless affections it has been applied to, as partial paralysis, hydrocele, anasarca, &c., Dr. Riberi has found it of little avail.

Dr. Riberi agrees with those who do not believe that acupuncture acts as a mere counterirritant, but that it operates by abstracting an imponder- able fluid.   He rests the foundation of such belief upon the change of colour imparted to the needles, resembling that induced by the electrical current, and upon the sensations which they sometimes convey to the finger brought in contact with them, as well as their occasional action on the gal- vanometer.   Moreover, acupuncture succeeds where the most potent counterirritants have failed, and relieves pain with a rapidity to which they offer no parallel.

After the trial of various sorts of needles, Dr. Riberi now almost exclu- sively employs fine, sharp ones of tempered steel; and he has not found

any superior advantage in using those of other metals, either successively or simultaneously. He has abandoned the use of flexible needles, and has never known any example of the inflexible ones breaking during the contraction of the transfixed muscles. He does not employ less than six, or more than twenty—the number, as a general rule, being greater as the affection is of older standing, or larger extent, but being also regulated by the age of the patient, nature of the disease, &c. When the patient tolerates the presence of the needles well, he allows them to remain-in from three to seven or eight hours. In the contrary case, they are removed when the punctured part becomes impatient of their presence. He is opposed to the opinions of those who state that the needles may penetrate deeply into the various organs with impunity—e. g., the heart, medulla spinalis, large arteries or veins, &c.; but always avoids these, and succeeds just as well. He employs needles of not less than $1\frac{1}{2}$ inch, and of not more than $3\frac{1}{2}$ inches long, passing them not perpendicularly into the substance of the various organs, but more or less obliquely in the cellular tissue. He has found no advantage in directing their points in this or that direction. So little is the disturbance created by acupuncture, that, with few exceptions, it can be repeated every second or third day.

Aggravated Onanism cured by Excision of the Clitoris and Nymphæ.— Maria A., æt. 39, was received into the Clinical Hospital, suffering from some of the extreme effects of onanism, to which she had addicted herself for the space of twenty years. The uterus and neighbouring organs were found congested, but not otherwise diseased. A variety of remedies was had recourse to, such as ice, sedatives, anodynes, &c., administered internally, and locally applied. The patient's limbs and body were secured by ligatures; but all in vain, as pollution followed the slightest movement, or occurred even without any. Finding that the voluptuous sensation seemed especially centered in the clitoris (which was not enlarged) and the nymphæ, M. Riberi amputated these after she had been a month in the hospital. Some considerable reaction followed the operation, which was subdued by low diet. The woman began to get good sleep at nights, and by the fifth day, the hypochondriacal symptoms, confusion of ideas, palpitation of the heart, and hurried respiration, had diminished. She reproduced the febrile action by automatically directing the hand to the wound in her sleep; and as it healed up, she found it impossible to prevent this movement. To correct this consequence of old and inveterate habits, it was deemed advisable to keep the wound open by irritating applications for two months, so that the pain induced by touching it in her sleep might awaken the patient. This plan had the desired effect. The habit was discontinued, and the patient recovered from the extreme state of constitutional irritation into which she had plunged herself, with remarkable rapidity and completeness. She was seen several years afterwards, in perfect health.

Anomalous Position of the Urethra in the Prostate.—Ordinarily the urethra, in its passage through the prostate, lies nearer the *pubic* than the rectal portion of the gland. In some cases referred to by Velpeau, however, it passed near the *rectal* portion; Senn has met with it once near the lower surface, and Tanchon below the lower surface of the gland. Demi-

chelis also, has seen the urethra situated near the lower part in two cases, and Professor Riberi has met with three in which it ran below it. In one of these cases, the catheter seemed to pass so superficially, that it was at first believed to have entered a false passage. In the two other cases, the anomaly was remarked during the performance of bilateral lithotomy, the prostate being found between the back of the finger and the pubis.

There is another anomaly less familiarly known—viz., the passage of the urethra, not in the median line of the gland, but either to the right or the left of this. The lateral operation for lithotomy was performed on a lad, the incision into the prostate and bladder being only of moderate extent. He died with all the symptoms of extravasation of urine. At the autopsy, the left side of the prostate was found much smaller than the right; and although the incision was not too large, it had extended beyond the base of the gland, and led to extravasation. In another case, the incision was so small as hardly to admit the index-finger. Intense peritonitis, dependent upon urinary effusion, carried off the patient on the fourth day. The prostate, as regards its entire size, was quite normal; but the left side was far less, and the right was much more, developed than it should be. Of course this congenital unequal development, rendering the lateral operation so dangerous when it occurs on the left side, is entirely different from that which so frequently results from disease.

On the utility of administering Opium by the Urethra.—Prior to 1830, the author had published a statement of the great utility derivable from the introduction of opium into the urethra, to alleviate or overcome certain painful or obstinate affections, and especially strangulated hernia, violent colics, and some forms of ischuria. Since that period, the opportunities of a large hospital have furnished him with abundant means of confirming his statements. The dose employed has varied from two grains to six, the effect being the same whether the opium is introduced merely into the urethra or reaches the bladder. Narcotism is much more easily induced in this way in the male than in the female. It has been found very useful in strangulated inguinal hernia, the hernia sometimes returning of itself when the narcotism has been induced, but the aid of the taxis being also required in most cases. When the accompanying phlogosis has already led to certain results, such as adhesion, purulent effusion, &c., or where strangulation has supervened upon long-continued irreducible hernia, the operation will still be required; but even in these cases, the patients who have been subjected to the narcotism are much less sensible to the pain of the operation, and suffer much less from the subsequent traumatic fever. It is surprising to see in these patients, twenty or thirty minutes after the introduction of the opium, so complete a cessation of pains which had been so violent; the pulse becoming softer, the hernia less tense, and the whole system falling into a state of relaxation as a consequence of the narcotism. In two cases the excessive suffering from nephritic colic, consequent on the passage of calculi, was relieved, after all other means had been tried in vain. In six cases of retention of urine, in four of which catheterism had become impossible, relief was obtained. In two cases of neuralgia of the urethra from onanism, the same result followed the reiterated production of narcotism. In cancer of the uterus, the practice

proves a highly useful means of palliating suffering. In spasmodic colic the method acts like a charm; and even in inflammatory colic it is a powerful adjuvant to antiphlogistic treatment. This mode of introducing opium is also valuable in cases of painful organic disease of the alimentary canal, in which it is obstinately rejected by the mouth. Thus introduced, it does not cause the obstinate constipation or irritation of the digestive organs, nor the excessive narcotism, which large doses, often repeated, are apt to induce when administered by the ordinary modes.

Complete Amaurosis cured by the Endermic employment of Strychnia.— Strychnia and nux vomica have long been recommended in the treatment of amaurosis; but in the cases in which M. Riberi has hitherto administered the former, he has derived little or no benefit from its employment. He suspects, however, that he has not, in some of these, persevered long enough with it. The successful cases, indeed, hitherto on record, seem to indicate its use in incomplete rather than in complete amaurosis. Induced by successful cases published by Maunoir and Petrequin, he resorted to the means again in a case of complete amaurosis, and with an entirely successful result. The patient was a country lad, æt. 16, of scrofulous habit, who at twelve years of age became addicted to onanism. When about fifteen, he accidentally found, on closing the right eye, that he was entirely blind of the left. The loss of sight was unattended by pain or any other symptom. About a year after, he fell from a height, and concussion of the brain resulted. Upon this confusion of vision supervened, and eventually complete amaurosis of the right eye also. When admitted, the power of distinguishing light had been lost for a twelvemonth by the left eye, and for two months and a half by the other. As some cerebral congestion was present, he was freely bled and purged, and put on low diet. At the end of a week, the skin, first of the forehead and afterwards of the temples, was denuded by blisters, and a powder, composed of one-eighth of a grain of strychnine and three grains of nux vomica, was applied morning and evening, from the 27th of August to the 20th of September. After the third day, the light of a candle brought near the right eye was distinguished; and by the sixth day he could distinguish natural from artificial light; on the tenth, he could see the fingers of the hand; by the twentieth, he was able to assist in the ward; and on the thirty-fourth, he had quite recovered the sight of the right eye, and could see his fingers indistinctly with the left. The right iris had quite recovered its movements, and a slight vermicular motion could be perceived in the left. He refused to remain longer in the hospital in order to attempt to obtain a complete cure of the amaurosis of the left eye.

Cases of Lithotomy.—Dr. Riberi prefaces his account of some cases of lithotomy, with a few interesting observations on the *spasm of the bladder* which is sometimes met with during this operation—not alluding to the slight spasm which almost always affects the organ, but to the severe form, which may give rise to more or less embarrassment. It is found to occur especially in young subjects, and in those of an impressionable character, and liable to nervous affections; as well as in those who, from the commencement of the disease, have suffered from anomalies in the expulsion of the urine, and are

liable sometimes to retention and enuresis, sometimes to great irritability of the bladder, and at others to a slothful action. It is especially to be expected when the bladder imparts sudden and violent shocks to the catheter during exploration, particularly when it is empty, or is emptied by the catheter during the examination. The spasm of the *entire* bladder comes on suddenly as soon as the neck is divided, and then the stone may be forced against or within the wound, and removed by the finger or the scoop, it being difficult and dangerous to expand the forceps. *Partial* spasm is rarely of sudden occurrence, but usually occurs only after the bladder has been long irritated by the introduction of the finger or forceps. Little cavities are thus formed, usually on the left side or the fundus of the bladder, in which the stone may lie hidden; so that it often happens, that in a second or third attempt the calculus cannot be detected by the finger, though passed in at once after the incision had made it distinctly sensible; the spasm having transported it to another locality. These little depressions can indeed be easily effaced by the finger, but are so rapidly reproduced, that the calculus again is hidden during the interval that elapses between the removal of the finger and the application of the forceps. Prolonged attempts of this kind render the operation painful, and expose the lining membrane of the bladder to injury. Although, in all the cases that have occurred to the author, he has been able to complete the operation favourably, he has had sometimes great difficulty in so doing. He has of late found the spasm yield in many cases, in a most decided manner, to the injection of water through the wound by means of a large syringe, having a long and rounded pipe.

Arrest of the Calculus at the Mouth of the Ureter.—Desault has published a case of this, in which he was obliged to incise the anterior wall of the ureter; and Mayor, of Geneva, related a similar case to the author, in which he had followed the same procedure. The present case occurred in a lad, æt. 13. It was intended to perform lithotrity, but, fortunately for the boy, the narrowness and irritability of the urinary passages forbad this. A calculus that was free in the bladder, being very friable, was easily crushed by the forceps; but intense spasm of the bladder came on, rendering the removal of some of the portions very difficult. During these manipulations another calculus was felt firmly lodged near the mouth of the left ureter, and all attempts at dislodging this caused great pain. The author convinced himself, by repeated examinations and the use of injections, that the calculus was not merely retained by spasmodic action, but lay within the mouth of the ureter. By seizing hold of the projecting part, and by repeated careful traction, he was enabled to extract this calculus entire. It was the size of a bean, having around its middle a circular depression, produced by the urethra, and a superficial grooving on one side, along which the urine had passed into the bladder. This was the longest of any of the author's lithotomy operations, occupying seventeen or eighteen minutes in place of two or three. The boy recovered without a bad symptom.

Another rare case, related by M. Riberi, is an example of the *Co-existence of Tumours of the Bladder, with Calculus.*—This occurred in G. Perotti, æt. 70, a man of athletic stature, and dissipated habits, who during his lifetime had suffered from various cutaneous diseases. As the urethra was capacious, it was determined to try lithotrity; but as the stone was found

to have a diameter of twenty-five lines, and to be exceedingly hard, the bilateral operation was substituted.   The neck of the bladder being divided to the extent of sixteen lines, a large stone was immediately felt, but could not be made to advance by means of the forceps.   Repeated attempts were made, and during a change of direction of these, a red tumour, larger than a fowl's egg, was thrust forwards into the wound, having a pedicle half as thick as itself, and six lines in length.   During the movements, the pedicle became lacerated, and the tumour, together with a large calculus, was safely brought away.   On exploring the bladder, another tumour, the size of a chesnut, having a very short pedicle, was also found, situated a little below the neck of the bladder.   With a very little traction, it, too, was separated.   No hæmorrhage took place.   The patient, after doing exceedingly well for three weeks, was seized, first, with diarrhœa, and afterwards with erysipelas; but although several times brought to death's door, he eventually completely recovered.   The tumours, on being cut into, presented a dense fibro-lardaceous tissue, possessed of little vascularity.   This texture, the sensibility of the tumour, and the absence of all hæmorrhage, distinguished them from polypi, which their pedicles at first caused them to resemble.

Backward Luxation of the Ulna alone.—This species of dislocation, first described by Sir A. Cooper, and since by Sédillot and others, is produced by a fall on the palm, and if not recognised, is followed by impaired use of the forearm.   The principal signs are, (1) Severe pain, due to the irritation of the ulnar nerve, and associated with numbness of the two last fingers; (2) flexion of the forearm; (3) impossibility of movement in the elbow-joint, while supination and pronation still continue; (4) shortening of the inner side of the forearm, and abduction of the hand; (5) deviation inwards of the forearm on the axis of the humerus; (6) increase of the antero-posterior diameter, and of the circumference of the joint; (7) osseous tumour at the bend of the arm formed by the inner condyle; (8) projection of the olecranon posteriorly, less, however, than in the dislocation of both bones, and much less in children, in whom the olecranon is naturally less projecting, which may lead to error in diagnosis; (9) the radio-humeral articulation usually uninjured.

Dr. Riberi has had many occasions to treat this dislocation, which occurs more frequently than is usually believed.   Fortunately it can be reduced long after its production, for commonly it is accompanied at once with so much swelling, as to render its detection often impossible until many days after, when this has subsided.   The author has reduced it six months after its occurrence.   Three cases that recently occurred in the hospital are related, all of which arose from the cause, and exhibited the symptoms, already stated; and all were readily reduced (one case having occurred forty-two days before) by the same means, the return of the bone being attended by a noise audible to all, and followed by immediate recovery of the movements of the joint.   Two assistants made extension and counter-extension, while the surgeon kept his left hand on the internal condyle and olecranon.   As soon as the condyle and ulna were brought, by means of the extension, to the natural plane of the joint, he, still pressing the olecranon downwards, grasping the forearm at its lower end, gradually

bent it upon the arm. In this way the reduction is aided by the action of the brachialis, and by the radius, which, brought during the flexion of the forearm against the external condyle, thrusts the humerus backwards.

Subungual Exostosis.—In vol. vii., p. 270, we gave an analysis of a paper by M. Legoupil, referring to all the cases of this affection on record. Dr. Riberi has met with ten such in his practice, but confines his account to two which were seen by all the pupils at the Clinical Hospital. He entirely approves of the resection of the exostosis as practised by Dupuytren, and blames M. Lenoir for having amputated the phalanx. Both the cases, like almost all others recorded, occurred in young women, and beneath the great toe-nail; and in both cases, tight shoes seem to have been the exciting cause. Whenever he has been able, the author has followed Dupuytren's advice of leaving the nail intact; but in some cases, from its covering a part of the tumour, this was impossible. He does not regard these tumours as true osseous productions, since their texture is areolar, and not fibrous; and they may be cut off without leading to any loss of substance in the bone itself. The disease presents itself with such distinct characters, that the slightest inspection should suffice for its detection; but still it has been confounded with warts and diseases of the nails. In one of the cases related, the tumour was sawn off, and in the other, removed by the actual cautery.

Speedy Cure of a Large Erectile Tumour of the Neck, by Injection of Aromatized Wine.—At the Scientific Congress, which met at Turin, in 1840, Dr. Riberi made a communication respecting the great benefits he had derived from vinous injections, in erectile tumours or *nævi materni*. He preferred these to any other substance that had been recommended as an injection. Although in the case of large tumours the injection had to be repeated at intervals, in different parts of the tumour, yet the cure was effected with little pain, and without leaving a cicatrix. The chief inconvenience was the length of time required; so that the ligature, excision, or caustic, might be preferable whenever the tumour had a large base, and the deformity left was not visible. Where this last was to be feared, as on the face, the injection was preferable. On many occasions, injections have been employed with success, when the whole substance of the lip was implicated; but in other cases, the ligature has proved just as useful, and more expeditious. Soon after the injection, the tumour becomes reddened, and the seat of a true phlegmon, which is, however, of short duration, and gradually subsides, the skin recovering its colour, though never assuming quite the natural condition. After this change in the tumour, the surgeon may be certain that it will not continue to increase; and it has lost the spongy or cellulo-vascular disposition, which was one of its chief anatomical characters. Sometimes the inflammation proceeds beyond the adhesive stage; and ulceration, which delays, but does not prevent, the cure, takes place. In some large tumours, the conjunction of other measures, as the partial ligature, caustics, &c., with injection, has been found useful. Since 1840, Professor Riberi has treated many other cases with the vinous injection, and with increased confidence in its efficacy. He selects one of these for narration, as being the most striking example he has yet met with, from the deep extension of the tumour, and the rapidity of its cure.

Maria Cedrino, æt 46, a countrywoman in robust health, applied at the Clinic, on account of a tumour on the right side of the neck. It was the size of a turkey's egg, colourless and painless, and its base penetrated amidst the muscles and large vessels of the neck. By uniform pressure it could be made to disappear, re-appearing again gradually when this was removed. It had remained stationary for some time, when a quack having punctured it, hæmorrhage ensued, to arrest which the actual cautery was required. Since then it had much increased in size. After steadying the neck and the tumour, this was punctured at its centre, by means of Anel's syringe, having the pipe-needle so shaped that it might readily penetrate, and wine aromatized by aromatic herbs was injected. The tumour at once became tense and hard, and bladders of ice were laid upon it. Considerable inflammation followed, so that a bleeding was deemed necessary. By the use of the ice and a low diet all this subsided, and the tumour rapidly diminished in size—so that by the seventeenth day it was no larger than a pea, and the patient was dismissed. She was seen a year after, and continued quite well.

In a note appended to the case, Professor Riberi takes a comparative view of the different procedures adopted for the treatment of erectile tumours. 1. The practice of tying the arteries supplying the tumour with blood, and thus arresting its nutrition, has been declared useless by the general experience of surgeons, except in the case of erectile tumours in the cavity of the orbit, and perhaps of the temple.—2. Other modes, as vaccination, acupuncture either simple or with heated needles, injections, issues over the tumour, and the seton, act by inducing inflammation, which changes the anatomical characters of the texture in the tumour, and leads to its resolution. Vaccination, however, only answers this end in very small tumours, and its sphere of action is therefore very limited. By acupuncture practised with many needles, a cure is not always obtained, and when it does result, it does so only after a very long period : and the practice of employing heated needles has not succeeded in the author's hands. Of injections he has already stated his favourable opinion. Issues applied over the tumour produce no effect, or only after a very long period. Nearly the same may be said of the seton, while from the use of this means, the author has known excessive inflammation and febrile reaction to result.— 3. Other modes, as compression, and to a certain extent, astringents, accomplish the *denutrition* of the tumour by causing expulsion of the blood and adstriction of the vessels. Compression, however, by inducing some inflammatory action, operates in a mixed manner. Astringents are of little efficacy, and act as mere adjuvants to other means. Compression is long and tedious in its operation, difficult, and often impracticable in application, and always uncertain in its result. In one case Dr. Riberi employed it with great advantage, before he was aware of the utility of injections. It was an erectile tumour, which spread out from the orbit of an infant over the forehead. Pressure was continued for a year and a half, with the effect of quite dissipating the tumour, and wearing down in part the superciliary ridge, which, however, was restored to its usual appearance in three months afterwards.—4. Another mode is the minute breaking-up of the intimate tissue of the tumour by a cataract-needle, and then employing pressure and astringents to secure its dispersion. The

facts are too few to allow of an opinion being delivered upon this method.
—5. Finally, other modes, as excision, caustics, and the ligature, have in
view the extirpation of the texture. Excision is the most speedy means
of effecting this; but the fear of hæmorrhage, so alarming at the tender age
at which these tumours usually occur, deters most practitioners from its
adoption. Of the efficacy of both the actual and potential cautery, the author
has had many opportunities of assuring himself; but the ugly scar they
leave behind constitutes a great objection to their use. The ligature is
much more extensively applicable. Of 28 cases occurring in 8 years, Dr.
Riberi applied it with complete success in 25. In treating nævi with a
broad base, instead of employing a double ligature, so as to tie each half of
the tumour with one of these, the base of the tumour may be transfixed by
two needles placed cross-wise, and a circular ligature applied beneath them,
they serving as points of support for it. In place of the needles, Dr.
Riberi has been accustomed to employ here, as also in tying hæmorrhoids,
two strongly-curved hooks. It is surprising how much more readily the
operation is performed by their aid, especially when the tumour is situated
within the cavity of the mouth. In those cases in which the erectile
tumour has a long, narrow base, the quilled suture may be employed with
great advantage. There are, finally, certain erectile tumours, which, from
their position or size, require to be treated by two or more of the above-
mentioned means.

Case of Excision of the entire Female Urethra.—This operation, which,
as far as Professor Riberi is aware, is *unique*, was performed upon a lady
in her fifty-eighth year, who, from her girlhood, had suffered more or less
from a tumour of the orifice of the *meatus urinarius*. Of late years it had
caused great suffering, and had given rise to frequent retention of urine,
for the relief of which, the catheter could only be introduced with great
difficulty. Worn out with suffering, she sought for its extirpation. The
urethra was found projecting from between the labia, and so enlarged in
size that the index finger could hardly be introduced into the vagina.
The vaginal surface of the urethra was found hypertrophied, and from the
lower and inner border sprang a large, hard, fleshy excrescence. The pain
was excessive, and the urine passed with great difficulty. The clitoris
and nymphæ were implicated in the disease.

It was determined to remove the whole of the diseased parts; and the
patient being placed on her knees and elbows, the tumour was isolated on
the right and left by two incisions; and on account of the great flow of
blood, and the contortions of the patient, all the rest of the operation was
guided by the sense of touch alone. The tumour having a much greater
lateral direction than was anticipated, the operation proved long and
painful. The period of the operation in which the tumour, isolated on all
sides, remained only in connexion with the neck of the bladder, was a very
critical one, for the patient was already enfeebled by the hæmorrhage,
which yet continued profuse, notwithstanding the injection of iced-water.
After dividing the urethra, the operator was lucky enough to be able at
once to insert a catheter through the portion that remained, and then to
arrest the hæmorrhage by plugging: but in any similar case he would
recommend that a female catheter having a groove on its exterior, should

be introduced into the urethra before this is divided.   A straight knife is then to be passed along the groove, so as to open the urethra as far as the disease extends, where it may be cut off upon the catheter. The excision in this way would be made more easily, the catheter is in the bladder to ensure the escape of urine, and the vagina can be completely plugged to arrest hæmorrhage.   The portion of the urethra excised measured thirteen lines— that is, it was the entire canal, as this in women measures from twelve to fourteen lines.   It was much hypertrophied, and in part degenerated into scirrho-lardaceous tissue.   On the twelfth day the catheter and plugging were removed, to the great relief of local irritation.   On passing-in the finger, a kind of *triangular valve*, having a thin apex and thick base, was felt hanging within the vagina, and interposing itself as a partition between the cervix uteri and the aperture of the bladder.   This proceeded from a prolongation of the natural uterine-vaginal fold; and as the process of cicatrization went on it became drawn upwards into the natural seat of the urethra, and there remained adherent, so as to form, so to say, a *new urethra*, placed scarcely more than two or three lines behind the place of the normal canal.   By the projection forwards and adherence of this valve, the neck of the bladder and adjacent segments, at first free and moveable within the pelvis, re-acquired their normal fixity to the arch of the pubes.   The urine came away involuntarily for twenty days, after which the power of retaining it was gradually and completely acquired.

The disease re-appeared in the locality of the operation, and in the inguinal (which were somewhat enlarged prior to the operation) and ileo-lumbar glands; and the patient died amidst dreadful suffering two years afterwards.

On the Best Age for the Performance of the Operation for Hare-Lip.— Adverting to this much-debated question, Dr. Riberi expresses himself an advocate for not resorting to the operation too early.   Notwithstanding the great number of high authorities who counsel it, and the fact that he has many times performed it himself with success at the earliest age, a more matured experience has proved to him, that not unfrequently the margins of the hare-lip, which have been made to unite by the twisted suture in young infants, give way, and the fissure in the lip returns, though not so extensively as at first.   Hence, for many years past he has pursued the plan of delaying the operation in cases of simple hare-lip, which produces no other inconvenience than the deformity, the nasal tone of voice, and the like.   He operates shortly after birth, however, in cases in which the hare-lip impedes the use of the parts, as in sucking, and when it is complicated with fissure of the palate, the closure of which is much favoured by the junction of the lip.   Examining into the cause of this giving way of the lip after operation, Dr. Riberi believes it may much rather be attributed to the nature of the suture employed, than to the restlessness and screams of the child.   By substituting the quilled for the twisted suture, he has of late years been able to obtain more prompt and durable union; and he has succeeded with it in four cases, in which its rival had failed of success.

Art. XII.

1. *Leçons Orales sur les Phrenopathies; ou, Traité Théorique et Pratique des Maladies Mentales. Cours donné à la Clinique des Etablissements d'Aliénés à Gand.* Par J. Guislain, Professeur à l'Université de Gand.—*Gand,* 1852. 8vo, tom. III.

   Clinical Discourses on Phrenic Diseases; or, a Theoretical and Practical Treatise on Mental Affections. Being the Clinical Course delivered at the Institutions for the Insane at Ghent. By J. Guislain, Professor in the University of Ghent.—3 vols. 8vo.

2. *Articles on Reform in Private Asylums.* By Henry Monro, M.B. Oxon. &c., Author of ' Remarks on Insanity, its Nature and Treatment.'—*London,* 1852. 8vo, pp. 112.

The work of Professor Guislain is the most complete treatise on Insanity we have met with. It is eminently practical; it is comprehensive; and being the result of many years' experience in the management of the insane, it is of considerable authority. The writer had personally felt the want of sound knowledge on the pathology and treatment of insanity, when he first undertook the duty of medical superintendent of the institutions for the insane; and, as a thoughtful observer, he could not but remark how lamentably defective in that kind of knowledge the medical profession generally was. Impressed with the importance of this class of diseases, with the gravity of their relations, and with the general neglect and ignorance of the whole subject, he perseveringly laboured to establish a school of clinical instruction at the asylums under his direction, and finally succeeded. His course is delivered alternately in the establishment for males, and in that for females. One lecture is delivered every week throughout the year, and each continues two hours. Twenty students who have passed their first examination for the doctorate, constitute the class, who at each lecture are introduced into one or other of the wards. When the architectural arrangements and details are being considered, they are taken over the whole establishment, but not at any other time. Professor Guislain's rules for their conduct are very simple : they are—" Be prudent. Ask no indiscreet questions of the patients. Do not address them or examine them until invited by the lecturer. Do not agitate them by conversation or looks, or make them feel that strangers are present." It has been found that, so far from the lectures thus conducted causing the slightest inconvenience, they have actually benefited the patients.

The practical style in which the work is written, induces us to give an analytical review of it; at the same time, however, our notice must be also partly critical, as we shall have to express our dissent on some points mooted by Professor Guislain.—The first lecture contains directions as to the examination of a lunatic, and M. Guislain takes the opportunity of pointing out the difficulties to be overcome. He examines his patients under five different relations. First he notes the physiognomy, then the gestures and language, the state of the viscera, and the history of the case. To judge as to the meaning of the phenomena, tact and the experience from which tact must spring, are alike necessary. Nor will one or two

examinations of the patient suffice; for there are cases which should be
observed for several days, or even weeks, under every variety of circum-
stance, before a decision is come to, particularly in medico-legal inquiries.
No one can have looked at a number of insane persons, without noticing
the peculiarities of *expression* or gesture which they present. On the
point of facial expression—" le masque de l'aliénation mentale," as he
terms it—Professor Guislain is very emphatic, and has given several
sketches of patients in illustration of his remarks. This pathological
physiognomy is founded upon the normal physiognomy. The colour of
the skin—the complexion—of the insane is sometimes peculiar; and when
it continues of a *bistre* tint, indicates that the patient is not really well,
although the more obvious symptoms have disappeared. *Pallor of the lips*
indicates intense passions. The hair undergoes significant changes in
insanity; so, also, the mode of speaking or pronouncing words. The
position habitually taken by the patient, or, in other words, the action of
muscles or groups of muscles, is very significant. Thus, when the indi-
vidual refuses to sit on a chair or bench, but sits doubled up, his chin on
his knees, recovery is very doubtful. The drooping of the head forwards
is one of the earliest signs of incurable imbecility. Sometimes a tension
of the muscular system is observable, and is best felt by taking the hand
or arm of the patient, when a peculiar sense of rigidity and resistance is
felt. Partial paralysis of the muscles of the face or extremities is rarely
observed; more frequently, great increase of strength, and a more perfect
co-ordination of all the movements. These often usher in a violent attack
of mania.

The mental condition, as manifested in conversations, is always an im-
portant point. Professor Guislain gives illustrations as to the mode of
conducting these in the various forms of insanity. Certain words may
serve as guides to the examination. Thus, *why* measures the degree of
intelligence, or the reasoning faculty—e. g., " *Why* are you here?" *How*,
the faculty of causation. *Since, when,* detect failures in the memory.
How much, tests the faculty of enumeration. The attention may be tried
by instructing the patient to perform certain simple acts, or noting whether
his eye catches that of the physician when he is addressed. *Letters*
written by the patient supply very important indications of his state of
mind.

In the *definition* of insanity, Professor Guislain finds the same difficulties
as other writers. He defines it as a chronic, apyretic disease, in which
the ideas and actions are subjected to an irresistible power. The indi-
vidual has experienced a change in his mode of feeling, perceiving, think-
ing, acting,—in his habits and character. It is a condition which contrasts
with the feelings, thoughts, and acts of those around him; it is an affection
which makes it impossible for him to act for his self-preservation, and as a
being responsible to God and to society. Or, more briefly, Professor
Guislain defines insanity to be " a derangement of the mental faculties—
morbid, apyretic, chronic—which deprives the individual of the power of
thinking and acting freely in reference to his happiness, preservation, and
duties." It is in this constraint that the true idea of insanity consists. A
diseased condition constrains or impels the man to perform actions injurious
to himself and to society. But this definition only applies to the *developed*

form; at the commencement there is a mixed condition, in which there is a certain amount of reason and self-control. It is manifest that a definition like this may have a very loose application to men's actions; Professor Guislain, therefore, devotes a portion of a lecture to the consideration of those mental states which ought not to be confounded with insanity, and he considers in succession various classes of men who display great irregularity or eccentricity of conduct. Coxcombs, or the fools of society; disturbers of public order, or men of an insurrectionary temperament, with subversive ideas; self-made martyrs, or the cenobites of the cloister, Trappists, &c.; reckless debauchees, male and female; *felo-de-se's* or *suiciders*, as Professor Guislain more correctly terms them; misers, thieves, murderers; visionaries of every kind,—all these may be confounded with the insane. We cannot, however, see that Professor Guislain throws much more light on the subject than we already possess. His remarks are just as far as they go, but they leave us pretty much in the old position, so that madmen may still go at large, and be treated as responsible beings, to the terror of society and the degradation of the law.

Yet it surely is not impossible to define insanity, and so indicate an insane person that he shall not be confounded with the vicious or criminal. To say, with many writers, that insanity is a derangement of the mental faculties, is to propound a mere periphrasis; and to say that the lunatic is deprived of self-control, is to state a truism. But such propositions are equally applicable to men who act under the blind impulse of the passions, or recklessly follow their evil propensities—men who deliberately say, Evil, be thou my Good. Now we think it certain, that the definition of insanity must include the idea of *corporeal* disease, for (as Feuchtersleben very correctly observes) mental disease, independently of a bodily substratum, is error, or sin—not disease. We should, therefore, incline to modify Professor Guislain's definition, so as to include this idea, and say—*Insanity is a chronic disorder of the brain, by which the mental condition of the individual is so modified that he is deprived, wholly or in part, of common sense.* Whenever the medical practitioner has to decide whether an individual placed under his notice be or be not insane, he has, in fact, to determine whether or not there be functional or structural disorder of the cerebrum of that individual. That is, in fact, the whole question. The irrationality and folly displayed in the man's acts *may* only demonstrate the irrationality and folly of the man, not his insanity; it is only when the existence of functional or structural disease of the cerebrum is demonstrated, and the irrational and foolish acts of the individual can be referred to that disease, that he can be pronounced insane.

Not a few writers have maintained that insanity does not depend upon cerebral disorder. In support of this assertion they have adduced various arguments:—First, they have advanced all those facts which tend to show that the normal mental operations are independent of cerebral action, and *à fortiori*, that derangement of these operations may be equally so. It is with some surprise that we find Professor Guislain giving credence to inferences of this kind. That there may be considerable destruction of the encephalon, or rather, of the cerebral convolutions, without at least a *striking* modification of the mental operations, is well established; but it by no means follows that the *whole* brain may be wanting, with the same result.

On the contrary, it is established, by myriads of facts, and, indeed, by every day's experience, that cerebral diseases directly modify the mental faculties. The instances we refer to are extremely interesting, nevertheless, as indicative of the extent to which we are yet in the dark as to the *exact functions* of the various portions of the encephalon; but to infer from thence that insanity may take place without any functional or structural disorder of that important and marvellously constructed viscus, would be to discard all the teachings of general and comparative neurology. Professor Guislain, following up the inference, argues in the style customary with the so-called immaterialists, as to the absurdity of referring the varied powers of the human intellect to the operation of fibres, cells, and nucleoli, and deduces the general doctrine of a something imponderable, invisible, &c., to put in motion these fibres and cells, and to play upon them as the pianist moves the keys of a piano or organ. Feeling as strong convictions as Professor Guislain on this particular doctrine of metaphysics, we apprehend nothing but doubt and scepticism from the line of argument which he has adopted. If our able author will look forth into the GREAT WORLD OF LIFE, he will get his eyes opened to see the wondrous works of that Spirit which operates IN ALL and THROUGH ALL; and will note that even the Divine Mind is manifested in its greatest terrestrial beauty, and glory, and perfection, by means of these same nuclei and cells and fibres. How else can he explain that admirable mechanical skill which is visible in every created thing, from the lowliest to the highest—those hydraulical, and acoustical, and optical, and electrical machines, developed in such wondrous diversity, adapted to surrounding circumstances with such wondrous fitness, and ministering to the conservation and pleasure of countless myriads of creatures with such incessant benevolence of design? All these require animal and vegetable tissues for their manifestation which are as really inscrutable in their working as the human brain itself.

Another argument against the *cerebral* pathology of insanity is based on the fact, that no *structural* change can be detected in the cerebra of numerous individuals deceased in a state of insanity. If we investigate the subject, however, aided by the lights of comparative psychology, we must come to the conclusion, that in the more perfect and more exquisite forms of mental derangement, it is *impossible that there can be any structural disease*—at least, in that part of the cerebrum which is implicated. When the mental faculties—one or more—are abolished, we might reasonably look for such a change; but when they are only perverted, the very fact that the mind still acts, proves that the organ is not structurally, but only dynamically modified. Illustrations of this proposition may be found in all those morbid mental states induced by the presence of a toxic agent in the blood, or by powerful external impressions on the sensorial nerves. These are multiform; and it is highly probable that it is only after a prolonged period of dynamical derangement, that structural change occurs.

Having thus cleared the way, we are in a better position to consider some important points in the general pathology of insanity. Following Professor Guislain's taxonomy we come to the *arrangement* of mental disorders. He very accurately points out the indefiniteness and paucity of the terms used to designate the different forms of unsound mind; and, after dwelling on the necessity of a better nomenclature, proposes a new

terminology. He takes the Greek word *Phren* as the basis, rejecting *Psyche* as disagreeable to the ear and indefinite to the understanding,— and passing over *Kephale*, because it is not a disease of the encephalon, the head, that has to be treated of, but " functional affections of the domain of the ideas, the feelings, the passions." We have, therefore, a number of words of which *Phren* is the radical—as, *Phrenopathy*, mental disease—*Phrenalgia*, moral pain, melancholy, lypemania—*Idiophrenia*, delirium—*Paraphrenia*, eccentricity, madness (*folie*)—*Hyperphrenia*, exaltation of intellectual acts, mania, and numerous others. Professor Guislain would not, however, discard well-known and established terms, as melancholy, mania, insanity; he would only use them in a more definite sense. He would say, generally, monomania, to indicate a partial disease; but to indicate a more precise meaning, he would use the phrases, monomelancholia, monophrenalgia, monodelirium, or polymania, polymelancholia.

Having thus explained his terminology, Professor Guislain proceeds to his taxonomy. He recognises elementary and compound forms. The elementary are six—namely, 1. Melancholia or Phrenalgia; an exaltation of the feeling of sorrow. 2. Extacy or Phrenoplexia; a " commotion morale," or moral shock, a suspension of the intellectual acts, with " *roideur generale*"—we give the original, for we frankly confess we cannot accurately render the author's meaning into English. 3. Mania or Hyperphrenia; an impassioned exaltation of the moral feelings (*du moral*). 4. Eccentricity, Vesania, or Paraphrenia (*folie*); anomalies of the impulsive will. 5. *Delirant* insanity (*delire*), or Ideophrenia; anomalies in the ideas. 6. Dementia, or Aphrenia; loss or abolition of the moral and intellectual powers. Each of these may occur as a simple or compound form; if the former, it is a monomania or monophrenia; if the latter, it is a polyphrenopathia, polymelancholia, polymania, &c. They may be combined with each other, as, for example, angry excitement may be combined with melancholy, or with eccentricity, or with dementia, &c., the combinations being almost infinitely varied. In each case, there is, however, a fundamental characteristic, and it is named accordingly. Thus, there may be maniacal melancholia, or melancholic mania, just as the one or the other is predominant. In like manner, Professor Guislain would speak of a delirious or hallucinate mania (perhaps *delirant*, as used by the French, would be better applied to the thing, reserving delirious for the person), or a mania with insane ideas—of mania with dementia, or dementia with mania, &c.

If we bring this arrangement of the pathological phenomena into comparison with the physiological, we find it altogether incomplete. The brain is a compound organ. This, as a disciple of Carus rather than of Gall, Professor Guislain fully acknowledges. As a compound organ it corresponds in its details to the different manifestations of the mind. Now these manifestations are of very various complexity and widely different nature. Without laying any stress whatever upon the mere cranioscopy of Gall and Spurzheim, we think comparative physiology and natural history afford us ample data for separating the faculties of the mind into the three great groups, of the instinctive, the emotional, and the intellectual. The love of life is the fundamental instinct, then the enjoyment of pleasure—the perversion of which is melancholia in its most general

sense.   The conservation and the well-being of the individual, and the conservation and well-being of the species, are maintained by fundamental instincts, to which numerous minor instincts are subordinate.   Grafted upon and growing out of these, we have the emotions and passions; and springing from these latter—more or less directly—the intellectual operations.   The mere instincts are wholly impulsive in the lower animals; they may or may not be accompanied by desire; they operate without a consciousness or knowledge, on the part of the animal, of the ends which the acts they excite are adapted to attain; if they be accompanied by a desire, it is a blind desire, and is a mere feeling of a pleasure to be attained, or a pain to be avoided, by the completion of the instinctive act, without any reference to the result.   When the instinctive acts rise into the emotional or pathemic, there is a consciousness or knowledge, more or less extensive and complete, of the impressions which excite the emotion, and of the ends to be attained.   There appear, also, to be more vivid feelings of pleasure and pain; it is indeed in this, the lowest class of mental acts, that we first can undeniably recognise the presence of these feelings.

A sound classification of mental disorders should, we think, not only be based on a physiological classification of mental phenomena, but should have reference also to those morbid conditions to which the nervous system is noticed to be subject.   If we take the nerves of motion and sensation, or the centres of reflex action, we note that they are subject to disordered actions, the phenomena of which are best grouped under the heads of (1) exaltation or exaggeration of function; (2) perversion; (3) abolition.   There may be increased motor action, yet not irregular or perverted; perverted motor action, as in chorea; abolished motor action, as in paralysis.   So in mental disorder an instinct may be exaggerated—e. g., the maternal instinct; or it may be abolished, when the offspring is simply neglected; or it may be perverted, and infanticidal monomania result.

Having premised these remarks, we will place Professor Guislain's classification *in extenso* before our readers.   We have already mentioned his principal heads—his *genera*—namely, (1) melancholia; (2) extacy; (3) mania; (4) chronic mania (*folie*—vesania); (5) delirant mania (mania with hallucinations); (6) dementia.   Melancholia presents itself under two forms: general, or polymelancholia; special, or monomelancholia.   In the latter are comprised—1. The *melancholia sine delirio* of Etmüller, synonymous with the moral insanity of Prichard, the melancholia simplex of Heinroth, the *monomanie affective* and the *lypemanie raisonnante* of Esquirol.   It is (to use Guislain's words) " a pathological emotion, a sorrow, a chagrin, an anxiety, a fear, a terror, and nothing more."   2. Fear regarding the health, pathophobia, or monopathophobia, known as hypochondriasis, nervousness, &c.   Guislain has two varieties, the corporeal and the mental; the former the common hyppishness, the latter, a true melancholia, characterized by all kinds of vague fears, not having particular reference to the bodily health. 3. Nostalgia, or a violent sorrowful desire to return home.   4. M. Amatoria.   5. M. misanthropica.   6. Melancholia with anguish (*anxieuse*) or pneumo-melancholia.   7. Religious melancholia, including demonophobia and melancholia with despair.   The complex forms of melancholia are— 1. Melancholia with mania.   2. The suicidal.   3. The homicidal.   4. Sito-

phobia, or refusal of food. 5. Delirium melancholia, or with hallucinations. The fundamental characteristic of all these forms of melancholia is a painful condition of the mind—mental pain, or phrenalgia.

Mania is subdivided as follows:—1. Tranquil mania; the *mania sine delirio* of Pinel, and " moral insanity" of Prichard and other English writers, or maniacal excitement. 2. Argumentative mania, the " mono-manie affective" of Esquirol. 3. Astute, malicious, cunning mania. 4. Kleptomania. 5. Monomaniacal avarice. 6. Monomaniacal extravagance. 7. Oinomania, or *mania ebriosa*. 8. Nymphomania and Satyriasis. 9. Joyous mania, mania saltans. 10. Amenomania, a variety of the latter, characterized by the most extreme urbanity. 11. Mania of vanity, the Narcissus mania. 12. Mania of ambition, pride, grandeur, riches. 13. Logomania, a " diarrhœa of words." 14. Mania of peevishness, and perversion, and fault-finding (*tracassière*). 15. Mania errabunda. 16. Insurrectional mania, mania agitans. 17. Destructive mania, including furious mania, and combative, homicidal, suicidal, and incendiary mania (pyromania). To these add epileptic mania, mania with *folie* or insane impulses, mania with dementia, and general mania, or polymania. The fundamental characteristics of the maniacal group are exaltation of one or several of the mental faculties, a state of excitement, and sometimes the manifestation of active and violent passions.

Under the term " folie" (which we think is best translated by vesania, or chronic mania), Professor Guislain includes what has been lately termed moral or impulsive insanity; also instinctive monomania. It is a mental disorder, in which the individual is impelled irresistibly to act capriciously, without reference to any directing ideas, emotions, or passions, but usually associated with depression of the feelings. It is either special or general. The special forms are—1. The Tearers with the teeth. 2. The Pickers; those who pick, or cut, or tear things to pieces. 3. The mutilators. 4. The suiciders. 5. The Homicides. 6. The Incendiaries. 7. The Stupid. 8. The Mute. 9. The Fasters. 10. Perverted taste (eating excrements). 11. The Daubers and Scribblers. 12. The Concealers. 13. The Stealers. 14. The Grubbers (as a mole—" talpa-folie"). 15. The Talkers, including orators, declaimers, dialogists, monologists. 16. The Bawlers, including howlers and imitators of animal-sounds, as of birds, dogs, cats, &c. 17. Gesticulators and Grimacers, including those who perform fantastic movements, or assume extraordinary attitudes.

Delirant Mania, or with hallucinations, includes a number of chronic forms of insanity, in which the individual has erroneous conceptions or ideas of things, and which he considers as real or true. This delirium is " monophrenia," when the patient's erroneous conceptions are limited to one thing, or a group of things, his mind being sound on other points. When it is *general*, it is rather a symptomatic affection. The forms placed in this group are—1. The Accusers (" accusateurs") a well-known class; these are constantly accusing persons of attempts upon their health, ease, or property, generally by secret and insidious means—as electricity, poisons, spies, conspiracies, &c. 2. The Erotic: in this group are comprised females who erroneously think they are married, have husbands, &c. Guislain terms this a *uteromonodelirium*. 3. The Religious, including hallucinations as to the person and faculties in reference to religion, the

Deity, Jesus Christ, prophets, universal benefactors, &c., abounding in asylums; and Demonophobia, or Monodemonolatry, in which the individual believes himself the subject of demoniac or satanic influence.   4. The Ambitious, as emperors, popes, kings, &c.   5. The Hypochondriacal; those with hallucinations as to the state of their body, or of their viscera, and who think they have lacerations and other injuries of their viscera or body generally.   6. To this class belongs a group placed under the title of " The Metamorphosed," in which the patient thinks various things have undergone a change, as the house, persons, money, days of the week, parts of their bodies, or that they have changed into beasts (Zoanthropia).   An exaggerated idea of things is often present; as when a patient thinks he is infinitely rich, or that the size of his body, or portions of his body, is indefinitely increasing.   7. The " Pyrophobic," or those who believe they are surrounded by flames, is another form.   8. The Incendiary.   9. The Homicidal.

Professor Guislain divides dementia into the ordinary forms of *amentia*, fatuity, imbecility, stupidity.   It may be *complete* or *incomplete;* it may be *true* or *false;* it may be *general* or *special*, or accompanied with *incoherence of ideas;* it may accompany or terminate any of the forms of insanity previously described, or it may be connected with general paralysis.

On taking a general view of this arrangement, one cannot but be struck with the great resemblance between forms of mental disorder which are classified apart; or, rather, it cannot but be seen that diseases classed as distinct are only varieties of the same affection.   If, indeed, we were to adopt Professor Guislain's principles of classification, we could add to the number almost indefinitely; inasmuch as the hallucinations in the ideas, and the groupings of deranged instincts, feelings, and faculties, are as numerous as the individuals.   Further, one or two important forms of monomania are not noticed at all—namely, insane sodomy, insane modesty, &c.   As a sound taxonomy is all-important to the progress of science, we feel that we ought not thus to criticise the method of so experienced and able a writer as Professor Guislain, without suggesting something as a substitute. With these convictions, we venture to occupy a page or two with some remarks on the classification and arrangement of mental disorders.

In the first place, we would observe that any scheme of classification should be founded on a natural method, so as to help the practitioner to discriminate, not merely the varied forms of mental disorder, but also, as far as possible, their etiological relations and natural history.   We observe, for example, that the instinct of love of life or of offspring with one or other of its subordinate instincts, is morbidly excited, leading to curious combinations of phenomena and remarkable manifestations of mental disorder.   It will be well, therefore, to determine the group of *normal* instincts; then the circumstances under which they are brought into operation naturally; next, the principal relations of the subordinate groups to each other; and after this, review the pathological and etiological phenomena.   Now to this end it will not be sufficient to limit our inquiries to *human* nature; the vast field of comparative neurology and psychology must be explored.   It need not be urged upon *our* readers that the instincts of man are in no respect different, as to their relations to the brain and nervous system, from those of their fellow-creatures amongst

the lower animals.   It is only in their moral and spiritual relations that a difference exists.   Now, since this is the fact, and since even comparative pathology (for infanticidal and suicidal monomania are observed in brutes) may assist us to unravel the great problem which this most lamentable of all diseases offers to our contemplation, it would be most unscientific and most unwise to begin otherwise than with the beginning.

The instincts may be classed under the four heads of self-preservation, self-defence, propagation of the species, preservation of the offspring.[*]   In each instinct, in animals endowed with sensation, there is a powerful desire or aversion (or both), obscure in its nature and unconscious of its object, but acting irresistibly so as to impel the animal to do certain acts determined by a law of its nature, or rather by the constitution of the organism, the objects of which are those we have mentioned.   This subjection of the animal organism to an irresistible impulse is self-acting, one instinct being subordinate to or controlling another.   In the higher animals, in which something more is present than a strong desire or aversion,—in which, in fact, there is more or less perception of the ends to be attained and the means to be used,—the instincts are subjected, in varying degree, to this higher mental power.   In the highest animal, man, the subjection of the instincts, and of their derivatives the passions, to the *moral* powers, constitutes the difference between him and the irresponsible animals below him; and when this control is so lost by cerebral disease that the instinctive and emotional impulses come into unrestrained operation, responsibility ceases, and a series of phenomena like those characteristic of the lower animals arises.   Hence, we are led to compare the forms of insanity characterized by a blind desire impelling the individual to sudden and insane acts, with the manifestations of the instincts in the lower animals.   We say the *lower* animals, because in the normally-constituted man—of civilized life, at least—we have no grounds of comparison. Perhaps good illustrations might be drawn from man in the more savage state—that is to say, in more immediate contact with the external world, and unaided by those appliances which recorded experience and science afford to the civilized man; but it will, perhaps, be better to use these only incidentally, preferring the simpler illustrations which the unrestrained instincts of irrational creatures present.

The instinct of self-preservation, or the perfect continuance of life, consists in a strong desire, having for its object the preservation and well-being of the organism.   It is the fundamental instinct, and includes the instinct of self-defence, which is founded on strong aversions for anything which will cause the destruction or ill-being of the organism.   The object of these two instincts is attained by an antagonistic mechanism; the one seeks to obtain, the other to repel.   The primary subordinate instincts of the former are the instincts for food and drink, and for light and air.   In insanity, these instincts may be exalted, perverted, or abolished.   Accordingly, we find the following forms:

Instinct for food—its modifications in insanity.

Exaltation or exaggeration of the instinct gives us—1. *Bulimia*, or enormous appetite; 2. Appetite for flesh-food; 3. Appetite for vegetable food.

* Unzer's Principles of Physiology, Sydenham Society's edition, p. 130.

Perversion of the instinct gives us—1. *Pica*, in its various forms; 2. Appetite for carrion ; 3. Appetite for excrement.

Abolition of the instinct for food gives us that common symptom, the absolute refusal to take food.

In the *instinct for drink*, we have *exaggeration* manifested as *thirst;* there is often *perversion*, giving rise to oinomania, or the appetite for intoxicating drinks ; and its *abolition* is seen as the hydrophobic mania, described by some writers. These forms will commingle with each other, or with modifications of other instincts. Thus, Professor Guislain remarks that the refusal to take food sometimes alternates with bulimia ; he has never, however, met with a case of hydrophobic mania.

The instinct for *light and air* is connected with another—namely, the instinct for movement of the voluntary muscles. Its *exaggeration* is seen in the constant exposure to the sun, so noticeable amongst the insane ; its perversion and abolition, in the refusal to leave their bed or their room— often manifested alternately with the former.

The instinct of *self-defence* is founded on aversions. Its fundamental acts are combat and destruction. Its exaggeration is one of the most common characteristics of mania ; its abolition, of dementia ; its perversion, of monomania, vesania, or *impulsive* insanity. This group, therefore, includes furious mania, and those forms of mischievous, combative, homicidal, suicidal, and incendiary monomania, in which there is no special object of aversion, but only an irresistible desire to destroy mischievously, to fight, kill, burn, lacerate, mutilate, or commit self-destruction. To this group belong the tearers with the teeth, the " pickers," the daubers, &c. A subordinate instinct, but an important war-instinct, is that of cunning, especially of destructive cunning ; this is so constantly associated, in one form or other, with the preceding, that it is one of the leading characteristics of the insane.

The reader will probably go readily along with us in this broad outline of the relation of the more important instincts to insanity. There are some of the subordinate class, which perhaps he will hesitate to place in the same category, until he has observed and reflected on the phenomena, natural and morbid. We refer to the group of instincts which provide for the care of the person, for the regulation of animal temperature, the cleanliness and comfort of the surface, the provision of a home, and for the wants of the future. In all these, we have morbid changes developing very remarkable forms of insanity. They are instincts which may be termed the artistic, as they are the basis of nearly all the arts of life. They may be grouped under the general terms of love of life, or self-love ; they consist essentially in a blind desire for ease and pleasure, and a blind aversion for things which will cause disease, or pain, or annoyance. We can only mention some of the more striking of the group. The desire for *warmth* involves clothing ; in the impulsively insane this fundamental instinct is occasionally exaggerated, but is more frequently perverted or abolished. The individual continually throws off all clothing, whether it be the body- or bed-clothes ; he is alike regardless of heat or cold, and will thrust his foot into the fire or into ice with equal indifference. The instinct for *cleanliness* is almost always modified ; vermin are allowed to accumulate ; the fæcal evacuations are passed anywhere, or disregarded ; or, contrarily, the instinct is wearisomely

exaggerated. The desire *for home* may be morbidly developed, and then appears as nostalgia; or is abolished, and gives rise to the mania errabunda, the vagabond monomania—a very singular perversion. This may, however, be also considered as the morbid development of the migratory instinct. A provision for *the future* is almost a universal instinct. In the insane it is often exaggerated, and gives rise to vague and suicidal fears for the future; or it is perverted, when insane hoarding is manifested; or it is abolished, and no regard is had for the morrow. Modifications of this fundamental instinct almost always accompany morbid development of the other general instincts. Another instinct of this group is the instinct for *movements* of the voluntary muscles, and of the groups of muscles subservient to the various instinctive acts. The playfulness and mischievousness of young animals is often reproduced in the insane; so also many instinctive acts, as running, springing, climbing, picking with the fingers, &c.—acts purely automatic and impulsive, and of which almost any asylum will afford examples. To this group belong the exaggeration of instinctive cries, giving rise to the howlers, barkers, &c. ; and to logomania—the diarrhœa of words—or else to their abolition, when dumbness is manifested, or the patient is obstinately mute. We may also class with this group the *imitative* movements of the insane—an instinct very prominent in many of the lower animals, and often combined with playful cunning or mischievousness.

The preservation of the race is, in the scheme of nature, of equal importance with the preservation of the individual. Hence we have, in reference to the propagation of the species and the preservation of the offspring, a reproduction of the various instincts we have glanced at. But as the congress of the sexes is necessary in the first instance, and as a new being results therefrom, having special relation to the parents, the group of *social* and *family* instincts comes into notice. The group resolves itself into the two primary instincts of physical love and maternal love; to these there are numerous instincts subordinate; and based upon the whole are a large and varied class of passions, emotions, and affections of the mind. The impulsive forms of insanity of this class are—firstly, those common to the sexes; secondly, those peculiar to each sex. To the former group belong—1. *Inordinate exaltation* of the sexual instinct—erotomania and nymphomania; 2. *Perversion*, as masturbation, sodomy, congress with impubescent youth, senile pruriency; 3. *Abolition*, the insane misogynist and virago. Peculiar to the males are combativeness, and the *narcissus* mania; peculiar to the females, idiotic adornment and kleptomania; common to both are homicidal, infanticidal, and suicidal mania, and the varied impulses to destroy. The infanticidal, as a pure impulsive insanity of this origin, is much more frequent in females than in males; so also is *cunning* destructiveness, as by fire and poison. Kleptomania is very infrequent in males. Amongst the subordinate instinctive forms may be mentioned, the impulse to textural art in the females, and to rhyming and music in the males. The morbid states of the social instincts, as the filial, fraternal, &c., give rise to perversion of the social feelings, causing impulsive hatred, malice, revenge, &c., or to exaggeration, inducing *amenomania*.

It must not be supposed, however, that in this large and fundamental group of forms of mental derangement, of which a blind, irresistible, irrational impulse or desire is the proper characteristic, the complex forms do

not constantly occur. Just as the instincts themselves rarely, if ever, act without the co-operation of the passions and the understanding, so these—the morbid forms—seldom occur without complications, indicating derangement of the intellect and the affections. They are important, however, pathologically, because they enable us to analyze better the complex forms; etiologically, because they not unfrequently usher in the latter; and morally, because all inordinate operations and perversions of the instincts are more or less criminal or immoral. Hence arise the forensic difficulties in determining when a person is or is not insane, which are found more especially in this class; for the unprofessional metaphysician cannot conceive of a man in apparent possession of all his faculties, acting from a blind impulse. If there be a hallucination, and he acts in accordance with this, the case is comprehensible enough to the popular mind, but not otherwise.

We have remarked, that the passions and emotions are instincts in which the end and the means are known, wholly or in part, to the individual. Instinct acts blindly, as is seen in the use of natural weapons, or in the constructive acts, as the formation of nests for the care of the young, &c. Natural history presents numerous familiar illustrations of this; but in the passions there are no such blind automatic proceedings, unless it be in the very moment and fury of the passion, when it has become ungovernable, or, in other words, when the co-ordination of the will and the understanding is abolished, and it is really a true instinct for the time being. Practically, the difference is certain, and it may serve to characterize a group of mental diseases which may be generally designated pathemic insanity. In these forms there is not the blind impulse to emotional acts only; the change in the feelings and temper is often complete, while hallucinations in immediate relation to one or more of the morbid passions or emotions are manifested. The forms of insanity grouped by Guislain under the term "Delire" (a term we have translated by hallucination), belong to this class, and may easily be referred by the reader to their respective passions or emotions. Other forms placed by Guislain under the head of Mania, belong also to this group; as well as some classed under the head of "Folie," or Vesania; although these latter belong for the most part to instinctive insanity. We shall not more specially indicate them; but we cannot omit noticing one of this group not mentioned by Guislain. It is that in which the sentiment of self-esteem, pride, or vanity, so far from being exaggerated (as in the emperors, popes, prophets, statesmen, &c., of asylums), is perverted, or rather abolished. It is usually combined with the vagabond mania or with oinomania, and the individual either tramps the country in the company of professed mendicants, or spends his days and nights in the lowest pot-houses, drinking with the usual ill-conditioned habitués of such places. It is a form of insanity periodic and paroxysmal—most frequent in men, but occasionally attacking women. There are one or two other morbid modifications of this sentiment, as, *e. g.*, extreme bashfulness.

The transition from this group to the next is direct. In fact, it is rarely found that the sentiments are deranged, and the intellectual faculties remain untouched. Such instances do *apparently* occur from time to time, and there is then the same difficulty in determining the limits of responsibility as is encountered in examples of instinctive in-

sanity.   We probably express the opinion of all practitioners experienced in insanity, when we remark, that the freedom of the *intellect* is *only* apparent in far the greater proportion of cases of this kind.   Often it is, doubtless, very *difficult* to detect the disorder in the ideas or the intellect, particularly in the earlier stages, or when the affection is purely "monophrenic," to use Professor Guislain's term ; but a well-conducted analysis of the phenomena rarely fails to elicit a correct diagnosis.

Having nothing to add to the group termed Dementia, this must conclude our sketch (brief and imperfectly delineated as it is) of the taxonomy of insanity.   Before proceeding, however, to the pathological anatomy and etiology of the disease, we will notice in a few paragraphs some valuable and important ideas promulgated by Professor Guislain as to the nature of insanity, but especially of melancholia, which he has so aptly designated phrenalgia, and of which he has given a most admirable history.

Noting that there is what may be termed a moral sensibility, and observing how often disorders of the mind originate in painful reverses of fortune and in the depressing passions,—for 66 per cent. of cases admitted to treatment were attributable to moral causes, and of these there were, reverses of fortune in 85 per cent., domestic griefs in 33 per cent., and sudden and violent moral impressions in 12 per cent.,—Professor Guislain devotes a lecture to the consideration of this moral sensibility.   Perhaps his own words will best express his views:

" Moral sensibility, that cord which vibrates with so much force, is a point of departure as well in the conservative as the voluntary acts.   It is closely identified with our dearest interests.   It is not unfrequently the origin of the passions. Reason and imagination draw thence their motives ; the latter, in particular, borrows from this moral sense its most glowing tints, its most vigorous tones, its boldest conceptions.   This sense, which has been termed psychal, originates in man those two conditions of his moral being which he designates HAPPINESS, MISERY. It is in virtue of the sensibility of which it is the source, that he is happy or miserable.   In these manifestations there is felt a tenderness, a satisfaction, an emotion, an enjoyment, a friendship, a love.   Heinroth has well defined this sense when he observed that it is related to the faculty which renders man capable of experiencing joy or sorrow.   It is a sensibility in which is found the love of God and sacred things—the love of moral and physical preservation—love, Platonic, sexual, paternal, maternal, filial ; the love of the good and the beautiful—of riches and of pleasure.   It is observed in all those who are known as the friends of an art, of a science ; in lovers of music, in philanthropists, in the partisans of an idea, of a system.   I have given to this sensibility the qualifying term *affective ;* I have named its factor the *affective sense ;* it is, in fact, an *affection*." (tom. ii. p. 122.)

After pointing out its synonyms, Gemüth in German, animus, thymos, in Greek and Latin, *cœur* in French, and we might add *heart* in English,— although Professor Guislain uses the word *moral*,—our author proceeds to remark, that it is this sense which has been painfully affected in the greater number of cases of insanity, and is, in fact, the origin of nine-tenths of the cases of true idiopathic disorders of the mind.   This proposition is attained, however, by leaving out of consideration all those forms of mental derangement or disorder which may be termed symptomatic.   Thus, idiocy and imbecility are rather cerebral than mental diseases ; mania arising in the course of an epilepsy, is only symptomatic ; so also the mental affections following after apoplexy, an attack of gout, metastatic rheumatism, or a

repelled eruption.   The mania resulting from exostoses or scrofulous deposits in the brain, or from the abuse of mercury, from masturbation, old age, &c., is equally symptomatic ; so also is the delirium caused by injuries to the head by the action of narcotic vegetable poisons, and by alcoholic drinks. In the true forms the first stage always consists in a painful condition of the mind—it is a real pain, a phrenalgia; in ninety-six per cent. of these, Professor Guislain has noticed that the countenance indicated a state of nervous irritability (*agacement*), disquietude, chagrin, irascibility; this unhappy condition is reflected in the eyes, the voice, the lines on the brow, the temples, and around the mouth.   It is revealed also in the attitudes, and in the state of the pulse and the urine.

"I distinguish two kinds of moral reaction in mental diseases; the one conservative, the other automatic, destructive ; the former tending to a cure, the latter complicating the malady and inducing morbid structural change.   Phrenic reaction is manifested in the passions, in the instinctive acts, in the volitional impulses, in the domain of the ideas.   Pain invades the centres of instinct, it leads to the most violent resolves, to the strangest and most fantastic movements.   In every disease, nature appeals to the conservative, the resisting forces; this law, it should be remembered, is met with also in the moral world.   This mental suffering, as well as corporeal pain, excites repulsive efforts directed against the inimical agents. In the insane these effects are observed in the loquacity of the patient, in his impatient movements, his accusations, vociferations.   Address a man insultingly, wound his self-esteem, outrage him,—if he be impressionable or irascible he will repel the insults; he will not remain quiet; he will walk about; an involuntary attraction will impel him to direct some violent gesture against the offender ; the accomplishment of this action relieves his distress.   A reaction of this kind may be limited to conversation.   It is certain that the communication of ideas by language is a great solace to the feelings.   This is felt in relating reverses.   The same is seen also in confession, which has often so powerful an influence over the miserable man, whom remorse has deprived of sleep.   Talking neutralizes pain; it soothes him who has received an injury.   To tell his thoughts, to say what he feels—is the instinctive means which the irritated man uses to procure calmness.   It is a crisis, for it removes the elements of the disorder.   I shall designate it the phrenic, psychical, or moral elimination.   I have often ascertained that the patient has forgot when convalescent all the troubles which had previously afflicted him," &c. (Ibid. p. 142.)

Sometimes, however, the reaction is so powerful, that the faculties break down, and dementia results instead of cure.   All those circumstances which debilitate the organism, predispose to this result—low diet, poverty, profuse hæmorrhages.   In developing his doctrine, that a painful passion or painful impression on the feelings is the exciting cause of insanity, Professor Guislain meets various objections which may be raised.   Thus, it occurs from time to time that no such impression is stated to have been felt.   In these cases, our author suggests that the history of the patient is often derived from individuals who wilfully conceal circumstances, the mention of which would, in fact, reflect upon themselves.   So, also, the patient will conceal circumstances of this kind.   It may be objected that there is joyous mania: this is really a secondary result; phrenalgia constituted the first stage.   Besides, just as there may be laughter from painful titillation of the feet, so there may be joyous mania from painful impressions—a sort of moral convulsion.   In those instances in which the paroxysm of insanity is sudden, and apparently without a premonitory

stage, Professor Guislain is of opinion that these cases are less sudden than they appear, although the premonitory stage may be really very short. Sometimes the painful condition is hidden under the maniacal phenomena, and appears only during convalescence.

There are other illustrations of his doctrine which Professor Guislain mentions—as, for example, that the melancholic stage almost invariably precedes that of hallucinations. We need not dwell further, however, on this point, because we think it will not be much questioned that this condition, described as *phrenalgic*, does actually usher in true insanity, and that it is the basis of melancholia. These points we are willing to concede fully; but we demur very much to the inferences drawn by Professor Guislain from these facts. It is a law of his nature that man shall react to impressions, whether they be mental or corporeal; and it is equally a general law (within certain limits), that in proportion as the impressions are painful, the reaction is violent. But it is just as certain a law, pathologically, that a very trifling impression may induce inordinate pain and inordinate reaction—so inordinate as to be out of all proportion to the impression itself. This occurs most obviously in all the various forms of neuralgia, to which, indeed, phrenalgia is so singularly analogous. The question mooted is so important, and its consideration in the latter direction may be so useful in removing difficulties in the way of a better comprehension of the nature of insanity, that we shall dwell somewhat upon the general pathology of the disease.

We have already stated, that the idea of insanity necessarily implies a diseased action of the cerebrum: it is not possible to conceive insanity otherwise than as a functional or structural disease of the brain. Disease of the *mind*, properly so termed, is, as we have remarked, *sin*, or *error;* mental *disease* is, therefore, a term signifying a disorderly manifestation of mental phenomena from *corporeal* disease. This view limits our inquiries at once to a definite and tangible field. We are thus spared all arguments as to "essences," and mysterious, impalpable, imponderable, invisible agencies, and all excursions into "the ethereal regions of science." It is well, indeed, that it should be so; for the successful study of the structure and functions of the human brain will demand the exercise of the most acute intellects and the most profound philosophy. Whatever may be the nature of its motive power, it is a piece of mechanism without parallel in the visible creation for wonder of construction and illimitable capacity of use. While writing this article, we have explored the heavens with the "telescopic tubes;" and far beyond the universe of stars, we find the greatest wonder in creation to be this same pulpy, easily-destroyed machine, whereby the human mind is enabled to estimate the size and distance of bodies yet unseen, and to send its plummet deep into infinity.

The feeling of pain implies the existence of consciousness—that power which is the basis of all mental phenomena. In innumerable instances, as well in animals as in vegetables, a thousand exquisitely-adapted acts are going on (even in our own bodies), in which consciousness takes no part whatever. Its earliest use is to *feel* what is painful or pleasurable; its highest, to *discern* what is good or evil. Now we find that it is *superadded* to instincts, or at least to instinctive acts; for (as we have already remarked) instincts act in myriads of instances without any reference to pleasure or

pain, inasmuch as the organism is utterly unconscious of either. This circumstance is an important consideration, for it tends to set aside the almost universally-received opinion, that sensation is the *cause* of various instinctive acts. Now, that can only be the cause of a thing which is its *invariable antecedent;* but in this instance, sensation is not the invariable antecedent in an immense number of instinctive acts, because it never occurs in them at all; while in another equally large number, it only occurs coincidently or concurrently, and not antecedently. Its antecedent or cause is, in fact, the same as the antecedent of the instinct itself— namely, the impression made on the mechanism of the instinct—the nervous system.

Sensation is really in more direct relation to the *self-consciousness* than to the instinct. It is through this, indeed, that it co-operates with the instinct in the great acts of conservation of the individual and the species. It is the elementary condition of that co-ordinating and combining unity which in intelligent beings is termed mind. As a general principle, pain is felt when an impression is made on the system, injurious to it, or caused by an injurious thing ; pleasure is felt when the impression, or the thing causing the impression, is beneficial or friendly to the organism. It is when the former occurs that the *insurrectional* efforts (as Professor Guislain terms them) are made to expel or repel the injurious thing. The *perception* of the latter, in virtue of the law of organized matter, is also a painful impression, although the really injurious impression is not made. It is painful, because there is an instinctive *anticipation* of injury; for, as we have previously remarked, the instinct for the future is as fundamental an instinct as that of the conservation of the individual or the species, and without it neither could exist or be continued. This anticipation of evil or pain is aversion, dread, terror. Now this, in insanity, is *phrenalgia.* How is it induced ?

We know sufficient of the healthy functions and morbid conditions of the nerves of sensation, to be able to state some general principles as to the pathology of nerve-pain; as to brain-pain, we know less, because we have sought for physical or nerve-pain in the brain. Finding that physical impressions of a certain kind (as pricking, tearing, burning, corroding, and the like) on the nerves, cause pain, because, in fact, they are injurious to the well-being of the organism, experimental physiologists have pricked, torn, burnt, and corroded the brain (the hemispheres); and because no pain was felt, they have concluded that the brain is insensible. But how could the brain feel such impressions? It is so constructed as to be impressed painfully or pleasurably by impressions never in any case derived directly from external injurious agencies, but transmitted by certain important subsidiary structures—namely, the nerves and ganglia of sensation. Cut off the cerebral hemispheres from these structures, and all knowledge of the external world is lost; the individual remains in a state of torpidity, and sensory impressions only excite automatic movements; the circle between the organ of thought (the great co-ordinating and combining unity) and the motor mechanism is interrupted, and no acts of intelligence are done. This is the abolition of mental phenomena.

In neuralgia, impressions so act upon a disordered nerve, that perversion of sensation results. Things that should feel hot, feel cold—that should

give pleasure or be indifferent, give pain. This state arises from a change in the function of the nerve or nerve-centre—for *either* or *both* may be the seat of disease. Nor is this morbific change always, or even usually, recognizable by the scalpel or microscope; some of the most inveterate forms of neuralgia are purely dynamic; the majority of those dependent on a morbid condition of the blood appear to be certainly such; or at least the predisposing cause in the nerve or nerve-centre is inappreciable. This may also occur, and we think does occur, in phrenalgia. The nerve-centres subservient to the instincts receive and transmit perverted impressions—forwarding an erroneous message to the consciousness, and transmitting a morbific impression to the hemispherical ganglia. If these ganglia be in the same condition as the ganglia of sensation, they will also receive and transmit perverted impressions—transmitting an erroneous impression to the consciousness, and thereby giving rise to a faulty co-ordination or combination of the mental powers, or, in one word, to insanity.

What, then, are the changes in the dynamics or structure of the centres devoted to instinctive, emotional, and intellectual feelings, thoughts, and acts, upon which those varied forms of mental derangement depend which we have briefly catalogued? We shall first gather what we may from Professor Guislain's researches into the pathological anatomy of insanity, and we will then touch upon the pathological dynamics. He first considers the views of Gall, and while he decides as to their inapplicability to mental disease, he admits that the phrenological theory contains "incontestable truths." It is cranioscopy rather than phrenology to which he objects, acknowledging, nevertheless, that there are certain portions of the brain with certain faculties appropriate thereto. In mania, melancholia, dementia, and general paralysis, he thinks the hemispheres are the most frequently the seat of disease; but in all the impulsive forms—those which we have classified under the heads of instinctive and emotional insanity—it is the base of the brain. Professor Guislain mentions especially the *vesane* of these classes, who are terror-struck, refuse food, gesticulate, are suicidal, homicidal, unmanageable, destructive. From various anatomical and physiological considerations derived from Foville, Flourens, Longet, Griesinger, &c., he more particularly specializes that part of the base of the brain comprised between the corpora olivaria and corpora quadrigemina.

"It is there where I place the seat of impulsive volition—of that volition which enables my tongue to speak, my hand to write, my legs to support and move me, my mouth to masticate food. From thence must emanate those strange impulses which characterize a large number of cases of insanity. It is to this point that commands are transmitted from the hemispheres along the crura, the fibres of which arise in the medulla [oblongata] and mesocephalon [?]; many of these are, indeed, a continuation of the fibres of the medulla spinalis, and the two layers which they form undergo, as M. Foville has so well shown, some remarkable transpositions in the optic thalami.

"According to Longet, the inciting will [volonté d'incitation] is situate in the annular protuberance [pons Varolii]. This theory is probably the nearest to the truth. Yet the bulk of the medulla is in much more intimate relation with the conservation of muscular acts, than the mesocephalon, which is rather in relation with the cerebellum." (tom. i. p. 174.)

The *structural* changes in the cerebral tissues are placed under four heads—namely, serous and sanguineous congestion; softening; induration. Sanguineous congestion includes congestion proper; hyperæmia; meningitis, cerebritis; ecchymoses, false membranes. The pia mater is most frequently found congested; in about 25 per cent. of the bodies examined, a congested condition of the encephalon was noted. Nothing is, however, more easy to determine than this state after death, nothing more difficult before. The following symptoms seem to accompany cerebro-meningeal congestion. The persistence of the disorder, increasing incoherence and absence of lucid or quiet intervals; the memory and perceptions failing as the delirious incoherence increases, a plethoric habit of body, red face, bright eye, an air of stupidity, great heat of scalp, frequent pulse, clammy sweats, especially of the head, ammoniacal urine. Inflammatory congestion is more particularly indicated where there is great excitement, spasm of the extremities, imperfect muscular action, feebleness; if the evacuations are involuntary, dementia succeeds to mania, and convulsions and paralysis come on. In some cases the congestion leads to spurious apoplexy. The early stage is generally ushered in by great mental activity, not unlike partial intoxication; there is unusual extravagance of manner, with very grand ideas of riches, &c.—the hallucinations of ambition —which Bayle was the first to notice, as having a special relation to congestion of the membranes and substance of the hemispheres. It is a state most frequently induced by habitual intoxication, exposure to the sun or a hot fire, the repression of a cutaneous eruption. When it occurs in the course of a maniacal affection it ushers in the transition to dementia; but it is rarely observed in melancholia, the vesaniæ, or extasis.

When there is hæmorrhagic effusion, the symptoms are those well known to pathologists, as apoplectic and paralytic. Congested cerebral matter undergoes, however, according to Professor Guislain, a special change, independently of any hæmorrhagic effusion. This he terms a cell-enlargement; it is as if the primitive cells of the cerebral tissue underwent a certain amount of distension, and were swollen out by a fluid within them. The field of the microscope appears also to be covered with cells of various sizes, intermingled with fatty granules, which at several points appear as if in strata. Delineations of these various conditions are given with the text.

Chronic serous effusions within the cranium induce characteristic phenomena in the insane, but as these are assimilated to those of chronic hydrencephalus, we need not dwell upon them. The general character of the patient is dulness and stupidity. Acute serous effusions lead sometimes to a comatose condition, maniacal excitement having preceded it. This state is simply arachnitis—acute or subacute.

Considerable space is devoted by Professor Guislain to the consideration of *general paralysis*, under the head of softening of the brain. He gives the result of his microscopic researches, with delineations; he found the same appearances as are observed in congestion, but in a more advanced form— namely, cells intermingled with granules (nucleoid cells), and large fat-cells. He thinks that there is a maceration of the cerebral tissue in effused serum, the result of congestion and imbibition, leading to distension and rupture of the cells. In addition, there is distension of the capillaries, with stagnation

of the fluid in them, an accumulation of fluid in the pia mater, and the admission of fluid into the grey substance, through the canals which give passage to the capillaries passing from the pia mater to the cortical substance. Professor Guislain claims the discovery of the latter, and observes, that they have, in miniature, the same relation to the capillaries of the cortical surface, that the canals of the liver, formed out of the capsule of Glisson, bear to the branches of the vena portæ, to the arterial vessels, and the biliary tubes. They are to be detected by the microscope, but in cases of congestion they become so large as to be seen by the naked eye. Considerable importance is attached to this point, in the pathological anatomy of the brain; for this peculiar structure gives the brain, but more particularly the convolutions, the remarkable property of imbibing fluids to an enormous extent. This has been shown by the experiments of the two Nasses, directed specially to determine the fact. Falret and Etoc have also attempted to show that the cerebral tissue is infiltrated with serum in certain forms of insanity. This infiltration of the brain in general paralysis was observed by Esquirol, who remarked (confirmed by Copland) a multitude of pores or small cavities in the cerebral substance, containing a limpid serum. Dr. Copland objects to the theory that these pores are the sequelæ of vascular extravasation, and thinks it "more probable that they are the consequences of softening, the pores being left by the removal of the molecules of the cerebral substance, which have lost their vital cohesion to the rest of the structure, and filled by a serous effusion." It is worthy of special observation, that Professor Guislain has observed instances of extensive structural change, of which no symptom was observed during life— "des alterations profondes, des gerçures dont rien pendant la vie du malade, n'avait fait soupçonner l'existence." This is an extraordinary statement, and there would seem to be a fallacy somewhere. That Prof. Guislain is of this opinion is probable, from the fact, that he does not seem to attach much importance to so remarkably exceptional a statement. Softening, infiltration, maceration of the cells and primitive fibrils in serum, are the morbid changes in general paralysis. These seem to be gradually progressive, but for a time the disease advances *per saltum*, concurrently with an aggravation of the symptoms; this subsiding, there is a partial restoration to the previous state, to be interrupted as before. Every fresh attack seems to be congestive and effusive, and Professor Guislain thinks it very practicable to trace the amount of infiltration and maceration by the course of the symptoms; the paralysis advancing, and the intellect becoming more and more impaired, after each attack of maniacal excitement. It is in his view a chronic inflammatory irritation, secondary in its nature. The infiltrated serum acts analogously to the effused blood in apoplexy and hemiplegia; partially compressing the cerebral tissue, irritating, distending it, and destroying the histological organization. Essentially, it is an asthenic disease, being, in fact, what may be termed *encephalic anasarca*—the term being ours, and not Professor Guislain's, but expressing, we think, his views.

Inflammation of the membranes leads to thickening, opacity, false membranes, ventricular effusion, and congestion and softening of the contiguous cerebral substance. Professor Guislain dwells upon the difficulty of diagnosis, during life, between mental disease, in which the disorder is of functional

origin, and that in which it depends on structural change. In melancholia, extasis, hallucinatio, and vesania, structural change is rarely found; in dementia and mania, its existence is doubtful; but where there are symptoms of effusion or compression, structural change may be diagnosed. Paralysis, especially the local form, as of the tongue, or of an extremity, convulsions, loss of memory, permanent maniacal delirium, are symptoms of this kind.

Induration of the cerebrum is met with in about 25 per cent. of the insane examined after death. It is seen most frequently in chronic mania, dementia, and epileptic mania and epilepsy. Professor Guislain thinks that the base of the brain and external walls of the lateral ventricles are most frequently affected. He has found the pons Varolii very much indurated more than once; and it is not uncommon to find the corpora olivaria the seat of this change. *Hypertrophy* is more particularly observed in the congestive form of mania; in cases of this kind, the convolutions are so compressed against the cranium, as to be nearly obliterated. Atrophy is seen in chronic dementia, particularly that form which supervenes on acute mania. It is generally associated with the other morbid conditions we have noted. Malconformations, as is well known, are most usual in the idiotic. This short sketch of the pathological anatomy of insanity, so far as the cerebrum is involved, is sufficient to show that the changes noted are really only the sequelæ of those upon which mental derangement depends. By the time they are completed, the faculties are in fact abolished. The dynamical changes are not at all indicated.

Before passing, then, to that portion of the pathological anatomy of insanity which considers the structural changes in the viscera, we will revert to the etiology of insanity, in so far as these organs re-act upon the instinctive, sensational, and intellectual centres. Now in this part of our subject we have a large number of facts, both general and special, to collate; and ought, therefore, to start with something definite, and, in the first place, inquire what is the healthy normal action of the viscera, the limbs, and the tissues of the body generally upon the cerebral, or rather, encephalic, centres? They are all combined into an harmonious whole by means of those centres for certain pre-determined purposes—namely, the conservation of the individual and the race; they are all mutually dependent upon each other for the means to effect the duties allotted to them; they *must*, therefore—when so connected in health—suffer reciprocally in disease. These two general facts are indeed so obvious, that the common language of mankind has been modified by the perception of it, and it would appear the merest commonplace to revert to them, were it not that scientific inquiries—probably from their universality—almost ignore them. But it is a fruitful scientific principle, that the heart sympathizes in all our emotions and passions, that the intestinal canal is impressed by all our feelings, whether of pity or terror, and that the whole corporeal unity thrills through every fibre when the physical union of its various components is threatened with dissolution. All the instincts, without exception, are placed in immediate relation, directly or through each other, with the whole organism, or with special parts thereof. This rule is general in the fullest sense of the word.

We will take one instinct as an illustration of the rest, and will select

the most primary—the instinct for nutrient materials.   So soon as the organism is in want of additional solid or fluid materials, *painful feelings* arise, which are termed the sensations of hunger and thirst.   This painful feeling, by a fundamental and innate principle of arrangement in the organism, is accompanied by, or co-existent with, a *desire (appetitus)* for that which will remove these painful feelings—namely, food and drink; and by an *impulse,* to seek for and procure that which, by an innate principle of arrangement in the organism, is the *appropriate* food and drink, and therefore *adapted* to attain the end desired—namely, the supply of additional nutrition, solid or liquid.   First, then, how do these painful feelings arise? Obviously, primarily they are connected with the nerves of the viscus mainly subservient to the function—namely, the stomach.   If this be empty of food, the painful sensations arise.   But this is not all.   Observation shows us, that mere filling the stomach, or even the digestion of the food, will not allay the sensation of hunger, if it so happen that the chyle cannot enter into the circulation to commingle with the nutrient fluid—as in tabes mesenterica, for example.   Hence a certain *condition of the blood,* as well as of the nerves of the stomach, will excite the sensation of hunger, or, in other words, will modify, dynamically, the condition of that nervous centre which is subservient to the instinct of hunger.   When this modification occurs, the instinct is put into operation; and the means already provided by a pre-existing scheme of machinery are put into operation also.   These means are regulated by subordinate instincts, varying in their nature in different animals, according to the kind of food each requires, and according to the weapons, methods, and mechanical contrivances by which each obtains its food.   Combativeness, destructiveness, cunning, the migratory or travelling instinct, may (to mention one or two of many examples) be thus excited.

These views apply to all instincts.   Now it will be observed, that the changes in the ganglia of the instinct, whereby it is excited into activity, cause three mental changes—a feeling of *pain,* a *desire,* and an *impulse* to act.   There is no proof that these are in the relation to each other of cause and effect, but the contrary, although the former doctrine is almost universally held.   A more careful collocation of facts, drawn from a wider field of observation, amply shows that the impulse to act (certainly the third in the series of effects) may take place without any *desire* or *feeling* of pain whatever.   This proposition the facts of natural history, not less than the vivisections of the experimental physiologist, abundantly establish.   Our own sensations constantly show to us, that there may be also the feeling without the desire or the impulse, or the desire without the feeling.   Hence they are coincident and not consequent—consentaneous phenomena, not sequelæ.   In like manner, by analysing the series of the instinctive phenomena we learn that their arrangement may be altered.   Thus if from any other cause than the want of nutriment, the blood be put into that condition in which it is when nutriment is withheld, we shall have the phenomena of the instinct developed just as if nutriment had been withheld.   Or if, from some change in the ganglia of the instinct, or in the nerves going to the ganglia from the stomach, the same changes occur which do occur when the instinct acts normally, we have again the instinct developed, although nutriment is abundantly supplied.   In each of these instances

(which are of not infrequent occurrence in pathology) we have a morbid condition of the instinct, and psychologically it is insane—or, if we may be permitted the phrase, there is an hallucination of the instinct—inasmuch as there is actually no such withholding and no such need of food as the morbid feelings and desires indicate.

But the perversion may not, and indeed does not, stop there. The *subordinate* instincts undergo modification from the same causes as those which operate on the primary instinct. Their natural and normal order may be broken up, so that they act singly and inadaptively. Destructiveness may be directed to the wrong object, together with a modification or hallucination in the *desire* or the *appetite;* or cunning may be developed when the circumstances are such as to make its operation ludicrous. Illustrations of these views, in the operation of the sexual instinct, love of offspring, of life, &c., may be readily found by the reader in any work on instinct.

We have, then, a clear etiological series—namely, 1. A morbid condition of the viscus, which is in direct relation to the instinct; 2. A morbid condition of the nerves, which maintain the communication between the viscus and the encephalic centre appropriate to the instinct ; 3. A morbid condition of the blood; 4. A morbid condition of the encephalic centre itself. We must not, however, neglect those secondary and tertiary changes which result from this etiological series, or any of its members singly; and which in man are all-important to a right appreciation of mental diseases. We have noted the transition of the instincts into the passions and emotions; now, in reference to these, the ganglia of the instincts bear the same relation to the pathemic or emotional centres, which the nerves (or ganglia) of the instinct-viscus bear to them. Hence, morbid changes in the instinct-ganglia will induce morbid changes in the emotional. This is, in fact, one of the most ordinary occurrences of life, the simplest individual knowing how much the temper and feelings depend upon a satisfactory condition of the viscera; and that a good dinner may determine whether a man will be generous or not.

The feeling of consciousness, or rather, the consciousness itself—the power of feeling—is an instinct; that is to say, it is something innate and essential to the organization of the sentient being. It is the most fundamental of all, inasmuch as on it depends the feeling of *unity*, or that which makes the *individual*—the *one* and *indivisible*. It is obviously of the greatest importance to that harmonious co-ordination and co-operation of the powers of the organism on which its conservation depends. Hence it happens, that inasmuch as the personality of the organism is a fundamental principle in the working of the instincts, so in disorder and derangement of the conservative instincts, this feeling is frequently modified. The whole group of hallucinations – so varied and so singularly curious— which have reference to the *person* as an individual whole, or to particular viscera or parts of the body, has an obvious relation to the primary instincts. In hypochondriasis, and even in the higher forms, it is so easy to trace the connexion, that current phrases, such as the term just used, melancholia, and the like, indicate the doctrine. Our space will not allow us to say more on this point, and it is far too large as well as obscure a subject to be treated cursorily. We will now, therefore, revert to the relations which

the various viscera bear to insanity and affections of the alimentary canal. We quote Professor Guislain's statements on this head.

"Some physicians have assigned considerable importance in melancholy to a pathological condition of the intestines; they think that in many cases it is established that inflammation of these viscera is intimately connected with that disease. The ideas which Esquirol first promulgated as to the occurrence of displacement of the colon, have been confirmed; it is actually the fact, that in the insane it is depressed into the pelvis. Sometimes contraction of this organ is observed. In the bodies of the insane are found manifest peritonitis, the epiploon adherent to the mesentery, and the mesentery adherent to the peritoneum of the abdominal parietes, which is marbled over with red, and covered with flakes of pus. These are evidently secondary affections : in suicidal cases, extensive abdominal lesions are observed." (tom. i. p. 428.)

Professor Guislain has noted in a special memoir the occurrence of pulmonary gangrene in those insane persons who refuse their food. He attributes it to a morbid condition of the blood, inducing a condition allied to scorbutus. The gangrene is therefore a *result* of the perversion of the instinct.

Cardiac diseases are of frequent occurrence in the insane; our author estimates that sixty per cent. suffer. He notes the connexion of the heart with the feelings, and refers also to the influence of sighing, vociferation, &c., on the circulation of the blood through its causing a distension of the right ventricle and auricle. This, concurrently with the enfeebled thoracic action of the melancholic, explains the preponderance of venous blood in the melancholic, as indicated by their complexion, the tint of their lips, &c. Besides these and the structural diseases common alike to the sane and the insane, Professor Guislain refers to the " all-powerful influence" of the eighth pair on the organs of circulation, respiration, and digestion. To a morbid condition of this nerve he refers insane refusal of food, as well as modifications of the functions of the heart and lungs. It is to be regretted that Professor Guislain, entertaining these views, has not favoured us with appropriate facts. We are satisfied (and in the expression of this opinion we shall be supported by all practically acquainted with mental diseases) that changes in the *sensibility* (not the structure or functions) of the heart are frequently connected with all those forms of insanity in which the instincts and emotions are involved. The sensation experienced in certain conditions of the cardiac nerves, sometimes in connexion with structural disease of the heart, but more frequently without, is described as of the most depressing kind, and resembling that felt in the deepest sorrow; it is the sensation of what is popularly termed a broken heart. Whether it be a cause of encephalic change, or whether it be an effect (as when it accompanies sorrow), it is certain that such a sensation is almost constantly present in the earlier stages of melancholia and impulsive insanity, especially the suicidal, and that the morbid innervation of the heart influences its action, so that the flow of the blood is retarded, and repeated sighs are required to relieve the congestive feeling in the lungs induced thereby.

Analogous to this is the sensation of " sinking" (as it is popularly termed) in the præcordial region; a sensation equally frequent with the preceding, although accompanying it, and either inducing or coexistent with an indescribable depression of the feelings. It is to relieve this sensation that small doses of alcoholic stimulants are taken in the first instance, and

afterwards larger, until confirmed habits of drunkenness are established. To this head of gastric and hepatic disorder we may refer the whole group of symptoms known as lowness of spirits, irritability of temper, biliousness, &c., although, in reference to these, as well as to the cardiac sensations, it is not easy to say whether they be cause or effect.

Utero-ovarian sympathies are just touched upon by Professor Guislain, but in the briefest manner. He is evidently not aware of their wide extent, and how many subordinate instincts are roused into operation by a morbid incident excitor action of these organs. What occurs normally in the lower animals in whom the æstrus is periodic, may occur in man abnormally when special morbid changes take place in the sexual organs themselves, or in the central ganglia in which the instincts are seated.

Dr. Laycock's treatise on the 'Nervous Diseases of Women' may be referred to as giving an outline of the curious relations and wide-spread sympathies of these organs, whose functions, under certain circumstances, are regulated by instincts which even dominate the otherwise greater instinct of self-preservation. *Amor vincit omnia et nos cedamus amori.*

Irritation of the rectum (from piles or ascarides), and probably of the prostate gland, has induced an almost irresistible impulse to commit an unnatural crime. We can speak to *one* instance of this kind, in which the patient, happily, was so convinced that he had some bodily disorder (and in which hæmorrhoids appeared to be the exciting cause), that he applied promptly for medical aid, and the irritation was removed before the impulse overcame the moral feeling. Our criminal records contain cases in which high evidence (we remember Sir James Clark giving his testimony in favour of a criminal of this kind) was brought to show that the offence could only have been committed in a sudden paroxysm of insanity, the accused being of the most spotless character and of the highest morals. In this respect, and also as to the exciting cause, these cases are analogous to those unfortunate instances of theft committed by females (especially the pregnant and parturient) of otherwise unexceptionable character. It is lamentable to see that *undoubted* instances of this kind have been severely punished, in consequence of the reckless manner in which the plea of insanity has been put-in to extenuate and excuse crime. Irritation of the cutaneous surfaces will induce some forms of mental disorder, but the classification of them is as yet imperfect.

In all these examples of visceral influence, we have had in view the direct action of the viscera, or of its nerves, or the encephalic centres in relation therewith; but there is another point of view in which they may be regarded—namely, as to the influence which the arrest or disorder of their functions may exercise on the blood, and through the blood on the brain. We have already shown that normal changes in the blood will excite instinctive actions; *à fortiori,* abnormal conditions will do so. Of late years, this humoral pathology has been more developed than ever, and we have so often referred to it in our pages, that we need not go further into the subject. Diseases of the liver, kidneys, and skin, are those which by throwing back the excreta into the circulating current, develop disease of the nervous system. In patients suffering from these, the irritability of the temper is extremely characteristic; and when there is predisposition to cerebral affections, epilepsy, convulsions, or insanity, easily result. Gout

and albuminuria may be mentioned as illustrations. An anæmic condition highly predisposes, as well as the various cachexiæ—the arthritic, the strumous, the syphilitic, the paludic, &c. Extreme hunger will also excite mental disorder, generally of the impulsive kind. The action of toxic agents is obvious; these not only differ in the symptoms (or forms of insanity) which they temporarily induce, but also as to the effect which their habitual use has upon the cerebrum. Haschisch, opium, and alcohol, are far from being alike in either of these respects. Undoubtedly the continued use of the latter perverts or abolishes many of the instincts and sentiments, and degrades the character. Its most common cerebral result, when mental derangement is the drunkard's fate, is to develop the impulsive forms of destructive insanity.

It must not be overlooked, however, that none of these causes act unless there be a morbid condition of the cerebral tissue itself. That this condition may be inflammatory is certain, but the delirium resulting from inflammation is hardly to be classed with mental diseases. There may, however, be a subacute inflammatory condition; yet even allowing for this, a large number of cases still remain, in which the idea of inflammatory action cannot be entertained. If this were so, all those passing affections of the temper and the feelings to which every one is more or less subject, and which in many present the leading characteristics of a paroxysm of insanity, should be considered as inflammatory. Now their transient character, and a consideration of the causes upon which they obviously depend, forbid any such theory. A hearty dinner, a glass of wine, an hour's sleep, will dispel a legion of blue devils, and make the incipient melancholic patient a cheerful man.

It is, indeed, in these simpler and mere transient forms of mental affections that we can best study their pathology, especially when taken in connexion with neuralgic affections and the convulsive group of the neuroses. There is no more satisfactory division of Professor Guislain's work, than that in which he traces out the analogies between insanity and other diseases of the nervous system, mentioning and discussing ten principal points of similarity; although we entirely differ with him in rejecting the doctrine that visceral or ganglionic irritation is often the predisposing or exciting cause of cerebral disease. We feel satisfied, from considerable internal evidence scattered through all the work, that he has not a thorough comprehension of the physiological doctrines (now more or less current for nearly a century) which indicate the influence and course of external impressions in and through the cerebro-spinal centres. We regret this; for we are deeply convinced that this doctrine (ordinarily termed now the doctrine of " reflex action") must be the basis of all cerebral pathology.

We shall not enter into the analogies which may be traced between the neuroses generally, and the various forms of mental derangement. It will be better and more useful to limit our remarks to the relations between neuralgia and phrenalgia—the latter considered as the basis of the larger proportion of the *true* forms of insanity. The term irritation has been applied to that condition of the nerves and of the spinal cord, which is manifested by great susceptibility to impressions, by multiform neuralgic and convulsive affections, and very often,—in females at least,—by singular states of mind, analogous to, if not identical with, certain forms of vesania,

or "folie." Every practitioner of experience must be practically acquainted with what has been termed spinal irritation, or anomalous hysteria; and if he conceive that the pathological condition of the cerebrum, in certain exquisite forms of insanity, is analogous to that of the spinal cord and sympathetic ganglia in the group of diseases just mentioned, he will have, we believe, a tolerably definite idea of their true nature. We have already stated, that in a very large proportion of cases the development of the mental disorder has been preceded by powerful emotions of the painful and depressing character; by excessive indulgence of the primary instincts, as the sexual; by debilitating causes, as hæmorrhages, hunger, &c. In all these instances we can trace out this general principle, that the reaction of the system is constantly excited against injurious agents morally or corporeally. Now the phrenalgic condition of the individual implies such a condition of the cerebral ganglia, as occurs normally when injurious agencies have to be removed; and since that normal state is actually a state incompatible with healthy action, it is virtually a morbid condition, which must necessarily lead, when continuous for a lengthened period, to important changes of an asthenic character in the ganglia themselves, increasing their susceptibility to all the impressions to which the instinct is adapted (just as a neuralgic nerve is increasedly susceptible), and finally ending in such a state, that hallucinations, or paralysis of the instinct or emotion, results. In the former state, incident-excitor impressions act with the same convulsive rapidity in developing appropriate muscular actions, as the touch of a neuralgic nerve induces convulsions. Thus, the suicidal patient will be seized with the impulse to drown himself at the sight of water, or to cut his throat at the sight of a razor, or to throw himself from a height when looking from a height. A female lately parturient illustrated this state, when she described to us certain "temptations of the devil" which she experienced. She assured us, in the most rational manner, that she must be seriously unwell, for she had an almost irresistible impulse to destroy her infant; that she dare not take a carving-knife into her hand, or even have it left in her sight on the table, lest she should cut the child's throat; nor could she venture up a passage leading to her house, with the infant in her arms, because she felt so powerful and unaccountable an impulse to dash its head against the wall. This is precisely analogous to what takes place in the normal instinct; the smell of blood or the sight of food renders the hungry, carnivorous animal unmanageable; the sexual female odour deprives the excited male of all self-control, &c.

We entertain a hope that these general views of the pathology and etiology of insanity, although so briefly and so cursorily announced, will not be without advantage in elucidating the treatment. It is obvious, we think, that inasmuch as the predisposing and exciting causes are so numerous, and the disease is so closely dependent upon them, the therapeutics should be almost entirely etiological; or, in other words, have less reference to the condition of the encephalon, than to the causes which have induced the morbid state. The former is not easily detected, the latter are, if due care be used. The following is the general formula of treatment of Melancholia which Guislain lays down:

"1. To practise the expectant system of treatment to a great degree. 2. To modify the use of remedies according as the disease is progressive, stationary, or

declining. 3. To calm the phrenalgic condition by the sedative influence of seclu-
sion, by mental and bodily repose, by soothing conversation, kindness, and agreeable
circumstances, by narcotic, sedative, and ante-periodic remedies, &c. 4. To excite
derivation on the cutaneous and gastric surfaces. 5. To excite moral derivation
by amusements and muscular exercise. 6. To ascertain the state of the viscera.
7. To consider the general strength. 8. To bear in remembrance the causes of the
affection. 9. To be acquainted with the mental and physical condition of the
patient." (tom. iii. p. 12.)

We shall not follow Professor Guislain through all these principles of
treatment, but glance at some of the more important. With reference to
seclusion, he strongly censures the indiscriminate removal of the insane to
asylums, being of opinion that in some instances the circumstance of being
shut-up in establishments of the kind has a most injurious effect. In
cases in which the disease has made little progress, and the patient is of
the better class, is very manageable, and is attached to his family, Pro-
fessor Guislain thinks the step should not be hastily decided upon. On
the other hand, the poor man has probably a better chance of recovery in
hospitals than at home, in the absence of all ordinary comforts and
rational treatment. In the earlier stages, it is better not to seek amuse-
ments or anything that is likely to exercise the mind powerfully. Pro-
fessor Guislain thinks it most useful to afford the patient repose from every
kind of excitement; music, lectures, promenades, visits, theatres, so fre-
quently recommended, are not of use in the earlier stages, but the con-
trary; it is only when the stage of languor and cerebral inactivity has
supervened, that these means should be adopted. He carries this plan so
far as to keep his melancholic patients in bed during the greater part of
the day; they have, in general, slept little before the outbreak of the affec-
tion, and have need of much sleep. The heart's action is also thereby kept
more quiet—a very important point in cases of this kind. He says that
he has found no plan of treatment so satisfactory.

The narcotic and sedative drugs are various; opium holds the prin-
cipal rank. Professor Guislain adopts Engelken's method of administra-
tion, prescribing it in cases of melancholia without hallucinations of the
ideas, in which there is great susceptibility to impressions, fear, terror, &c.
Professor Guislain administers it in half-grain doses, giving from two to
five doses daily, and continuing them for from one to two months.
He treated 12 per cent. successfully by this method, the patients being
dismissed on the average between the third and fifth month. In cases of
profound melancholy, hypochondriacal delusions, feeble pulse, &c., he sub-
stituted the acetate of morphia, administering from $\frac{1}{8}$ gr. to $\frac{1}{4}$ gr. at bed-
time for five or six days, and gradually increasing the dose to $\frac{1}{2}$ gr. and $\frac{3}{4}$ gr.
per day, giving the latter quantity in $\frac{1}{4}$ gr. doses in the morning, after
dinner, and in the evening. At the same time, if the pulse will allow,
warm or hot baths, of from one to two hours' duration, are prescribed, and
the diet is nourishing. Sometimes the theraica is used, sometimes opium
is combined with belladonna in the proportion of $\frac{1}{2}$ gr. of the latter to ij. grs.
of the former, or with hyoscyamus; but Professor Guislain has not found
this drug of much use. Digitalis is useful when the pulse is very quick,
and there is great feeling of anxiety. In cases with decided hysterical
symptoms, he has administered full doses of assafœtida—two to three ounces
of the tincture—with decided benefit. The sulphate of quinine is most

useful in the uncomplicated forms of melancholia; the sulphate of copper combined with digitalis or quinine has been found useful in simple phren-algia.   The emetic tartar he cannot recommend.   Blisters, tartar-emetic ointment, and the moxa, have been found beneficial where derivation is necessary, either for the body or mind.

Every possible kind of employment is found for the patients at Ghent, so soon as they begin to show an interest in what is passing around them; cards, books, lectures, music, are ordered as soon as practicable.   The last requires considerable judgment in prescribing it.   It is only when con-valescence is decidedly established, that active muscular exercise, walking, travelling, &c., are beneficial.   Family visits, the confessional, prayers, &c., are injurious in the early stages, but often useful in the later.

In treating Mania, Professor Guislain keeps the following principles in view:

" 1. To arrange measures for the safety of the patient, his friends, and the public.—2. To lessen or reduce the mental excitement by the calming influence of seclusion, or the judicious employment of a 'depression morale.'—3. Next, to excite organic action on the skin and intestinal canal by hot or cold baths, emetics, purgatives.—4. To modify the action of the nervous system by narcotics and ante-periodics.—5. To prevent and relieve the congestive erethism by local or general depletion.— 6. In the advanced stages of the disease, to re-excite activity in the muscular system, the intellect, and the feelings.—7. To pay attention to the organs of nutrition.—8. To estimate the strength of the constitution and the character of the disease, as sthenic or asthenic.—9. Not to lose sight of the causes.—10. To estimate the resources of nature and the influence of crises, bodily or mental.—11. To direct attention to the moral and physical constitution of the patient." (tom. iii. p. 81.)

We regret that our limits do not permit us to notice at length these and other modes of treatment, with which Professor Guislain occupies the third volume.   We feel, however, that any summary would be too brief to do justice to the author; and as his experience and reading are alike exten-sive, we think we ought to recommend the work strongly, not only to those who have the care of the insane, but also to the profession in general, by whom a work of this kind will be found particularly valuable.

In addition to the general pathology and treatment, Professor Guislain treats largely of the construction and management of establishments for the insane, interlining the text with ground plans of buildings, and entering fully into every particular of the subject.

We had intended noticing Dr. Monro's " Articles." We have only space to say that their tendency is to extend the system of Government manage-ment and control.   It is too much the fashion to depreciate the manage-ment and efforts of private individuals in the treatment of the insane; in some particular instances we believe they have been much calumniated, and both their feelings and property most unjustly injured.   While it is true that abuses may and do exist in private asylums, we cannot forget that by far the greater number of improvements in the condition of the insane were proposed, and partly carried out, long before the Commis-sioners in Lunacy existed as a body; and that it is probable that the same spirit will continue to actuate the generally estimable class of psychiatric practitioners.   There may be exceptions to the rule of good and kind

management, but we must not forget that there are exceptions to the rule of able and conscientious commissioners; and that if the public confidence be misplaced in this direction, an arbitrary, dogmatic, and tyrannical board may take the place of the private practitioners in lunacy, with great detriment to the public service, and with greater detriment to the unfortunate lunatic.

We do not, therefore, agree with Dr. Monro in his general views. The intrusion of almost irresponsible public boards into the management of matters wholly within the legitimate sphere of the profession, is a growing evil, and may eventually inflict serious mischief on society. The great danger that such boards will be charlatanic in their principles and practice, is obvious; with such principles, and an immense power over private interests and conduct, they require to be narrowly watched, and rigidly subjected to the salutary control of public opinion.

ART. XIII.

Traité des Fistules Vésico-Utérines, Vésico-Utéro-Vaginales, Entéro-Vaginales, et Recto-Vaginales. Par A. J. JOBERT (DE LAMBALLE), Docteur en Médecine, Chirurgien de l'Hôtel Dieu, &c. &c. Avec 10 Figures.—*Paris*, 1852. 8vo, pp. 420.

A Treatise on Vesico-Uterine, Vesico-Utero-Vaginal, Intestino-Vaginal, and Recto-Vaginal Fistulæ. By A. J. JOBERT (DE LAMBALLE), D.M.P., Surgeon to the Hôtel Dieu, &c. &c.

IN the midst of an almost complete dearth of really new and valuable medical literature, on the continent of Europe no less than in this country, the treatise before us may fairly claim no slight amount of attention. The condition of a woman suffering under incontinence of urine, or from inability to retain the contents of the rectum, is so truly deplorable, that we think no apology can be necessary for making more widely known the success which has attended the treatment of these cases in the hands of M. Jobert. We do not profess to present a complete view of the subject; on the contrary, our object will be fully attained if we succeed in awakening the reader's attention to the labours of M. Jobert, which appear to have resulted in a more satisfactory issue than those of any preceding or contemporary operator. The difficulties of the subject are acknowledged, but from recent accounts would appear in some instances to have been exaggerated. At all events, there is an opportunity given for further experiment in this country, and we can hardly conceive any practical investigation which promises more encouraging or more useful results.

M. Jobert's treatise is divided into seven parts, and illustrated with a few wood-engravings incorporated in the text, which very much enhance its value, and render descriptions clear which without them would be rather obscure. We shall not notice each of these divisions fully, but shall select such portions as appear most adapted to the object we have in view.

A paper, by Dr. Marion Sims, on Vesico-Vaginal Fistula, has also appeared in the 'American Journal of the Medical Sciences,' for January,

1852. Its chief point seems to be, to recommend the employment of a particular kind of clamp-suture, which has been found very efficacious in the author's practice.

Vesico-Uterine Fistulæ are situated at the upper part of the commencement of the vagina, and establish a communication between the vaginal portion of the neck of the uterus and the corresponding part of the bladder. Most commonly the fistula occupies that portion of the bladder and of the neck of the uterus which is uncovered by peritoneum; but when the exciting cause has engaged so large a space as to invade the serous membrane, the fistula necessarily occupies a higher position.

Although these vesico-uterine fistulæ are generally single, yet Professor Stoltz of Strasburg has pointed out that it may so happen that both the opposite walls of the uterus have been involved, in which case there is a double fistula—a vesico-uterine fistula, and a utero-abdominal fistula; so that there is, in fact, a double communication between the mucous surface of the bladder and the peritoneum.

Sometimes the communication resulting from a loss of substance is by a round aperture; sometimes by a sort of fringed opening, more or less irregular; and occasionally, a thickened membrane covers the opening. Sometimes, again, the os uteri is sound, but is more often partially destroyed; and in various cases the interior of the uterus is red and vascular, or sometimes studded with indurations.

The cause of these fistulæ is sufficiently obvious—namely, a tedious, difficult labour, whether produced by the pressure of the child's head, by the use of instruments, or by fragments of bone when the infant has been destroyed; but it does not seem necessary, in this place, to follow the author's speculations on the exact manner in which these various causes act, especially as his observations are chiefly derived from a memoir on the subject, published by Dr. Stoltz of Strasburg, and from Madame Lachapelle's well-known treatise.

The sign by which this accident may be known, is the constant escape of urine when the patient is in bed, and its partial escape when she is in the upright posture; but it is not so easy a matter to detect the exact locality of the opening. The speculum reveals nothing, and manual examination only shows that the septum between the bladder and vagina is entire. It is only by closely examining the os uteri, that one observes the escape of urine by its central orifice; and it is necessary further to assure oneself, by pinching the neck of the uterus, that the urine actually passes out of the os uteri, and does not merely flow over the os, in escaping from the fistula which is in its neighbourhood. By injecting the bladder the matter may be made more sure, but it is necessary to use the eye very carefully in order not to be led into error. If the fistula be large enough, the introduction of a sound into the bladder, and of another into the uterus, and making them meet each other in the axis of the neck of the uterus, will assist the diagnosis.

A vesico-uterine fistula is a very serious accident, which has a constant depressing effect on the physical and moral health, and leads to extensive changes both in the uterine and genital functions, and therefore demands the most prompt attention. Such fistulæ as consist in a simple perforation of the neck of the uterus and of the bladder, are much less dangerous than

those which are accompanied by softening, ulceration, or destruction of the posterior wall of the uterus, and by a wound of the peritoneum, which may inflame in a greater or less degree, and give rise to diffused peritonitis, instead of to adhesive and reparative inflammation, when an operation is attempted.

The treatment of these accidents is of course the most interesting question for us.   With respect to prevention, M. Jobert's observations have led him to the conclusion, that malformations in the mother are very seldom the cause of urinary fistulæ; but that it is to the size of the child's head, and to its prolonged detention in the pelvis, that the accident is attributable. But however this may be, it is necessary, in order to cure them, not only that the course of the urine be changed, but further, that the opening itself be entirely blocked up.   Of late it has been recommended to attack these fistulæ by the nitrate of silver carried into the interior of the neck of the uterus; but this caustic is so difficult to manage in a locality where the eyes can afford no assistance, that few good effects can be anticipated from it. It is impossible, also, to produce obliteration by so mild a caustic as this; which, indeed, has the further disadvantage, that it may produce most violent inflammation if used too freely.   This may be readily understood, when we reflect that in vesico-uterine fistula, produced either by labour or by foreign bodies, there is a loss of substance such as is impossible to be repaired by granulations, which in this situation, on account of the structure of the parts, attain only the minimum of development.   It is therefore only by the knife, by paring the edges, and by suture, that these fistulæ can be cured.

The author, in this place, again repeats that all his operations are founded on observations made on the dead subject; and that from different experiments he is convinced, that by following his directions it is quite feasible to divide the neck of the uterus to a considerable distance, without wounding the peritoneum.   These incisions are to be made in the course of the commissures, as far as the insertion of the vagina into the uterus; and M. Jobert has observed that the obliquity of the neck does not produce such noticeable changes in the relations of the uterus to the surrounding parts, as to require any modification in the method of operating.   A straight probe-pointed pair of scissors is the preferable instrument, which in its progress divides the neck of the uterus and the vagina, and finally penetrates the loose cellular tissue of the great ligament.   In no instance on the dead subject has the uterine artery been divided, even when the incision was made very high up.

There is always danger in overstepping the point where the vagina is attached to the neck of the uterus.   A lateral incision made to the right and to the left, affords great facilities for the examination of the communication between the bladder and the neck of the uterus, and gives space for any manipulations that may be necessary.   This will be more readily understood, when it is recollected that the bladder rests upon the anterior part of the neck, which is the situation in which to search for the fistula.

After having made this double incision, the aspect of affairs will be this.

1st. Two incisions will be observed at the root of the vagina, which run parallel in the axis of the neck of the uterus, and follow the course of the commissures.

2nd. These divisions permit the two halves of the os uteri to be turned respectively backwards and forwards.

3rd. Two openings are seen in the shape of fissures, which are continuous with those in the lateral halves of the os uteri. These edges being left to themselves, come in close contact with each other. By their natural gravity and attachment to the vagina, they are prevented at any time from widely separating; and the maintenance of the contact between the eight edges which are formed by the quadruple division to the right and to the left of the vagina and uterus, necessarily permits of their easily uniting.

Having disposed of these preliminary considerations, the author now proceeds to give an account of the manner in which the operation is actually to be put in practice; and this account he divides into ten stages.

In the first operation the attempt is to be made to obliterate the aperture in the bladder, leaving to a later period the uterine fistula.

The neck of the uterus is first to be divided to the right and to the left in the course of the commissures.

The vagina is also implicated here, and requires to be carefully divided laterally and above.

The finger is from time to time to be inserted between the lips of the wound, in order to detect the vesical opening of the fistula.

As soon as this is found, the os uteri is raised up, and the scarification of the fistula effected with forceps, scissors, and probe-pointed bistoury. Sutures are then inserted into the sides of the wound, where they can be readily brought into contact.

In the second stage, all communication between the uterus and the vagina is cut off, so that the bladder alone communicates with the uterine canal. Here the resection is carried, not only over the opening in the bladder, but extends also to the surface of the neck of the uterus; for its object is to close the opening between the vagina and the bladder. The paring requires to be effected gently, and after the division of the neck in the course of its commissures. The knife must be carried not only on the surface of the neck, but it must also pare whatever is left of the neck itself. Thus when the operation is completed, there are two bleeding surfaces, which can readily be adapted to each other, by means of two lateral sutures in the course of the commissures, and one suture in the middle. These stitches form three loops which embrace a certain thickness of the uterus and vagina. They should be placed as low as possible, in order to leave the upper portion of the uterine canal quite free; and they may be successively withdrawn from the sixth to the tenth day. The operation, therefore, allows of a ready approximation of the edges of the wound, so that it can heal easily, and this because of the natural tendency of the edges to come together, and of the position of the sections, which gives the opportunity of keeping them in contact almost without effort. When the wound has healed, it is plain that the communication between the vagina and the uterus must be closed, and that the uterus, on the other hand, communicate with the bladder, into which the menstrual discharge is poured at every catamenial period.

The actual practice of this operation is detailed at length by the author, in the following case, which we shall present entire to the reader's notice:

Rosalie Lazillaire entered the clinical ward on the 5th of August, 1849.

She had been confined on two occasions at the Maternité ; the first time six years before, and the second three years back ; but her labours had been long and difficult, and had not been completed without the employ-ment of instruments.   In both instances the children were still-born. On the 5th of December, 1848, she menstruated for the last time, and is supposed to have become pregnant within a few days afterwards.   Accord-ing to this calculation, which also corresponded with the size of the uterus, she was in the eighth month when admitted, and proceeded to the com-pletion of the full period without any accident.   The patient's diminutive height, various indications of rickets, and the difficulties which had attended her two former accouchements, all pointed to some malformation of the pelvis ; and M. Paul Dubois accordingly found, on admeasurement, that the antero-posterior diameter was not greater than from eighty-five to eighty-six millimétres.   It was therefore determined to induce premature labour. On the morning of the 9th of August, Prof. Dubois introduced a sponge-tent five or six centimétres long, and fifteen millimétres in circumference at its base, and maintained it in contact with the neck of the uterus by means of pieces of common sponge placed in the vagina.   A few trifling pains occurred during this day and the following night ; but at ten o'clock on the following morning they became more frequent and violent.   This day also passed, however, without any definite result ; and on the following morning a larger piece of sponge similarly prepared was substituted.

On examination the next day, the night having passed without an instant's sleep, the os uteri was found to be considerably dilated, and the upper lip soft and dilatable.   A gum-elastic bougie, armed at the point, was passed into the uterus, and the membranes ruptured eight or ten centimétres from the orifice.   About eighty grammes of liquor followed the puncture.   During the remainder of the day, and the ensuing night, sharp irregular pains followed each other, at irregular intervals.   On the 13th, they were still more frequent ; the neck of the uterus had almost disap-peared, but was a little dilated.   At three o'clock, the os uteri had attained the size of a franc-piece, and at eight the dilatation had proceeded as far as eight centimétres.   The membranes swelled out at each pain, the head being moveable and situated above the brim.   At a quarter-past six, the membranes were ruptured, and discharged a considerable quantity of greenish, fœtid fluid.   After five minutes' rest, the pains recommenced with increased violence ; the head, however, did not become fixed, and it was impossible to determine its position.   The child's heart beat regularly 144 or 148 times in the minute.   At half-past nine the patient was put into a tepid bath, and a lavement, with twenty drops of laudanum in it, was thrown up the rectum.   After frequent attacks of vomiting, the head became engorged, and at four in the morning, the woman was brought to-bed of a dead female child, which weighed 3300 grammes. The first few days after the labour passed without remark, but on the sixth day the patient perceived that her linen was soiled, and that she could not make water voluntarily, but that it escaped every moment without her being able to retain it.   As this condition continued, M. Depaul recom-mended her to M. Jobert, and on the 22nd October, 1849, she was admitted by him into the Hôtel-Dieu.

On examination of the external organs, little was found but slight

inflammation, with some granulation about the labia majora and the fourchette, accompanied with œdema, and a few hæmorrhoids at the anus.

The speculum showed that the vaginal septum was entire, and the question arose, whence did the urine proceed which filled the vagina, and flowed over the external organs of generation. A careful examination, made by separating the labia majora, and depressing the recto-vaginal septum with the univalve speculum, showed that a flow of urine escaped at the os uteri. It was only by raising the anterior lip, that the spot was found whence this urine flowed, but it was not difficult to see that a considerable portion of the posterior lip had been destroyed in the progress of the labour. A finger introduced into the dilated os readily passed into the bladder, proving that a lesion existed, which was rightly called a vesico-uterine fistula. It was evident that all the anterior parts of the neck of the uterus had been destroyed, together with the corresponding portion of the bladder. As the patient was anxious that something should be attempted immediately for her relief, the following operation was performed on the 24th October.

1st. The os uteri was seized anteriorly by two hooks, and gradually the neck was brought down. As soon as the uterus was brought as low down as practicable, two incisions were made at the sides of the os, in the course of its commissures; and in thus enlarging the uterine orifice, the vagina was also detached laterally.

2nd. The neck being widely open, it was possible to scarify the track of the urine, to pare its thickened edges, and to dissect-off the utero-vesical mucous membrane. The lateral openings were then allowed to come together near the vesical opening, and sutures were inserted.

3rd. This stage, that of inserting the sutures, was the most difficult of the whole operation. It was necessary, in fact, to bring the sides of the fistula into contact by numerous points of suture, and this M. Jobert was enabled to do without any very extreme difficulty, by having taken care to *detach* the vagina from its attachment at the neck of the uterus, as well as to its sides. Two lateral sutures, and a third in the centre, were inserted.

A bleeding vessel was tied; injections of cold water were thrown into the vagina; a plug of amadou was introduced, and a gum-elastic catheter left in the bladder. Nothing remarkable occurred for some hours after the operation, but, in the course of the day, a flow of blood took place by the side of an artery which had been tied. The house-surgeon kept up pressure by means of a piece of agaric introduced into the vagina. The same evening, the patient began to complain of colic, and of a desire to vomit, the belly being distended with flatus. The next day she vomited several times, but M. Jobert considered her symptoms to be principally spasmodic, such as are common after many operations, and he therefore merely prescribed some carminatives.

On the 25th October, the gastro-intestinal tenderness had almost disappeared, and the pieces of agaric came away in the midst of clots of blood.

On the 27th, the patient had an opiate injection, as she had been four times purged during the night.

On the 28th, this was repeated as the diarrhœa had not ceased, and she was desired to abstain from everything except broth.

On the 1st November, an examination was instituted by injecting some tepid water into the vagina.

The next day, the injection having been repeated, the patient was examined by the speculum, and a suture was removed. A catheter, introduced into the bladder, drew off a quantity of thick fœtid urine; and this evacuation was followed by several jets of urine, such as are common in health, and proved that the functions of the bladder were being regained.

On the 3rd, the patient passed her water twice without the aid of the catheter.

On the 5th, another stitch was removed, and the edges of the wound were found perfectly adherent. A catheter drew off a large quantity of water, and the patient remarked, that none came out of the vagina, and that she was not at all wetted.

On the 8th November, there was much diarrhœa, which, however, ceased under the employment of sinapisms, lavements, &c.

On the 9th, the catheter was removed entirely.

On the 11th, frequent inclination to urinate distressed the patient, and on the 14th, this was so bad as to cause the patient to pass her water every eight or ten minutes.

On the 18th, this symptom was ameliorated, the patient retaining her water about half-an-hour; but whenever the desire was manifested, there was not power to retain it; and on the 26th, she was restless, and said she was wetted by the urine. M. Jobert examined with the speculum, and withdrew a suture, which, without doubt, had caused the vesical irritation, for the cicatrix of the wound was sound, and there was no water in the vagina. From this time all went on well, until the 4th December, when she retched several times, and complained of a sensation of weight in the belly and kidneys.

On the 5th and 6th December, there was a marked attack of intermittent fever, for which a *quinine lavement* was administered. These and various other similar symptoms subsided, and on the 10th December, another examination was made with the speculum. The vagina did not contain a drop of urine, but the bladder, on the contrary, held a certain quantity; and on introducing the finger into the vagina, a thread was felt in the locality of one of the sutures. Portions of it were removed by the forceps and scissors, but not an entire piece.

On the 11th, there was nothing remarkable to note, and the patient rose for a part of the day, and took her food with appetite. The urine could not be retained for any length of time, but escaped involuntarily at night, and indeed in the day also, when the patient was in the horizontal position; but was held well enough when she was upright.

On the 21st, at another examination with the speculum, when a catheter was introduced into the bladder, a small quantity of urine was found in the vagina, but this, it was clear, had got there owing to the presence of a suture, which was withdrawn.

On the 28th, at another examination, it was perceived that urine flowed from a catheter passed into the bladder; it was also clearly perceived in the interior of the vagina. There was, in fact, a small aperture, which permitted the escape of the urine, and explained the incontinence existing during sleep and in certain positions of the body. An injection into the bladder showed

this clearly, for the water was seen to distil from the anterior part of the vagina and to drop into this situation. The aperture, however, was so small, and so evidently produced by the presence of the thread, which had not been withdrawn from its original situation for two months after it had been placed there, that M. Jobert judged it to be sufficient to touch it a few times with the nitrate of silver.

Without tracing the history so minutely as is done by M. Jobert, it may be sufficient to state, that after various cauterizations, this fistula entirely closed, the bladder recovered its tone, and on the 22nd of January, 1850, the patient was pronounced well.

A final examination was instituted on the 4th of March, and disclosed the following state of things:

1st. The introduction of a sound into the bladder demonstrated that the organ had regained its normal capacity, and this examination also showed that the neck of the uterus was met by the sound when the instrument was introduced in the direct line.

2nd. The urine was perfectly clear and transparent, both that which had lain for some time in the bladder, and that which had only just flowed into it.

3rd. A digital examination showed that the vagina had very nearly recovered its natural dimensions; the anterior part was slightly more raised than in the natural state.

4th. The remainder of the posterior lip was completely healed.

5th. The anterior lip of the os uteri, which had not been interfered with during the labour, preserved as nearly as possible its natural position.

6th. There were a few linear cicatrices in the circumference of the vagina, in the position where it is attached to the neck of the uterus. There was not the slightest trace of a fistula anywhere existing.

It was thus shown that the sloughing had seized upon the neck of the uterus above the insertion of the vagina, and had formed a considerable opening into the bladder—that is, in the situation where it rests on the anterior surface of the neck; and this, too, without any interference with the vesico-vaginal septum. It was also curious to observe the destruction of the posterior lip of the os uteri, although the anterior one had not suffered the least loss of its substance. There was thus a large direct communication formed between the bladder and the uterus, by means of which the urine made its way through the orifice of the neck before entering the vagina, thus forming a true vesico-uterine fistula. M. Jobert has never since met with one of precisely the same character. In all other cases there has been a destruction of the anterior part of the neck, and of more or less of the vesico-vaginal septum, so that the fistula was properly called vesico-utero-vaginal. The case also shows that a thread left in the thickness of the tissues may, in time, form an aperture for the passage of the urine.

Vesico-Utero-Vaginal Fistulæ are more common than the simple vesico-uterine variety. They generally have a cavity into which the urine is received, and thence poured into the vagina, so that they form a species of cloaca—the result of extensive lesion both of the bladder and uterus, and also of the vagina. In short, they are the result of extensive sloughing, produced by the excessive pressure to which the parts have been subjected

during parturition. They produce precisely the same symptoms as the other fistulæ, the only difference between all such fistulæ consisting in the greater or less amount of urine which escapes from the bladder. They are easily detected when the vesico-vaginal septum has been destroyed at the point of its insertion into the neck of the uterus; and when this destruction is only partial, there is always a drain which points out the seat of the opening. But sometimes the neck of the uterus has been destroyed as well as the vagina, and there is nothing discernible but a round aperture, which permits the indiscriminate passage of the menses and of the urine; and it is only by introducing a sound into this orifice, that a cavity is to be detected at the base of the vagina, in which all the discharges accumulate. The prognosis of these cases depends upon the double or triple lesion which exists in the urinary and genital passages, and is rendered more grave by the locality of the lesion, and by the existence of disease within the pelvis, as well as by the presence of adhesions in the vagina, which impede the play of the instruments, or prevent the uterus and bladder from obeying the traction necessarily made upon them, in order to render them accessible to instrumentation. The principle of the former operation holds good here; and it is divided into distinct stages.

In the first proceeding, the vagina is separated in those points where it still retains an attachment to the neck of the uterus; and by means of lateral incisions, made obliquely through its sides, above and below, the passage is loosened, and the lips of the fistula are enabled to come together.

The second step consists in scarifying the remainder of the septum and of the neck of the uterus.

The third proceeding is that of bringing together the septum over the neck, so as to impose one bleeding surface on another, and to maintain their contact by sutures. The sutures ought to be so managed as to embrace the septum and a considerable part of the thickness of the neck of the uterus. If there is any tension of the parts, it is be remedied by incisions, and the threads should be withdrawn on the fifth or sixth day.

The second operation consists in scarifying the remaining portion of the neck of the uterus, and in dissecting-off a strip from the circumference of the fistula, so as to carry the dissection over all those surfaces which are nodulated.

The other steps consist in placing in contact the remnant of the septum and of the neck of the uterus by their extremities, without paying any attention to the cavity which exists behind the edges of the wound. The threads pass through the septum and the posterior part of the neck of the uterus towards its margin. When the reunion has taken place, that which is left of the neck of the uterus presents the appearance of a sort of flap, which, in looking at the septum, is seen to close the fistula like a lid. The neck of the uterus, in fact, performs the same office as the sole in partial amputations of the foot. A furrow is also to be made in the anterior part of the neck of the uterus, with the view of more easily keeping the vesico-vaginal septum in its situation.—Four cases, reported at great length, illustrate the practice here recommended.

Françoise Nodre, æt. 33, as the sequela of a tedious labour caused by a hydrocephalic child, laboured under incontinence of urine, which she discovered eight days after her confinement.

Examination showed that there was an aperture communicating with a sort of sac between the bladder and uterus, into which the urine was poured previously to its passing into the vagina. Behind this opening was the seat of the posterior lip of the uterus, and to the left of it a little elevation, which was nothing else than a rudiment of the anterior lip. No traction on the vagina and the remainder of the neck of the uterus sufficed to drag it to the external opening of the vagina, thus proving that strong adhesions existed between the vagina, the uterus, and the neighbouring parts.

On the 22nd November, 1850, M. Jobert proceeded to operate. As the organs could not be brought down to the vulva, the operator was obliged to manœuvre in the interior of the vagina. Both edges of the fistula were first pared by means of scissors and long-handled knives, and by this means increased somewhat in size, and then, after repeated injections of cold water into the vagina, the two surfaces were brought together by sutures in such a manner, that the remaining portion of the neck of the uterus was placed in apposition with the anterior part of the fistula, so that the communication between the cavity of the uterus and the vagina was interrupted.

After this, two lateral incisions were made, parallel to each other, traversing the whole length of the vagina, from the vulva along the side of the rectum, and a third transverse one was made behind the meatus urinarius in the substance of the septum. The object of these incisions was to permit the parts to approximate more closely, and to diminish tension.

A plug of sponge was placed in the vagina to guard against hæmorrhage, and a catheter put into the bladder to permit the escape of the urine.

On the 28th November, six days after the operation, the patient was examined. The recto-vaginal septum was pressed down by means of the univalve speculum, and at the base of the vagina a small quantity of pus was observed mixed with urine. This was cleared away by means of a little tepid water injected into the canal, and then the three threads constituting the suture were withdrawn. She was then put to bed, and a new catheter introduced into the bladder, with the effect of giving exit to a flow of urine.

On the 2nd December, at another examination, a certain amount of pus was found at the base of the vagina, but not a drop of urine. The incisions were nearly cicatrized, and everything betokened a speedy and complete cure.

On the 5th, in the middle of the day, the patient withdrew the catheter, in order to make water herself, and immediately became conscious, as she thought, that a stream of urine had passed into the vagina. This was, however, a mistake, for the next day nothing but a little pus was discovered in the vagina.

On the 12th, the catheter was removed, but as the bladder had lost the habit of retaining the urine, the patient was obliged to make water every ten minutes, and the following day it was replaced, in order to obviate this inconvenience.

On the 23rd, the urine became tinged with blood, which was manifestly the catamenial discharge that had passed into the bladder. With the exception of the irritability of the bladder, she was now quite well; this symptom, also, gradually subsided, and in a short time she left the hospital.

On the 6th January, a careful examination displayed the following

points among others. The vagina had lost something of its length, but nothing of its transverse diameter. There was no trace of the neck of the uterus in the vagina, and the cicatrices of the operation were not such as to be of any importance.

The next case is that of Madame B., a lady thirty-seven years of age. She was married at twenty-one, and in eight years had seven children, always brought forth after a difficult labour; only two of them survived, the rest either being still-born, or dying shortly after birth.

In the year 1848, she was confined of a still-born male child, of which she was in labour thirty-six hours. The forceps had been employed to deliver; and a few days afterwards the patient perceived that she laboured under incontinence of urine. Eighteen months subsequently, M. Jobert saw her, and after several minute examinations, made out that there was a considerable loss of substance of the vesico-vaginal septum, and of the anterior portion of the neck of the uterus; as a result of which, that part of the vagina which is attached in front to the neck of the uterus had entirely lost the antero-lateral portion of its circumference, whilst the posterior attachment was alone sound.

At the point of junction of the neck and body of the uterus, there was an elevation which marked the situation where the uterine canal was preserved in a state of integrity. Three fingers could be passed through the fistulous opening into the bladder. That portion of the vagina which protects the canal of the urethra still existed, and formed a thick cushion. The urine flowed continually into the vagina, and escaped involuntarily. So unfavourable a case as this was, offered little encouragement for an operation; but the result proved that such cases are never to be despaired of, however bad they may seem.

On the 16th December, 1849, the following operation was performed. The remaining part of the neck of the uterus was seized and dragged down in front, by means of hooks, and there kept during the whole operation, which occupied nearly three-quarters of an hour. The surface of the remainder of the septum was extensively scarified, as were also those portions of the os and cervix uteri which had not been destroyed, and to which the remains of the septum could be fixed. M. Jobert then carefully separated in its whole extent what remained of the vagina behind, at its insertion into the neck of the uterus. This dissection, or separation, loosened the wall completely, and permitted its lateral and anterior portions to obliterate the loss of substance, and it was then only necessary to carry the neck of the uterus from behind forwards to close up the enormous gap which had been left by the labour. A few strokes of the knife made near the neck of the uterus, and from below upwards, caused the tissues to yield so as to approximate closely. In carrying the dissection upwards, the greatest caution was necessary not to wound the peritoneum. Three sutures, one in the median line and two lateral ones, were sufficient to keep this vast wound in contact, but the needles were passed through the whole surface of the septum, and through a great part of the thickness of the neck of the uterus. A transverse incision from the left to the right was made so as to relieve all tension. The uterus being released from the hooks which had held it, reascended to its place, and a catheter having been passed into the bladder, the patient was put to bed. A plug of amadou was also introduced into the vagina.

On the 26th, an examination was made, and one whole suture and a portion of another were removed. The vagina contained a considerable quantity of urine.

On the 29th, another portion of the suture was removed, and more urine found in the vagina.

On the 4th of January, the union seemed complete, and the catheter was withdrawn. Half an hour afterwards, the patient felt a desire to make water, and was delighted to find herself able to pass it as does a person in perfect health, and with the same force.

On an examination instituted some time afterwards, M. Jobert found that the vagina had the same length and size as in the normal condition; and that it formed a complete cul-de-sac, not having the least communication with the uterus, whilst the latter opened into the bladder by the junction of the posterior part of the neck with the posterior wall of the urinary pouch. She was thus quite cured; the menstrual discharge passing at the regular periods into the bladder, and mixing with the urine, which it coloured red.

Two other cases are also related, in which this operation was practised. The first proved fatal by peritonitis; and the second, a most formidable one, was at last cured, after various operations had been performed. The patient had been seen in her confinement by M. Jobert himself, who was called into consultation after she had been in labour many hours. Sloughing of the internal parts took place to a frightful extent, resulting in the destruction of a great portion of the urethra, the neck of the bladder, the vesico-vaginal septum, and the neck of the uterus. A highly interesting account is given of the treatment necessary to cure this dreadful lesion; but as the principle of the operation was precisely similar to that which we have already detailed in two instances, we shall not now dwell upon it.

The next chapter is devoted to the subject of what M. Jobert calls *superficial vesico-utero-vaginal fistulæ.* These hold an intermediate position between vesico-vaginal fistulæ, and those which form a communication between the bladder and the uterus. The severity of this form is due to the destruction of the vagina surrounding the neck of the uterus, which shows no trace of its existence for a variable space. They involve the bladder, vagina, and neck of the uterus, in different degrees. The neck of the uterus is in general superficially affected; the loss of substance never extending to the cavity of the uterus. The vesico-vaginal septum, however, is very differently affected; being constantly destroyed at its insertion into the neck. Sometimes the fistula occupies only a part of the septum, but in other cases it extends for its whole length. Generally there is a large triangular opening, anteriorly at the top, and posteriorly at the base. The patients have all the symptoms of vesico-vaginal fistula; the urine constantly collecting, and flowing out of the vagina. They are more difficult to remedy than vesico-vaginal fistulæ, because the lesion is more extensive, and because in all cases the cure can only be effected by uniting the septum to the neck of the uterus, so that several successive operations have often to be undertaken.

The operation consists of two steps—viz. first, the scarification of the edges of the wound, which must include the vesical and vaginal surfaces equally; and, secondly, their approximation. The detail of the necessary

proceedings will probably be made more clear by an actual example, than by systematic description.

A female, thirty-five years of age, entered the hospital of St. Louis, on the 26th September, 1848, having sustained severe injury from a protracted labour two years before. The speculum showed that there was a large opening which occupied the whole base of the bladder, extending from the neck of the uterus to the neck of the urinary pouch, leaving an interval of barely two centimetres between the two. The fistula was triangular in shape, and usually filled by a hernia of the anterior part of the bladder, as large as a pigeon's egg. The anterior lip of the neck of the uterus was partially destroyed, and that which did remain was so soft and friable that it would not bear the employment of a hook; but at the operation it was brought forward as much as possible by indirect traction exercised on the vagina. The edges of the wound were first of all scarified, both on the bladder, and on the vaginal side. This was not merely carried out in the vesico-vaginal septum, but also in all parts of the neck of the uterus which were deprived of vaginal covering. Three sutures kept the central portion of the fistula in contact, and two others united the septum to the neck of the uterus. These three sutures were successively inserted into the base, the middle, and the apex of the triangle. Two long incisions were made along the sides of them, so as to take off all tension. A catheter was then passed into the bladder, and the patient was put to bed with the knees flexed and the thighs bent upon the pelvis, a pillow maintaining them in position.

The patient had a narrow escape of her life from an attack of fever, but eventually got over it; the fistula being about half closed.

Another similar operation was afterwards performed, attended with the same serious perils to the patient, and resulting in the almost complete closure of the remainder. The patient left the hospital to recruit her health in the country, promising to return and have the remaining small opening closed, but she did not do so. Enough, however, was done to demonstrate the feasibility of the operation, and to afford the unfortunate patient very considerable relief.

A similar case to this is that of Madame D., in whom the fistula presented a triangular form, the base resting on the uterus, which was completely closed, after several autoplastic operations, and a few subsequent cauterizations with the nitrate of silver.

A number of other such cases we are obliged, by that inexorable arbiter "space," to omit, notwithstanding their interest and importance. And we much regret being obliged to pass by the fourth division of the treatise, which relates to the cure of various kinds of fistulæ, and to the re-establishment of the functions of the urinary organs, which have been destroyed in the autoplastic operations.

Respecting *Intestino-Vaginal Fistula,* or communication of the vagina with the small intestine, M. Jobert gives a *résumé* of the knowledge possessed by the profession, and of the propositions for its cure which have emanated from various individuals—as, for instance, MM. Roux and Casamayor; but he does not appear to have had any opportunity of carrying into practice some ingenious proposals of his own, and we shall not therefore dwell upon it.

Recto-Vaginal Fistulæ may be either congenital or accidental. Of the existence of the former, however rare they may be, there can be no doubt; but of the exact causes which give rise to them, little can be said. M. Jobert relates the particulars of a few cases which are reported in various publications; but in this instance also his *personal* knowledge appears defective.

Accidental fistulæ of this kind generally result from labour, and are produced either by direct laceration, or by sloughing following excessive pressure; but there are also other and more infrequent causes which give rise to them. The latter class of fistulæ is called by the author constitutional, and comprehends such as have a syphilitic, a scrofulous, or a cancerous origin.

It is needless here to dwell upon the symptoms, physical or moral, which are produced by the existence of this lesion; it is sufficient to consider the treatment necessary for its cure. The employment of various caustics to cauterize and induce contraction of these fistulæ is well known to the profession, and needs but little illustration at our hands. The seton has been made use of chiefly in America, by Drs. Mott and Barton. The operation, however, recommended by M. Jobert, is an autoplastic one, and is to be executed thus:—

In the first place, it is of great importance that the patient's health be placed in as good a condition as possible, and that the parts themselves be prepared by removing from them every source of irritation.

The patient is to be placed on cushions, with the legs bent on the thighs, and the thighs on the pelvis. Two assistants are then to keep them in this position, and at the same time to separate the labia majora with their hands, or sometimes indeed hooks are to be employed for this purpose. A univalve speculum is also to be introduced into the vagina, for the purpose of raising its superior wall together with the bladder. The edges of the fistula are then to be thoroughly pared, and the scarification is to be carried a certain distance into the rectum and vagina, in doing which a considerable quantity of blood may be lost. This hæmorrhage is, however, easily controlled when the sutures are in their places. Interrupted sutures are afterwards to be introduced, taking care to insert them deep enough to maintain the whole thickness of the parts in contact, and to make use of so many as not to leave intervals through which gas or solid materials can escape. Following the rule which he has inculcated in his other operations, M. Jobert then recommends that lateral or transverse incisions be made into the vagina, in order to loosen it, and that a catheter be left in the bladder, or else be frequently introduced in the course of the day.

In the after-treatment, some recommend that the bowels be maintained in a constipated condition; and others,—as, for instance, Saucerotte,—that they be kept loose. Our author advocates the former plan, on the plea that everything which excites the movement of the rectum and the contraction of the neighbouring muscles is dangerous, so long as the sutures are left in their position; and he administers opium with the object of maintaining this constipation.

On the sixth day the sutures are to be withdrawn; and when the cicatrix is firm, a purgative is to be given.

Of the cases related by M. Jobert, in which this operation was per-

formed, we select the following, as the one best adapted to show the nature and extent of the lesion which it is designed to remedy.

Madame H., a robust female, 35 years of age, was confined of her first child on 5th September, 1850. The labour, a tedious one, during which the ergot of rye was administered, terminated in her delivery, by the forceps, of a child with a large head, which had been dead about forty-eight hours. She suffered a good deal afterwards from retention of urine, and from diarrhœa, and in a short time found that she had incontinence both of urine and of fæces. The use of opium produced constipation, with such a solid condition of the evacuations, that only a small portion of them passed into the vagina when she went to stool; but the urine flowed incessantly into the vagina, without her having the power to retain a single drop. In the course of the next six months, she placed herself under M. Jobert's care, when he thus describes the condition of the parts.

The labia majora and minora, as well as the inside of the thighs, were erythematous, and the seat of a sort of nettle-rash, produced and maintained by the passage of the urine over them. The vagina was obliterated, except for a space of between five and six centimétres, at which exact spot there were two openings, situated one over the other; the superior one communicating with the bladder, the inferior one with the rectum, and thus forming the vagina into a true cloaca. On the 26th of June, after some slight constitutional preparation, the following operation was performed:— By means of two incisions, made parallel with the commissures. the thickened bundle of tissue which connected the fistulæ together was destroyed, and the two fistulæ made perfectly independent of each other, so that they could be more accurately examined. The edges of the vesico-vaginal fistula were then scarified for a distance of about a centimétre all round. Three interrupted sutures were inserted, and great care was taken, in tying them, to include a good quantity of tissue. There was nothing remarkable in this operation, except the rapidity with which it was possible to perform it.

The operator's attention was next turned to the communication with the rectum. By means of the univalve speculum the superior wall of the vagina was raised, and then the edges of the fissure into the rectum were scarified in the same manner as those of the other had been. In doing this a small artery was wounded, and bled a good deal. A single suture was placed in the centre of the fistula, and its edges brought together, and then two hare-lip pins were inserted, one at each end of it. Afterwards a semicircular transverse incision was made between the sutures and the fourchette, and two other longitudinal incisions were made on the sides of the recto-vaginal fistula, extending to nearly even with the vulva. Finally, two longitudinal incisions were made by the sides of the vesico-vaginal fistula, as far as the bulb of the urethra. The effect of these various incisions was to relax the parts entirely, and to take off all strain from the sutures. A tampon was also introduced into the vagina, and a catheter into the bladder, and the patient was put to bed with the thighs bent on the abdomen. For the first few days there were many disagreeable constitutional symptoms, especially sickness and vomiting; and on the 1st of July, the patient fancied she passed air through the vagina.

On the 3rd of July,—that is to say, seven days after the operation,—an

examination was made. The vesical opening was found to be quite closed, but it was not so with that into the rectum. The needle inserted into the left angle of the fistula had fallen out, and left an aperture through which air and liquid matter made their escape. The other needles were withdrawn, but the middle suture left in its place. All the right side of the suture was in a satisfactory condition.

On the 10th, the last suture was withdrawn, and the edges found to be perfectly united; but the surface of the cicatrices being granular, they were touched with nitrate of silver.

Some air still continued to pass through the vagina, but this gradually ceased, and on the 16th, the patient had a natural evacuation. We need not pursue the detail of the case further; suffice it to say, that the patient was cured of her infirmity, and that careful examination, instituted some time afterwards, showed that all the parts maintained their natural condition.

ART. XIV.

On Diseases of the Liver. By GEORGE BUDD, M.D., F.R.S., Professor of Medicine in King's College, London; and Fellow of Caius College, Cambridge. Second Edition.—*London*, 1852. 8vo, pp. 486.

THERE have been fashions in physic. At one time, gout was on every physician's tongue, and the twinges of gout in every man's viscera. At another time, bile tinged all our thoughts, and physicians and surgeons, too, were as bilious, or perhaps more bilious, than their patients. Bile "flew about" where gout had been wont to wander. We believe that fashions in physic, so far as concerns physicians, are indeed bygones :—of course, we do not allude to the homœopathists, hydropathists, *et hoc genus omne;* they cater for the weak side of men's minds, and so long as there are weak-minded patients with deep purses, there will be knaves to play on their hopes and fears, and fools in the profession, too, to be gulled by these knaves. Fashionable works are, by their nature, transitory; those which aim to disseminate sound views in medicine, although less popular at the moment of their birth, may hope for a longer and a sounder life.

When Dr. Budd's book appeared in 1845, its worth was at once appreciated by one of our predecessors,* who analyzed it at considerable length. From that time it has taken its place as the standard British authority on the subject of which it treats. A second edition has now appeared, and Dr. Budd has evidently given a careful revision to the whole work. The two editions are got up in the same style, and printed in the same type; the present, however, is about one-sixth larger than the former; and some sections have been entirely re-written, so as to bring the work up to the present state of science on the subjects discussed. We shall offer our readers a brief outline of Dr. Budd's work generally, and a more full analysis of those parts which appear for the first time in the present edition.

In the Introduction, Dr. Budd describes the structure of the liver, the cause of the variations in its form, size, and colour; the physical qualities

and composition of the bile; the sources and uses of the bile; and chola-
gogue medicines.

Dr. Budd adopts, generally, Mr. Kiernan's account of the anatomy of the
organ; at the same time, he holds, with Mr. Bowman, that "in man and
many other animals, the lobules are not distinct, isolated bodies, but merely
small masses defined more or less distinctly by the ultimate twigs of the
portal vein, and the injected or uninjected capillaries immediately con-
tiguous;" and he gives a fair account of Dr. Handfield Jones's opinions of
the structure of the lobules, and the arrangement of their secreting cells.
Some pathological phenomena, Dr. Budd thinks, lend support to this
observer's views.   We miss, however, any account of the researches on the
structure of the Liver which have been made by Retzius, Guillot, and other
continental anatomists, subsequently to the date of the first edition.

After a few words on the general causes of the variations in the form
and size of the organ, Dr. Budd proceeds to consider on what circumstances
the colour of the liver depends; these are, the quantity of blood in the
capillary vessels, and the quantity of oil and of biliary colouring matter in
the cells; the actual tint of the liver being the combined effect of the tints
due to the vessels and the cells respectively.

Dr. Budd adopts Mr. Bowman's opinion, that tendency of the blood to
collect after death in the centre of the lobules, is owing to the capillaries
in the marginal portions of the lobules being subject to greater pressure
than those in the centre, in consequence of the cells in the former part
being more often distended with oil and larger than those in the latter; and
the frequency with which the central portion of the lobules is found deeply
injected, while the margin is pale, affords, Dr. Budd thinks, a striking con-
firmation of Dr. Handfield Jones's theory, that the process of secretion
begins in the centre of the lobules and reaches its completion at their
margins.

All the recent chemical researches into the composition of the bile are
incorporated by Dr. Budd into this part of his work.   The statements of
Demarçay, Kemp, Liebig, Mulder, and Strecker, are ably and clearly
analyzed.   According to the most recent researches, ox-bile is composed
essentially of glyco-cholate and tauro-cholate of soda, in nearly equal pro-
portion—compounds which differ essentially, inasmuch as the latter con-
tains six per cent. of sulphur.   It is the tauro-cholate of soda, especially,
that holds the cholesterine of the bile in solution.

Glyco-cholic acid is resolvable into glycocol, or sugar of gelatine
(C_4 H_5 NO_4), and an acid, called by Strecker cholalic, and by Demarçay
cholic (C_{48} H_{40} O_{10}); tauro-cholic acid is, in like manner, resolvable into
taurine (C_4 H_7 $N S_2$ O_6), and cholic, or cholalic acid.   When reviewing
Lehmann's work,[*] we described at length the mode of testing for cholic
acid, which is, in fact, practically the same thing as testing for bile.   The
relative proportion of the tauro-cholic and glyco-cholic acids varies in
different animals; in marine fishes the basis of the bile, instead of being
soda, as it is in man and such of the domestic animals as were examined
by Strecker, is potash.

As to the colouring matter of the bile, it is, according to Berzelius,
identical with chlorophyl, the green colouring matter of plants.   In his

* British and Foreign Medico-Chirurgical Review, vol. viii. p. 162.

'Lectures on Animal Chemistry,' Dr. Bence Jones has given,—Dr. Budd observes,—

"Various good reasons for believing that the urine mainly owes its colour to the same source; and that the various shades of yellow, brown, and pink, which the sediments of the urine present, like the similar tints of the autumnal leaves, are due to different degrees of oxidation of the peculiar matter to which the green colour of plants and of the bile is owing. This hypothesis may serve to account for the influence, frequently to be noticed in the following pages, which organic diseases of the liver have in causing red and pinkish sediments in the urine." (p. 34.)

Dr. Budd considers it "probable that the peculiar colours of bile, urine, and blood, result from different modifications of the same pigment."

As to the question, whether the bile is formed in the liver, or merely separated from the blood by that organ, Dr. Budd thinks that the fact of an accumulation of the colouring matter of the bile in the blood, when the secretion of bile is suppressed, leaves "little doubt that the colouring matter of bile exists ready-made in blood;" while he holds as probable that the biliary acids "are formed in the liver by the agency of the secreting cells." The results of the observations of Bernard and Lehmann, on the presence of sugar in the blood of the hepatic vein, are given at some length by Dr. Budd. With reference to the uses of the bile, Dr. Budd remarks, that although observation has shown conclusively that all the staminal principles of the food may be digested and absorbed without the aid of the bile, still, a gradual impairment of nutrition ensues when there is permanent closure of the common gall-duct. The bitter principle of the bile is supposed to prevent the fermentation of the chyme, and the occurrence of putrefactive changes in the nitrogenized constituents of the food. "It is well known," Dr. Budd remarks, "that it is common in jaundice for the bowels to become flatulent, and the stools unusually fœtid."—The concluding part of the Introduction on the remaining uses of the bile, and on cholagogue medicines, is merely a reprint from the former edition.

The First Chapter, which is devoted to *Congestion of the Liver*, has been entirely re-written. A mechanical impediment to the return of blood through the veins to the heart, is one of the most common causes of congestion of the liver. The impediment is ordinarily some organic disease of the valves on the left side of the heart. Under these circumstances, the edge of the liver may sometimes be felt "two or three inches below the false ribs. If the circulation be relieved by bleeding, or by diuretics, or by rest, the organ returns to its former size." This increased diminution in the size of the liver, Dr. Budd says, often takes place very rapidly.

Enlargement of the liver from this cause is unattended by pain; the only local sensations experienced by the patient being a sense of weight or fulness in the right hypochondrium. Jaundice is a common result. The immediate cause of the jaundice, Dr. Budd considers to be the slowness of the current through the capillary vessels of the hepatic lobules interfering with the secretion of the bile, and the pressure on the small gall-ducts by the gorged bloodvessels impeding the excretion of the bile which is formed.

After death, the organ is found of a deep red colour, and more friable than natural. If any portions of the lobules are uninjected, they are deeply tinged with bile in consequence of "biliary congestion." If the biliary con-

gestion be kept up for any length of time, then the hepatic cells are permanently injured.

" Now and then, in persons who die of valvular disease of the heart of long standing, the liver is found much diminished in size and weight, without presenting any marks of inflammation or other striking change; and this may occur in persons who have led temperate lives; where, consequently, the atrophy can only be explained by the influence which the long-continued congestion has had in impairing the functional activity and nutrition of the cells. The wasted condition of the muscles of the legs that so constantly results from serious valvular disease of the heart, affords another and familiar example of atrophy thus produced." (p. 57.)

No mechanical impediment to the return of blood from the liver, Dr. Budd thinks, can give rise to cirrhosis, or " to *inflammation* of any kind." Bloodletting, local or general, saline purgatives, and mercurials in small doses, are the remedies which afford the most marked relief.

Congestion of the liver may arise from changes in its tissues, calling for an increased supply of blood, and also from an alteration in the blood itself.

" All the matters absorbed by the bloodvessels in the intestinal canal have to pass through it. Its lobular substance is the first filter, if we may so term it, through which the impure liquid must strain. All the alcoholic drinks, all the noxious ingredients that may chance to be present in our food, and such hurtful products of faulty digestion as are readily soluble, are immediately—before they have been diffused throughout the whole mass of the blood, and before they have been submitted to the influence of oxygen—carried to the liver. Amid the continual excesses at table of persons in the middle and upper classes of society, an immense variety of noxious matters find their way into the portal blood that should never be present in it; and the mischief which this is calculated to produce is enhanced by indolent or sedentary habits. The consequence often is, that the liver becomes habitually gorged. The same, or even worse effects, result in the lower classes of our larger towns, from their inordinate consumption of gin and porter." (p. 61.)

Other conditions of the blood which induce congestion of the liver are those which exist in the hot stage of ague, and in purpura hæmorrhagica.

Hæmorrhage, either into the substance of the organ or immediately under its capsule, is a rare consequence of congestion. In the former situation no symptoms indicate its occurrence.

" When hæmorrhage takes place at the surface of the liver, the blood may collect under the capsule, and form a palpable tumour; or may even rupture the investing membranes, and thus become effused into the peritoneal sac. It then, of necessity, causes much pain and tenderness, which are more or less widely diffused, according to the nature of the injury. The pain and tenderness are, however, seldom of long continuance. The effused blood does not inflame the serous membrane, and, in consequence, the pain and tenderness disappear entirely, or much abate, in the course of a few days." (p. 63.)

An interesting case is given in illustration of these remarks.

Dr Budd's Second Chapter is allotted to *Inflammatory Diseases of the Liver.* It is divided into five sections. Inflammatory diseases should be arranged, Dr. Budd thinks, according to the nature of their causes : in the present state of knowledge, however, such an arrangement of inflammatory diseases of the liver is impracticable.

" But, as the nature of the cause mainly determines the character of the inflammation and its mode of termination, some approximation to such an arrangement

will be obtained by classing them according to their effects. I propose, therefore, to range the inflammatory diseases of the liver under the following heads :

" 1st. *Suppurative* inflammation, or that which leads to suppuration and abscess;

" 2nd. *Gangrenous* inflammation;

" 3rd. *Adhesive* inflammation, or inflammation that causes effusion of coagulable lymph;

" 4th. Inflammation of the veins of the liver;

" 5th. Inflammation of the gall-bladder and gall-ducts." (p. 67.)

Suppurative Inflammation of the Liver.—Dr. Budd's experience, since the appearance of the first edition of his work, fully confirms the opinions he then advanced as to the causes of this affection. These causes are :—1st. Mechanical injury ; an exceedingly rare cause, however. 2nd. Contamination of the blood with pus; the consequence of suppurative inflammation of a vein. 3rd. Ulceration of some part, the blood of which is returned to the portal vein. This last is by far the most common of the causes of hepatic abscess. The vitiated condition of the blood in these cases may be the result of an admission of pus, the products of softening of the tissues, the fœtid gaseous and liquid contents of the large intestine in dysentery, or any other irritating substance.

The lesions of structure consequent on suppurative inflammation of the liver, and the symptoms of that affection, are given at considerable length. We pass these by, little new matter having been added. Two interesting cases of recovery from hepatic abscess, the pus having been ejected through the bronchial tubes, are briefly detailed.

With reference to the treatment, Dr. Budd adheres to the opinion he previously expressed, that mercury is " on many grounds peculiarly unsuited to suppurative inflammation of the liver." Abscess of the liver, even when it projects at the side, should, according to our author, be allowed to open of itself.

" The prominent part should be poulticed, and the matter be allowed to escape in the poultices, but should not be squeezed or pressed out. Nature performs the operation better than the surgeon. When the abscess opens of itself, it is usually by a very small aperture, like those in worm-eaten wood, which never closes ; and the matter gradually oozes out as the sac contracts. No air gets mixed with the matter of the abscess, and no violence is done to its walls ; and, consequently, no fresh inflammation is set up. The discharge is very gradual, and as small in quantity as it can be. There is less shock to the system, and less drain from it, than when the abscess is freely opened by the knife." (p. 117.)

We shall pass over the remaining sections on inflammatory diseases of the liver, merely remarking that the only new matter introduced consists of a few well-narrated cases, and some good observations bearing on particular subjects, confirming the opinions previously maintained by Dr. Budd on those diseases, and some new explanations of obscure points offered.

When describing the treatment of inflammation of the gall-bladder and ducts, Dr. Budd speaks in high terms of the value of muriate of ammonia.

The Third Chapter is devoted to a consideration of diseases which result from faulty nutrition of the liver, or faulty secretion.

The first section of this chapter is on *Fatal Jaundice*. It is one of the most interesting in the work. In the former edition, this section contained thirty-one pages ; in the present it occupies forty-eight. Destruction of

the hepatic cells, the active agents in the elaboration of the bile, "may result," Dr. Budd states, "from long retention of the secreted bile from closure of the common gall-duct." The tissue of the organ under these circumstances is "flabby, but not readily broken down by the finger, and *presents no appearance of lobules.*" Under the microscope, only free oil-globules and irregular particles of solid biliary matter are to be detected.

"But," Dr. Budd adds, "destruction of the hepatic cells may take place rapidly, without any obstruction of the gall-ducts, and instead of being consequent on protracted jaundice, the impaired nutrition of the cells may be the cause of jaundice that proves rapidly fatal from disorder of the functions of the brain." (p. 235.)

In the first edition of his work, Dr. Budd detailed five cases which he considered examples of this affection. One only of the five, however, had fallen under his own observation, and in that case only had the liver been examined by the microscope. He has now added the particulars of five other cases which have fallen under his own or his immediate friends' observation. The following is a summary of one of these cases:

"A lad, seventeen years of age, who had been leading a dissolute life, became jaundiced. The jaundice had continued three weeks before he came under observation, and, it would seem, before he was compelled to lay up. At the end of that time, it was attended with much depression and with considerable gastric and intestinal disorder; but there was no tenderness in the region of the liver, nor pain of any kind. The matters discharged from the stomach and bowels were seldom tinged with bile. At the end of another week, without any other striking change having occurred, delirium came on, and he died five days afterwards, in a state of coma.

"On examination after death, great part of the liver was of a yellowish-brown colour, and much softened, presenting disintegrated cells, but not entirely disorganized, and still showing the lobular structure. The rest of the liver, which was firmer, and of the dark colour of venous blood, seemed already to have undergone atrophy from destruction of the cells. The lobules could not be distinguished in it, and when a particle from it was placed under the microscope, only a few small hepatic cells could be seen.

"The cortical substance of the kidneys was large, soft, and friable; and was clearly the seat of a morbid process analogous to that which had so changed the texture of the liver.

"Hæmorrhage from the stomach or bowels, which occurred in some of the preceding cases, was not noticed; but the omentum was thickly sprinkled with ecchymosed spots." (pp. 251, 252.)

In one case, the notes of which were taken by Dr. Budd's brother, the substance of the liver and the bile in the gall-bladder had an intensely acid reaction; this fact, Dr. Budd thinks, "will turn out to be very important."

From the very outset of the affection, the liver ceases to secrete bile, "and before long the secreting cells break down, or are not reproduced;" the consequence of this is rapid diminution in the size and weight of the organ. At one part of the liver the disease may be considerably advanced, while at another part it is only just commencing. An observation of Dr. Handfield Jones, quoted by Dr. Budd, renders it probable that the "process of disorganization begins in the centre of the lobules." In one case there were purpuric spots on the omentum, and in others blood was passed from the gastro-intestinal mucous membrane. The congestion of the alimentary canal, in these cases, our author thinks, is caused by the arrest of secretion in the liver, and consequent impediment to the passage of blood through it.

As to the causes of the disease, age seems to exert a marked influence. In one case only of the ten detailed by Dr. Budd, had the patient attained middle age; seven were between 17 and 30, one 35, and one 37 years of age. Dissipation and depressing passions seem to be efficient exciting causes of this fatal disease.

Before the supervention of the cerebral symptoms, these cases are not to be distinguished from ordinary cases of jaundice. The stools are clay-coloured, or slightly tinged with bile. Pain or tenderness in the region of the liver is usually present. Delirium may be the first symptom to alarm the physician or the friends of the patient. The head-symptoms in such cases, Dr. Budd observes, " have been generally attributed to the retention of the principles of the bile in the blood; but there is abundant evidence to show, that the mere retention of bile in its natural state produces no such effects." The truth of this statement cannot be doubted; every physician's experience enables him to testify to it. Casting this supposition on one side, two others suggest themselves:

" The first is, that these symptoms are caused by the *direct* action of the poison which caused the jaundice. A great difficulty in the way of this supposition is the *sudden and unexpected* occurrence of the head-symptoms, in some of the cases related above, after the jaundice had lasted for some time. It is clear from the symptoms that some deadly agency—sufficient, in one case, to destroy life in fourteen hours—came then *suddenly* to act on the nervous system. If this were the poison that had before arrested the secretion of the liver, and caused the jaundice, the poison must at first have been retained in the liver, like globules of pus or mercury, and from some cause or other have been suddenly liberated to exert its action on the nervous system. It is well known that poisons which kill by stupifying the nervous system, take effect rapidly on their passing into the blood; and in some of the preceding cases there was no sign, for several days or even weeks after the occurrence of jaundice, that the brain was under the action of a narcotic poison." (p. 263.)

The above reasoning will, we think, be regarded as offering insurmountable objections to this hypothesis.

" Another supposition that offers," Dr. Budd continues, " a better explanation of the facts than either of the others, is, that in consequence of *decomposition* of the retained principles of the bile, or of the broken-up hepatic cells, some peculiarly noxious agent is evolved which is the real cause of the malignant symptoms." (p. 264.)

In substance, this is the same hypothesis as that propounded by Dr. Bence Jones and Frerichs, to account for the sudden supervention of cerebral symptoms in cases of uræmia—viz., the sudden decomposition of foreign matter circulating in the blood. If Frerich's observations be correct, it will be remembered that, as regards uræmia, this is now a well-founded theory. In respect to cholæmia, it can only be yet held as an hypothesis, although a very ingenious and not improbable one.

" Circumstances much in favour of this supposition," Dr. Budd adds, " are, that the cerebral disorder occurs rather suddenly, at very variable times after the occurrence of jaundice; and that sometimes jaundice caused *by fright* proves fatal exactly in the same way." (p. 264.)

In some forms of fever, Dr. Budd remarks, as well as after the bites of some poisonous serpents, the secretion of bile is arrested, and jaundice is present.

Although almost all the cases of jaundice from suppressed secretion detailed by Dr. Budd proved fatal, yet he thinks that this is by no means necessarily a fatal disease; nay, in his opinion, in a large proportion of the cases of jaundice occurring in youth, " the fault is in the secreting cells, and the jaundice results from suppressed or deficient secretion."

As to the diagnosis between jaundice from arrested secretion, and jaundice from temporary closure of the gall-ducts, Dr. Budd thinks we may infer the former to be its cause, when it immediately follows a powerful emotion, occurs in the course of suppurative phlebitis, or in consequence of poisoning, or when it affects several members of the same family in succession. The other circumstances which would lead to the same inference, though less conclusively, are, the patient being young and of dissolute habits, or depressed in spirits; the absence of enlargement of the liver; much vomiting, hiccough, or other gastric or intestinal disorder; the occasional passage of bilious matters from the stomach or bowels; and symptoms of depression. Hæmorrhage from the stomach or bowels, delirium, coma, or convulsions, supervening in the course of jaundice, Dr. Budd regards as almost diagnostic of suppression of the secretion of bile.

Diminution in the size of the liver, if it can be satisfactorily made out, is " one of the surest signs of this kind of jaundice."

Hereafter, Dr. Budd thinks it probable, some peculiar condition of the urine may be discovered in this affection :

" I have long suspected that the presence of oxalate of lime in the urine will turn out to be an important indication of it. In many cases of jaundice which I have supposed to be of this kind, the urine contained oxalate of lime, which disappeared from it as the jaundice went off. In more than one case of the kind I have found in the urine, with the oxalate of lime, casts of the secreting tubules, or evidence of the rapid shedding of the epithelium of the secreting tubules of the kidney." (p. 280.)

None of the cases, however, here referred to, proved fatal.

In regard to treatment, Dr. Budd states that in cases of jaundice which he has supposed to arise from suppressed secretion, he has found the most useful medicine to be

" From ʒss. to ʒj. of sulphate of magnesia, in conjunction with gr. xv. of carb. of magnesia, and ʒss. of aromatic spirits of ammonia, three times a day,—the sulphate of magnesia to keep up free action of the bowels; the carbonate of magnesia to neutralize any excess of acid in the stomach or bowels; and the aromatic spirit of ammonia to support the nervous system, and to keep up the action of the skin." (pp. 280, 281.)

The second section of the Third Chapter is devoted to *Fatty Liver.*

The livers of two individuals, aged respectively 31 and 40 years, who died suddenly, the one in consequence of a fall, and the other from cerebral hæmorrhage, were analyzed, at Dr. Budd's request, by Mr. L. S. Beale. In that removed from the former, a male, he found 3·82 per cent. of fat, in that of the latter, a female, 4·28 per cent. As these results accord pretty well with those obtained by Von Bibra, it may be considered that it is rare for a healthy liver to contain more than 5 per cent. of fat, and that usually it ranges between 2 and 4 per cent. In the liver of a drunkard, which " was in a state of cirrhosis, as well as of fatty degeneration, and, in consequence, presented a very remarkable 'hob-nailed' appearance, from the

nodules of cirrhosis being enlarged by the accumulation of oil," Mr. Beale found 65 per cent. of fat—"about six-sevenths of all the solid matters in the liver consisted of fat." It is interesting to observe, that in this case no organ save the liver was found notably diseased.

It is unnecessary to follow Dr. Budd through the remainder of his remarks on fatty liver, as they contain little that is new. In diabetes and in the scrofulous liver, there is less fat than in the healthy viscus.

The third section contains a description of *Scrofulous Enlargement* of the liver, and other kindred states.

With reference to the waxy liver, Dr. Budd's observations agree with those of the most recent pathological anatomists, that the foreign matter which produces the waxy appearance is albuminous, and not fatty as Laennec supposed; this waxy liver is, doubtless, as Dr. Budd considers, identical with what is termed scrofulous enlargement of the liver.

Dr. Budd details at some length four cases of this disease. From these cases we learn, 1st, that a liver thus affected becomes enormously enlarged and thickened. 2nd. It is extremely pale, and contains after death but little blood. 3rd. When the disease is far advanced, the lobules can scarcely be distinguished; "the hepatic substance is uniform and compact, and at the same time somewhat glistening, or semi-transparent, so that the cut surface looks very much like that of compact bacon." It is very tough, and generally whitish. 4th. The foreign matter to which the liver owes its size is albuminous. It exhibits no tendency to contract. 5th. The foreign matter is situated within the lobules, but it is not within the cells; the latter are few in number, not enlarged, and contain certainly not more than their normal amount of fat; thus Mr. Beale analyzed the livers in two of the cases detailed by Dr. Budd : in one he found one per cent. of fatty matter, and in the other only half a grain of fat in one hundred of the liver-substance. But then, in these cases, the liver was twice its normal size, from interstitial deposit of the albuminous material.

Dr. Budd's cases further show, that scrofulous enlargement of the liver frequently accompanies scrofulous caries, with profuse suppuration; and that a similar affection of the kidney is frequently found in the same subject. This lesion of the kidney Dr. Budd considers to be the consequence of the hepatic disease.

Scrofulous enlargement of the liver comes on without pain, and most insidiously. "The first evidence that the liver is diseased is furnished by its large size." The surface of the organ is smooth; its lower edge is rounded. After a while, the superficial veins of the abdomen enlarge, a little fluid collects in the peritoneal cavity, and there is slight yellowness of the skin. These symptoms occurring in a young person, the subject of scrofulous disease of the bones, and accompanied by albuminous urine containing waxy-looking casts of tubes, are unequivocal evidence of the disease in question.

The scrofulous or waxy liver is sometimes found in persons whose health has been destroyed by syphilis and the abuse of mercury. With reference to treatment, the principal aim of the physician must be, to remove the peculiar cachexy to which the enlargement of the liver is secondary. Dr. Budd thinks he has seen benefit from the prolonged use of muriate of ammonia. And in a case which he details, "the amendment

seemed to result from properly regulated diet, and the prolonged use of the sesquicarbonate of ammonia, in doses of five grains twice a-day."

In the fourth section of this Chapter, the subject of *Excessive and Defective Secretion of Bile,* and of *Unhealthy States of the Bile,* are considered. But little has been added by Dr. Budd to his former excellent remarks on the two first-mentioned conditions. With regard to unhealthy bile, he remarks that little is known of the chemical constitution of abnormal bile; at the same time he refers to four cases, detailed in his present work, in which the bile had a markedly acid reaction; and then adds:

" I have met with several other instances in which the bile, and the liver itself, had a very distinct acid reaction ; and in most of them the bile, as in the instances noticed above, was unnaturally pale. Deficiency of the proper biliary colouring matter seems to promote the occurrence of an acid condition in the bile.

" The immediate cause of this acid condition of the bile is most probably decomposition of the bile, or of the mucus it contains. When ox-bile is allowed to decompose exposed to the air, it becomes after some days distinctly acid ; and if it be then neutralized, it becomes, on further standing, acid again." (p. 342.)

The circumstances under which we may expect to find the bile acid are, Dr. Budd says, when it is very pale, or contains a large proportion of unhealthy mucus; when rapid decomposition of the body ensues; or when the body is examined long after death in hot weather.

" We may expect, also, sometimes to find it acid in those cases of jaundice from suppressed secretion, in which the unexpected occurrence of fatal head-symptoms, the softened state of the liver after death, and the early putrefaction of the body, evince the existence of some noxious agent which seems to be developed in the system by decomposition of the broken-up hepatic cells, or of the retained elements of the bile.

" In some instances the bile may be rendered acid, as Gorup-Besanez has suggested, by the presence of pus, generating lactic acid It is possible, however, that, under certain circumstances, bile may be acid when first secreted." (p. 343.)

In cholera, urea has been detected in the bile. After death from typhoid fever (Louis), pneumonia (Gorup-Besanez), and dysentery (Parkes), the bile is very frequently found to be pale, thin, and watery. In other diseases, and in some cases of those above mentioned, the bile is unusually dark-coloured and thick; this is " always the case in persons who die during the cold stage of malignant cholera," and often so in those who die after long fasting. In these cases, Dr. Budd supposes the bile to become concentrated after its entrance into the gall-bladder. But in some cases the bile is " *secreted* unusually viscid, and unusually dark-coloured." Temporary retention of viscid or unhealthy bile in the gall-bladder, Dr. Budd thinks, occurs now and then about the middle period of life; Annesley considers it a frequent ailment in India. Solid biliary matter may be deposited from thick, unhealthy bile in the gall-bladder, and form the nucleus of a gall-stone.

Dr. Budd observes, that he has never seen crystals of cholesterine in the hepatic duct, common as they are in the cystic bile. " Cholesterine," he says, " seems, in most cases, to be formed in the gall-bladder, or at least to be there deposited in crystals, and its presence in visible scales in the bile is generally associated with disease of the gall-bladder."

A few lines only of new matter have been added to the fifth section of this chapter, which is devoted to the subject of *Gall-Stones.* This section is decidedly the best English treatise on the subject in question.

The Fourth Chapter is *On Diseases which result from some Morbid Growth foreign to the natural structure.* It includes cancer of the liver; encysted knotty tubera of the liver; and hydatid tumour of the liver. Little addition has been made to this chapter. Five cases of hydatid tumour of the liver, which have fallen under his observation during the last six years, are given by Dr. Budd. One of these cases was fatal, from the situation and size of the hydatid tumour interfering with the respiration and circulation. Three recovered; in one of the three, the hydatids were discharged into the intestinal canal, and in the other two into the lung. One case, in which the patient has, during four months, coughed up more than two quarts of hydatids, their source being the liver, is still under treatment.

Dr. Budd leans to the opinion, that hydatid tumour of the liver should, like abscess of that organ, be allowed to open spontaneously.

The Fifth Chapter is on *Jaundice.* It contains little that is new, excepting such as we have already extracted from the preceding chapters.

The Appendix on the *Liver-fluke* remains as in the former edition.

The full digest we have given of the new matter introduced into the present volume, is evidence of the value we place on it. The fact, that the profession has required a second edition of a monograph such as that before us, bears honourable testimony to its usefulness. For many years, Dr. Budd's work must be the authority of the great mass of British practitioners on hepatic diseases; and it is satisfactory that the subject has been taken up by so able and experienced a physician.

Art. XV.

1. *On the Reciprocal Agencies of Mind and Matter, and on Insanity: being the Lumleian Lectures delivered at the Royal College of Physicians, 1851.* By J. C. BADELEY, M. D. Cantab., F.R.C.P., Inspecting Physician to the Lunatic Asylums of Essex, &c. &c.—*London,* 1851. Royal 8vo, pp. 60.

2. *The Philosophy of Spirits in Relation to Matter: showing the real existence of two very distinct kinds of Entity, which unite to form the different Bodies that compose the Universe, Organic and Inorganic, by which the Phenomena of Light, Heat, Electricity, Motion, Life, Mind, &c., are reconciled and explained.* By C. M. BURNETT, M.D.—*London,* 1850. 8vo, pp. 312.

3. *Enquiry into the Nature and Effects of the Nervous Influence, and its Connexion with the Vital, Moral, and Intellectual Operations; A Physiological, Metaphysical, and Moral Essay, in Three Parts.—Paris,* 1836. 8vo, pp. 238.

WE are not about to inflict upon our readers, as the titles of the above works might lead them to anticipate, any *réchauffé* of those worn-out controversies, wherein Matter and Mind have been placed in antagonistic positions, and have been made to do fierce battle with each other, not for any quarrel of their own, but because it has pleased the philosophers of the day to set them up as standards of hostile opinions, and to use the

words *materialism* and *spiritualism* as the symbols of party creeds, round
which all kinds of prejudices and malignant feelings—the offspring of
ignorance and ill-nature—found it convenient to rally.   We need scarcely
tell our readers that ' Materialism' has been continually regarded by those
who have assumed the title of Spiritualists, as synonymous with Atheism,
or at any rate with Pantheism ; a charge which has had its foundation in
the unblushing avowal of these forms of *dis*belief by the Materialists of cer-
tain continental schools, for whose scepticism in regard to all which they
could not recognise by the direct evidence of their senses, no absurdity has
seemed too monstrous.   And it has been by these much-abused men, who
seem to have been partly driven to the extreme position which they took,
by the ignorant credulity, superstition, and (too frequently) hypocrisy of the
religious professors of their day, that the coarsest abuse and the vulgarest
sneers have been uttered against all that minds capable of appreciating
the realities of their own higher nature, and of spiritually discerning the
hidden soul of the Universe that environs us, have agreed to hold most
sacred.   But to identify the refined materialism of Priestley,—who was a
firm believer, not only in the existence of the Deity, but also in the immor-
tality of Man, and whose conception of *matter* itself was such as almost to
justify a claim for him to a place among the ' spiritualist' philosophers,—
with the gross doctrines professed by the French Encyclopedists, is about
as just as it would be to make the advocates of Spiritualism answerable
for all the absurdities which have been sheltered under that designation,
and which are even now finding their credulous dupes among those who
esteem themselves more enlightened than their fellows.   It is no more
an essential part of materialistic philosophy to reject the notion of a per-
sonal Deity and of the Immortality of the Human Soul, than it appertains
to the spiritualist's creed to admit the reality of witchcraft, to believe in
the visions of Emmanuel Swedenborg, to accredit the revelations of the
Ponghkeepsie seer, to acknowledge the ' knockings' of the Salem girls as
angel-visitations, or to regard the fasting girl now exciting the wonder of
the good people of Suffolk, as a divinely-inspired prophetess.   The cause
of truth is never benefited by the use of unjust and opprobrious names,
nor by the attempt to saddle an opponent with a discreditable or absurd
doctrine which he does not profess.   And it is to the credit of many of
the greatest thinkers of this country, that they have made a stand against
the prejudices of the ignorant multitude ; and, whilst themselves advo-
cating a spiritualist creed, have boldly proclaimed that we know too little
of the abstract nature, either of Mind or Matter, to be able to determine
their mutual relations or essential differences, and that there is nothing in
the fundamental dogma of the Materialist doctrine, which is necessarily
associated either with infidelity or with immorality.   We might fortify
this assertion with a host of quotations ; but to do so would be altogether
beside our present purpose.   For we are not about to present ourselves
as the champions of either party, nor do we intend to attempt a review of
the whole of this antiquated controversy; being firmly convinced, that its
further prosecution can lead to no benefit, either to science itself, or to
those who engage in the warfare.   In fact, a controversy of this kind
resembles a war between two nations, which are capable of mutually bene-
fiting each other in the highest degree, but which, under the influence of

some paltry pique, engage in the unprofitable task of endeavouring to determine which of them can do the other the most harm. Here, as has been too often the case in philosophical disputations, that which God hath joined together, man has foolishly striven to put asunder; and the aim of the disputants, on both sides, has been, to show the distinctness and irreconcilableness of the separate notions of Mind and of Matter, instead of attempting to trace out their mutual relations, as shown in the connexion of their actions.

In bringing this subject, then, under the notice of our readers, we have it in view to suggest whether there be not a mode of looking at it, which is essentially different from that under which it has been hitherto surveyed. The two knights in the fable, it will be remembered, nearly slew each other in the encounter about a shield, which one maintained to be of gold, the other of silver, the two seeing it from opposite sides ; and having struck up a temporary truce, they made the discovery, that each was right about his own side, and wrong in regard to the other. Now the moral which we would draw from this fable is, that as the gold and silver sides *touched each other* midway, so Mind and Matter, notwithstanding their differences, come into a mutual relation, which it is impossible for any one who thinks of the subject to ignore ; and it is to the enquiry into the nature of that relation, that we would now direct our readers' attention, under the belief that more valuable information is to be gained by seeking for it at the points of contact, than can be obtained by the prosecution of the older methods of research, in which Mind has been studied apart from its material instruments, and Matter has been weighed and measured, tested and analyzed, as if its properties were self-derived and self-dependent.

We do not claim for ourselves any novelty in this enquiry; for it has been started more than once—though never, that we are aware of, systematically prosecuted. " There is one view of the connexion between Mind and Matter," says Professor Dugald Stewart, " which is perfectly agreeable to the just rules of philosophy. The object of this is, to ascertain the laws which regulate their union, without attempting to explain in what manner they are united. Lord Bacon was, I believe, the first who gave a distinct idea of this sort of speculation ; and I do not know that much progress has yet been made in it. In his book, *De Augmentis Scientiarum*, a variety of subjects are enumerated, in order to illustrate its nature ; and undoubtedly most of these are in a high degree curious and important." The following list comprehends the chief of those he has mentioned, with the addition of several others recommended to the consideration of philosophers and medical inquirers, by the late Dr. Gregory, in his ' Lectures on the Duties and Qualifications of a Physician.'—1. The doctrine of the preservation and improvement of the different senses. 2. The history of the powers and influence of imagination. 3. The history of the several species of enthusiasm. 4. The history of the various circumstances in parents that have an influence on conception, and the constitution and characters of their children. 5. The history of dreams. 6. The history of the laws of custom and habit. 7. The history of the effects of music, and of such other things as operate on the mind and body, in consequence of impressions made on our senses. 8. The history

of natural signs and language, comprehending the doctrine of physiognomy and of outward gesture.   9. The history of the power and laws of the principle of imitation.   To this list various other subjects might be added ; particularly, the history of the laws of memory, in so far as they appear to be connected with the state of the body ; and the history of the different species of madness.—There are many other topics of inquiry which we might suggest, as tending to throw light on this mysterious question. In fact, there is not one of those affections of the intellect, of the temper, or of the passions, that is clearly traceable to a perverted condition of some part of the bodily system, which does not suggest the inquiry, *how* the mind comes to be thus acted on.   Our very familiarity with this class of phenomena has been an obstacle to our deeper scrutiny into its nature ; or the fact that a little alcohol or opium in the blood proceeding to the brain, quickens the intellectual faculties, and exalts the passions, then suspends the controlling power of the will, and finally extinguishes for a time all mental activity ; or that a deficient elimination of the constituents of bile from the circulating fluid tends to weaken the intellectual power, and to induce depression of spirits, even exciting the desire of self-destruction,—would have been regarded as of fundamental importance. If we first briefly consider the physiological and psychological bearings of the antagonistic doctrines which have been hitherto in vogue, we shall then be better prepared for the examination of what we might term the 'plane of contact.'

Few if any physiologists would be disposed to deny, that the Cerebrum is the instrument of our higher psychical powers; but the ideas which are entertained of the nature of this instrumentality have been seldom clearly or consistently defined.   Some, who have attended exclusively to the close relationship which indubitably exists between corporeal and mental states, have thought that *all* the operations of the mind are but manifestations or expressions of material changes in the brain; that thus man is but a thinking machine, his conduct being entirely determined by his original constitution, modified by subsequent conditions, over which he has no control, and his fancied power of self-direction being altogether a delusion; and that notions of *duty* or *responsibility* have no real foundation, man's character being formed *for* him, and not *by* him, and his mode of action in each individual case being simply the consequence of the reaction of his cerebrum upon the impressions which called it into play. On this creed, what is commonly termed criminality is but one form of insanity, and ought to be treated as such; insanity itself is nothing else than a disordered action of the brain; and the highest elevation of man's psychical nature is to be attained by due attention to all the conditions which favour his physical development.   The latest and most thorough-going expression of this doctrine that we have met with, is contained in the extraordinary correspondence published last year between Miss Martineau and Mr. Atkinson, from which we make the following brief quotations, whose purport, we can honestly assure our readers, is not in the least altered by their detachment from the context : "Instinct, passion, thought, &c., are effects of organized substances." "All causes are material causes." "In material conditions, I find the origin of all religions, all philosophies, all opinions, all virtues, and 'spiritual conditions and in-

fluences,' in the same manner that I find the origin of all diseases and of all insanities in material conditions and causes." "I am what I am; a creature of necessity; I claim neither merit nor demerit." "I feel that I am as completely the result of my nature, and impelled to do what I do, as the needle to point to the north, or the puppet to move according as the string is pulled." "I cannot alter my will, or be other than what I am, and cannot deserve either reward or punishment." Now this honestly-expressed *Materialist* doctrine recognises certain great facts, on which the unprejudiced and observant physiologist can scarcely entertain a doubt, notwithstanding that their validity may be denied by those who have had comparatively little opportunity of studying them, or who have so made up their minds to a foregone conclusion, as to be ready to admit nothing which is not in accordance with it. The whole series of phenomena which so plainly mark the influence of the body on the mind, of physical upon psychical states,—the admitted dependence of the normal activity of the mind upon the healthful nutrition of the brain, and upon its due supply of oxygenated blood,—the extraordinary influence of local affections of the cerebrum upon the normal succession of intellectual operations, as is espe-cially seen in the strange disturbances or dislocations of the memory con-sequent upon blows on the head,—the large share which certain states of bodily disorder on the part of the parents, or conditions tending to induce defective nutrition during the periods of infancy and childhood, have been proved to possess in the induction of Idiocy and Cretinism,— the complete perversion of all the mental powers and moral feelings, amounting to a temporary insanity, which is produced by intoxicating agents,—these and numerous other phenomena might be cited in support of the materialist doctrine; and must be accounted for by any one who undertakes the solution of this mystery. But these phenomena are not to be looked at, to the exclusion of the facts of our own internal consciousness. In reducing the Thinking Man to the level of " a puppet that moves accord-ing as its strings are pulled," the Materialist Philosopher places himself in complete opposition to the undoubting conviction which every one pos-sesses, who does not trouble himself by speculating upon the matter, that he really possesses *a self-determining power*, which can rise above all the promptings of external suggestion, and can, to a certain extent, mould external circumstances to its own requirements, instead of being com-pletely subjected to them. We can scarcely desire a better proof that our possession of this power is a reality and not a self-delusion, than that which is afforded by the comparison of the normal condition of the mind, with that in which the directing power of the will is in abeyance. This last condition is seen in certain states of somnambulism, both natural and arti-ficial, in the ' biologized' state, and some other abnormal conditions; the subjects of which may really be considered (so long as those conditions are allowed to last) as mere thinking automata, puppets pulled by directing-strings; their whole course of thought and of action being determined by suggestions conveyed from without, and their own Will having no power to modify or direct it, owing to the temporary suspension of its influence. Such individuals really are in the state which Mr. Atkinson affirms Man-kind in general to be; and we pity any one who is obtuse enough to be unable to discern the difference. To whatever extent, then, we may be

ready to admit the dependence of our mental operations upon the organization and functional activity of our nervous system, we cannot but feel that there is *something* beyond and above all this, to which, in the fully-developed and self-regulating mind, that activity is subordinated; and thus, putting aside, as speculative, all the difficulties that arise out of the essential differences in our conception of the respective properties of Mind and Matter, we find that this hypothesis is in such complete antagonism to our everyday experience, that we cannot entertain it for a moment; whilst, in rudely trampling on the noblest conceptions of our nature as mere delusions, it is so thoroughly repugnant to the almost intuitive convictions which we draw from the simplest application of our intelligence to our own moral sense, that those who have really experienced these, are made to *feel* its essential fallacies with a certainty that renders logical proof quite unnecessary.

Let us turn now to the opposite doctrine held by the *Spiritualists*, in regard to the nature and source of mental phenomena; and consider this in its Physiological relations.   To them the Mind appears in the light of a separate immaterial existence, mysteriously connected, indeed, with a bodily instrument, but not dependent upon this in any other way for the conditions of its operation, than as deriving its knowledge of external things through its agency, and as making use of it to execute its determinations, so far as these relate to material objects.   On this hypothesis, the operations of the Mind itself, having no relation whatever to those of Matter, are never themselves affected by conditions of the corporeal organism, whose irregularities or defects of activity only pervert or obscure the outward manifestations of the Mind, just as the light of the brightest lamp may be dimmed or distorted by passing through a bad medium; and, further, as the Mind is thus independent of its material tenement, and of the circumstances in which this may chance to be placed, but is endowed with a complete power of self-government, it is responsible for all its own actions, which must be judged of by certain fixed standards.   Now this doctrine fully recognises all that is ignored in the preceding; but, on the other hand, it ignores all that *it* recognised and served to account for; and is not less opposed to facts of most familiar experience.   For in placing the Mind *outside* of the body (so to speak), and in denying that the action of the Mind itself is ever disordered by corporeal conditions, it puts us in the dilemma of either rejecting the plainest evidence, or of admitting that, after all, we know nothing whatever about the Mind itself,—all that we *do* know being that lower part of our mental nature which operates on the body, and is in its turn affected through it.

Those who most fully and consistently carry out this doctrine, are ready to maintain that even in the state of Intoxication there is no truly mental perversion, and that, in spite of appearances, the *mind* of the Lunatic (*divinæ particula auræ*) is perfectly sound, its bodily instrument being alone disordered.   But we cannot ourselves hesitate for a moment in the conclusion, that in the delirious ravings of Intoxication or of Fever, or in the conversation and actions of the Lunatic, we have precisely the same evidence of *mental* operation, that we have in the sayings and doings of the same individuals in a state of sanity; and ample testimony to this effect is borne by those who have observed their own mental state during the

access of these conditions, and who have described the alteration which takes place in the course of their thoughts, when as yet neither the sensorial nor the motor apparatus was in the least perturbed.* Nothing, we think, can be more plain to the unprejudiced observer, than that the introduction of intoxicating agents into the circulating system really perverts the action of the *mind,* disordering the usual sequence of phenomena most purely psychical, and occasioning new and strange results which are altogether at variance with those of its normal action. And when once the reality of this influence of physical conditions upon purely mental states is forced upon the Physiologist, he can scarcely refrain from attributing to it a very wide range of action ; and thus he is led to the conviction, that however true it may be that there is something in our mental constitution beyond and above any agency which can be attributed to Matter, the operations of that agent are in great degree determined (in our present state of being) by the material conditions on which they are made dependent.

The whole theory and practice of Education, indeed, involves the distinct recognition of external influences, as having a most important share in the formation of the character. A being entirely governed by the lower passions and instincts, whose higher moral sense has been repressed from its earliest dawn by the degrading influence of the conditions in which he is placed, who has never learned to exercise any kind of self-restraint (or, if he has learned it, has only been trained to use it for the lowest purposes), who has never heard of a God, of Immortality, or of the worth of his Soul,—such a being, one of those heathen outcasts of whom all our great towns are unhappily but too productive, can surely be no more morally responsible for his actions, than the lunatic who has lost whatever self-control he once possessed, and whose moral sense has been altogether perverted by bodily disorder. But let the former be subjected to the training of one of those benevolent individuals who know how to find out " the holy spot in every child's heart ;" let patient kindness, continually appealing to the highest motives which the child *can* understand, progressively raise his moral standard, and awaken within him the dormant susceptibilities which enable him to feel that he has a conscience and a duty, that there is a Father who made him, and who watches over his welfare, that there is an hereafter of rewards and punishments, that he has a power within himself of controlling and directing his thoughts and actions ;—then, and not till then, in our belief, does he become truly responsible for his actions, either morally or religiously,—then only does he rise from the level of the brute, and begin to show that he is indeed made in the image of his Maker. All educational efforts, as it seems to us, *must be* based upon the assumption, that until the self-directing power has been acquired, the character is the resultant of original constitution, and of the circumstances in which the individual is placed ; and that so long as these circumstances are unfavourable to the development of the self-directing power, and to the repression of those higher tendencies which should furnish the motives to its exercise, so long the character of the individual *is* formed *for* him and not *by* him. The real *self-formation*

* See especially the work of M. Moreau, ' Du Hachisch et de l'Aliénation Mentale,' of which a critical analysis will be found in the British and Foreign Medical Review, vol xxiii. p. 217 ; and in the well-known ' Confessions of an English Opium-Eater.'

commences with his consciousness of the power of *self-control;* a power which is exercised by the Will, in virtue of its domination over what may be designated as the *automatic* operation of the mind.

Thus, then, we see that the Materialist and the Spiritualist doctrines alike recognise, and alike ignore, certain great truths of Human Nature; and the question returns upon us, whether any general expression *can* be framed, which may be in harmony alike with the results of scientific inquiry into the facts of the case, and with those simple teachings of our own consciousness, which must, after all, be recognised as affording the ultimate test of the truth of all Psychological doctrines. Such an expression may be framed, we think, in strict accordance with true philosophy, by withdrawing ourselves entirely from the futile attempt to bring Matter and Mind into the same category, and by fixing our attention exclusively on the relation between *Mind* and *Force.*

We not long since endeavoured to show* that the tendency of the Physical and Physiological Philosophy of the day is decidedly *dynamical;* that is to say, that the attention of scientific men is being fixed more and more upon those *forces* which are the *active agencies* whereby all change is produced, and less and less upon the *material substrata* through which these forces manifest themselves to us. Now of Mind, as of Force, our idea essentially consists in the succession of different states under which its manifestations present themselves to our consciousness. And further, our consciousness of Force is almost as direct as that of our own mental state, our notion of it being based upon our internal sense of the exertion which we determinately make to develope one form of Force, which may be taken as the type of all the rest; that, namely, which produces or which resists *motion,* and which must be regarded as the direct expression or manifestation of that Mental state which we call Will. Hence in the phenomenon of Voluntary movement, we can scarcely avoid seeing that Mind is *one* of the dynamical agencies which is capable of acting-on Matter; and in what we know of the physiological conditions under which Mind produces Motion, we have evidence that certain forms of Vital Force constitute the connecting link between the two; so that it is difficult to see that the dynamical agency which we term Will is more removed from Nerve-force, on the one hand, than Nerve-force is itself removed from Muscular force. Each, in giving origin to the next, is itself expended, or ceases to exist *as such;* and each bears, in its own intensity, a precise relation to that of its antecedent and its consequent.

But we have not only evidence of the excitement of Nerve-force by Mental agency; the converse is equally true, Mental activity being excited by Nerve-force. For this is the case in every act in which our Consciousness is excited through the instrumentality of the Sensorium, whether its condition be affected by impressions made upon organs of sense, or by changes in the state of the Cerebrum itself; a certain active condition of the nervous matter of the Sensorium being (we have every reason to believe) the immediate antecedent of *all* consciousness, whether sensational or ideational. And thus we are led to perceive that, as the power of the Will can develope Nervous activity, and as Nerve-force can develope Mental activity, there must be a *Correlation* between these two modes of

* British and Foreign Medico-Chirurgical Review, vol. viii. p. 206.

dynamical agency, which is not less intimate and complete than that which exists between Nerve-force on the one hand, and Electricity or Heat on the other.

The idea of the *Correlation of the Mental and Vital Forces*, thus based on the mutual action whereby alone the Mind can be brought into relation with the Universe around, or with its Corporeal instrument, will be found strictly applicable, we believe, to all those phenomena already adverted to, which indicate the influence of physical conditions in the determination of mental states; and thus, whilst differing essentially from the " materialist" hypothesis, it explains all which that doctrine seemed adequate to account for.   Conversely, it is in like manner applicable to all those phenomena which manifest the influence of bodily states upon the material changes which take place in the organism—a very curious group of facts, which will amply repay a careful and discriminating study. We shall, at present, only particularly notice those having reference to *Emotional* excitement, which seem to us to be of special interest. It is a fact, which is practically familiar to most observers of human nature, that any violent disturbance of the feelings most speedily subsides, when these unrestrainedly expend themselves (so to speak) in their natural expressions. Thus, it may be commonly noticed that those who are termed *demonstrative* persons are less firm and deep in their attachments, than those who manifest their feelings less; for, without any real insincerity or intentional fickleness, the strongly-excited feelings of the former are rapidly calmed down by the expenditure of the impulse to action which they have generated; whilst in the latter, the very same feelings acting internally, acquire a permanent place in the psychical nature, and habitually operate as motives to the conduct.   So, again, persons who are " quick-tempered," manifesting great irascibility upon small provocations, real or supposed, are usually soon appeased, and soon forget the affront; whilst those who make little or no display of anger, are very apt to brood-over and cherish their feelings of indignation, and may visit them upon the unfortunate object of them, when some favourable opportunity chances to occur, long after he had supposed that the occurrence which had given rise to them was forgotten. There is an instinctive restlessness, or tendency to general bodily movement, in some individuals, when they are suffering under emotional excitement, the indulgence of which appears to be a sort of safety-valve for the excess of nerve-force, whilst the attempt at its repression is attended with an increase in the excitement. Most persons are conscious of the difficulty of sitting still when they are labouring under violent agitation, and of the relief which is afforded by active exercise; and this is particularly the case when the movements are such as naturally express the passion that is excited. Thus, many irascible persons find great relief in a hearty explosion of oaths, others by a violent slamming of the door, and others in a prolonged fit of grumbling.*   It is well known, again, that

* This view is most fully confirmed by certain phenomena of Insanity. It is a doctrine now generally received among practical men, that paroxysms of violent emotional excitement are much more likely to subside, when they are allowed to " work themselves off" freely, without any attempt at mechanical restraint; and maniacal patients are now placed, in all well-managed Asylums, in padded-rooms, in which their movements can do no injury to themselves or others. The following case was related to us by Dr. Howe, of Boston, N.E., the instructor of Laura Bridgman. A half-idiotic youth in the Lunatic Asylum of that place was the subject (like many in his

the depressing emotions are often worked-off by a good fit of crying and sobbing; and the absence of any such external manifestations of these emotions gives them a much greater influence upon the course of thought, and upon the bodily state of the individual. Those who really " die of grief" are not those who are loud and vehement in their lamentations, for *their* sorrow is commonly transient, however vehement and sincere while it lasts; but they are those who have either designedly repressed any such manifestations, or who have experienced no tendency to their display; and their deep-seated sorrow seems to exert the same kind of *anti-vital* influence upon the organic functions, that is exercised more violently by " shock," producing their entire cessation without any structural lesion. The influence of concentrated attention, especially when accompanied with the *expectation* of a change, in modifying the Organic functions,—which is the real agent, as Dr. Holland has so well shown, in the cures worked by Mesmerism, religious enthusiasm, vulgar " charms," and the like,—is fully accounted for, and to our minds can *only* be accounted for, by the recognition of this direct dynamical relation between certain active states of mind, and the forces which are concerned in the nutrition of the bodily fabric.

But there is another great class of facts to which this explanation applies—those namely, which may be generalized under the designation of the " reflex," or " automatic" action of the Cerebrum.* All metaphysical inquirers have recognised the fact, that the succession of our ideas, when not interfered with by a voluntary effort, takes place according to certain *laws of thought*, which they have variously designated under the terms " association," " suggestion," and the like. And the Physiologist who looks at the Cerebrum as constituting the material substratum through which the transformation of nerve-force into mental agency is effected, cannot fail to recognise in this series of phenomena a respondence to external stimuli, which is the proper *modus operandi* of the Cerebrum, just as that simple affection of the consciousness by certain organic impressions, which we call sensation, is the *modus operandi* of the Sensory Ganglia. We think that there is ample evidence that the whole range of Mental phenomena originally called into activity by sensorial changes,— from the simplest act of Perception to the highest operations of Intellectual power,—consisting also in the play of Fancy and Imagination, and including those active states known as Passions, Emotions, Moral Feelings,

condition) of frequent and violent paroxysms of anger; and with the view of moderating these, it was suggested that he should be kept for some time every day in rather fatiguing exercise. Accordingly he was employed for two or three hours daily in sawing wood, to which task he made no objection; and the paroxysms of rage never displayed themselves, except on Sundays, when his employment was intermitted. It having been considered, however, that it was better for him to spend part of that day in sawing wood, than to be irascible during the whole of it, his occupation was continued through the whole week, when he became completely tamed-down, and never gave any more trouble by his passionate displays. This case appears to us to be a most valuable confirmation of the doctrine laid down above, whose practical bearings are most important.

* The application of the doctrine of " reflex action" to the Brain, was first fully developed by Dr. Laycock, of York, in a paper ' On the Reflex Function of the Brain,' read before the Medical Section of the British Association at its meeting in York, September, 1844, and afterwards published in the British and Foreign Medical Review, vol. xix.—Not having recognised what appears to us to be the essential distinction, both in their anatomical and physiological relations, between the Sensory Ganglia and the Cerebrum or Hemispheric Ganglia, Dr. Laycock did not mark out the distinction between the " *sensori*-motor" or " consensual" actions, which are the manifestations of the reflex power of the former, and the " *ideo*-motor" actions which depend upon the reflex action of the latter.

Sentiments, &c.—must be considered as the manifestation of the dyna-
mical activity of the Cerebrum; and that they *may* and *do* present them-
selves without any of that determinate self-direction, on the part of the
individual, which is requisite to give them the character of volitional
actions.   We referred in our last number (p. 229) to Coleridge as an
example of a man of the highest intellectual abilities, who had so little
power of self-direction as never to be able to make a worthy use of these.
The composition of the poetical fragment, ' Kubla Khan,' in his sleep, may
be taken as a typical example of automatic mental action; and his whole
life might almost be considered as a sort of waking dream, in so far as
regards his deficiency in that power of *utilizing* his abilities, which is the
characteristic of every really great mind.   And it would be easy to adduce,
from the life of Mozart, abundant illustrations of the automatic character
of the artistic creations of that great musical genius, who wrote some of
his finest works as they *spontaneously* evolved themselves in his mind,
without the least intentional elaboration, and who, like Coleridge, could
scarcely be kept to any determination, however much for his own interest,
except by completely submitting himself to the guidance of others.

Referring to Dr. Laycock's paper for the more detailed exposition of the
doctrine of the " Reflex action of the Brain," we shall here content our-
selves with remarking that, with certain general accordances, there is far
less of uniformity in the actions of the Cerebrum, than we observe in the
reflex actions of other parts of the Nervous system.   But this does not
constitute any valid objection to the doctrine; for it may be fairly attri-
buted, on the one hand, to differences in *original* constitution, of which we
have evidence at a very early period of life; whilst very much may be also
set down to differences in the *acquired* constitution of the organ, arising
out of the mode in which it has been habitually exercised, and analogous
to those which produce the secondarily-automatic actions of the inferior
portions of the nervous apparatus.   Now this acquired constitution is not
the mere resultant of the circumstances in which the individual may have
been placed; for it mainly depends upon the use which he has made of his
Will.   For when this power has been duly cultivated (see p. 229), it
acquires so complete a domination over the automatic actions of the
cerebrum, that it can regulate the course of the thoughts and the degree of
emotional excitement; intensifying some of these actions, and repressing
others, by determinate efforts directed with a special purpose.   Its power
is so far limited, however, that it can only *select* from the objects which
spontaneously present themselves to the consciousness, those which it
desires to retain and employ; and has no direct power of bringing before
the mind any object not actually present to it.   Hence it is, that, whilst
we have an almost unlimited power of turning to the best account the
endowments we possess, by strengthening our intellectual powers, expand-
ing our higher emotional tendencies, and bringing the lower under whole-
some restraint, we cannot, by any effort of the Will, introduce *new* elements
into our psychical nature.   No one, for example, has ever *acquired* the
creative power of genius, or *made himself* a great artist or a great poet,
or *gained by practice* that peculiar insight which characterizes the original
discoverer; for these gifts are mental instincts or intuitions, which may be
developed and strengthened by due cultivation, but which can never be

generated *de novo;* though it not unfrequently happens that the gift lies dormant, until some appropriate impression excites it to activity.

It will be obvious, that the view here taken does not in the least militate against the idea, that Mind may have an existence altogether independent of the material substratum through which it operates; since all that we contend for is, that the connexion between Mental Agency and Nervous Force is such, as to give them that kind of determinate action on one another, which we have seen to exist between the Nervous Force and the various agencies to which, as formerly shown (vol. i. p. 233), it is so closely related. And although, in so far as the automatic action of Mind is alone concerned, it might be difficult to show that there is any positive evidence of its essential independence of the material substratum through which it is here manifested, yet we do seem to have such evidence in the wonderful power which the Will has, of placing itself in opposition to the automatic tendencies, and of subjugating them to its domination,—elevating the individual, in proportion as he has acquired such self-command, to the condition of a *free agent.* And, truly, in the existence of such a Power, which is capable of dominating over the very highest of those operations that we find reason to connect with corporeal states, we find a better evidence than we seem to gain from the study of any other part of our psychical nature, that *there is* an entity, wherein Man's nobility essentially consists, which does not depend for its existence on any play of physical or vital forces, but makes these subservient to its determinations.

This sketch of our views will be scarcely complete, unless we extend their application from the constitution of our own minds, and their relation to our bodily organism, to the notion which we form of the Divine Mind, and of the relation of the Deity to that universe which is one of the modes wherein he reveals to us His presence. And we advert to this subject, not merely because we wish to show that the doctrine we have propounded is strictly conformable to the highest teachings of religion, but because we believe that it affords the solution to difficulties which have perplexed many deep-thinking men, and which have especially tended to keep science and religion apart from one another, rendering the physical philosopher either an avowed sceptic or a mere speculative religionist, and inspiring the religionist with a bigoted horror of science.

The conception formed by each individual of the Divine Nature, depends in great degree upon his own habits of thought; but there are two extremes, towards one or other of which most of the current notions on this subject may be said to tend, and between which they seem to have oscillated in all periods of the history of Monotheism;—these are, Pantheism and Anthropomorphism.

Towards the Pantheistic aspect of Deity, we are especially led by the philosophic contemplation of His agency in external Nature; for in proportion as we fix our attention exclusively upon the "laws" which express the orderly sequence of its phenomena, and upon the "forces" whose agency we recognise as their immediate causes, do we come to think of the Divine Being as the mere *First Principle* of the Universe, an all-comprehensive "Law," to which all other laws are subordinate; that most general "Cause," of which all the physical forces are but manifestations. This conception embodies a great truth, and a fundamental error. Its

truth is, the recognition of the universal and all-controlling agency of the
Deity, and of His presence *in* Creation, rather than on the outside of it.
Its error lies in the absence of any distinct recognition of that *conscious
volitional* agency, which is the essential attribute of Personality; for with-
out this, the universe is nothing else than a great self-acting machine; its
laws are but the expressions of "surd necessity;" and all the higher
tendencies and aspirations of the Human Soul are but a "mockery, a delu-
sion, and a snare."

The *Anthropomorphic* conception of Deity, on the other hand, arises
from the too exclusive contemplation of *our own* nature as the type of the
Divine; and although, in the highest form in which it may be held, it
represents the Deity as a being in whom all the noblest attributes of man's
spiritual essence are expanded to infinity, yet it is practically limited and
degraded by the impossibility of *fully* realizing such an existence to our
minds; the failings and imperfections incident to our human nature being
attributed to the Divine, in proportion as the low standard of intellectual
and moral development in each individual keeps down his idea of possible
excellence.   Even the lowest form of any such conception, however, em-
bodies (like the Pantheistic) a great truth, though mingled with a large
amount of error.   It represents the Deity as a *person;* that is, as having
that intelligent volition which we recognise in ourselves as the source of
the power we determinately exert, through our bodily organism, upon the
world around; and it invests him, also, with those moral attributes which
place him in sympathetic relation with his sentient creatures.   But this
conception is erroneous, in so far as it represents the Divine Nature as
restrained in its operations by any of these limitations which are inherent
in the very constitution of Man; and in particular, because it leads those
who accept it to think of the Creator as "a remote and retired mechanician,
inspecting from without the engine of creation to see how it performs,"
and as either leaving it entirely to itself when once it has been brought
into full activity, or as only interfering at intervals, to change the mode
of its operation.

Now, the truths which these views separately contain, are in perfect
harmony with each other; and the very act of bringing them into combina-
tion effects the elimination of the errors with which they were previously
associated.   For the idea of the universal and all-controlling agency of the
Deity, and of his immediate presence throughout creation, is not found to
be in the least degree inconsistent with the idea of his personality, when
that idea is detached from the limitations which cling to it in the minds
of those who have not expanded their anthropomorphic conception by the
scientific contemplation of nature.   On the contrary, when we have once
arrived at that conception of *Force* as an expression of *Will*, which we
derive from our own experience of its production, the universal and con-
stantly-sustaining agency of the Deity is recognised in every phenomenon
of the external universe; and we are thus led to feel that in the Material
Creation itself, we have the same distinct evidence of His personal existence
and ceaseless activity, as we have of the agency of intelligent minds in the
artistic creations of genius, or in the elaborate contrivances of mechanical
skill, or in those written records of thought which arouse our own psychical
nature into kindred activity.

PART SECOND.

Bibliographical Notices.

Art. I.—*Insanity; its Causes, Prevention, and Cure; including Apoplexy,
   Epilepsy, and Congestion of the Brain.* By Joseph Williams, M.D.
   Second Edition. 8vo.   1852.

Dr. Williams wrote, in 1848, a prize essay, on 'The Use of Narcotics
and other remedial agents calculated to produce Sleep in the Treat-
ment of Insanity.' He has recently enlarged this essay into a goodly
volume, containing upwards of three hundred pages; the original grain of
wheat contained therein (if it did contain a grain) being increased by nearly
a bushel of chaff. To call this book a second edition of the prize essay, is
simply an absurdity; it is what its name denotes, and not a treatise on the
use of hypnotics in mental diseases.—The *spirit* in which this book is
written may be gathered from the following:

   "Disease of the mind is complicated, and the persons who have specially under-
taken to cure that disease have, at present, individually done very little in the way
of suggesting either therapeutical, moral, or general means for alleviating or curing
such an afflictive disorder, and this, too, with ample means of investigation before
them; the desire has always been to keep the system, or plan of treatment, ' close.'
Even to this day their practice is often secret—empirical." (p. 117.)

   This comprehensive slander must surely have been penned in utter
ignorance of the psychiatric literature of the nineteenth century; yet the
ignorance thus implied is almost as incredible as the hypothesis of wilful
falsehood.   A writer who assumes to be so sweeping a judge, and so
luminous an author, must at least have heard of such " persons" as Pinel,
Esquirol, Haslam, Heinroth, Willis, Guislain, Seymour, Prichard, Burrowes,
Thurnam, Jacobi, Engelken, Leuret, Conolly, Morison, not to mention
many other names less familiar to English literature? If so, then are
these the " persons" referred to, who, with ample means of investigation,
have done so very little in " the way of suggesting" moral or therapeutical
treatment of the insane ?
   Such a statement indicates very lax habits of writing and thinking, and
throws a doubt upon every page of the book.   Nor is this doubt uncorro-
borated by facts.   Thus the modern treatment with morphia is likened to
the ancient treatment with hellebore—the latter a drastic purgative, and
used as such by the Greeks and Arabs.   Again, Dr. Williams has caught
at the modern doctrine which traces a close connexion between cerebral
disease and imperfect renal action, especially as manifested in albuminuria.
He evidently has not the most remote comprehension of the theory; but
he must needs take some notice of it; and he does it thus:

"It is here worthy of inquiry, how does the albumen act upon the brain? Does it, under such circumstances, exist more largely in the blood? and if so, is it its tenacity, or gummy or glutinous characteristic, which causes obstruction and congestion, and subsequently inflammation? We all know the effect of injecting gum into the arteries and veins; it causes inflammation, and in this way pneumonia may be artificially induced." (p. 209.)

After this, scientific criticism is unnecessary.

Art. II.—1. *The Natural History of the Varieties of Man.* By Robert
    Gordon Latham, M.D., F.R.S., &c. &c. — *London*, 1850.   8vo,
    pp. 574.

2. *Man and His Migrations.* By R. G. Latham, M.D., F.R.S., &c.—
    London, 1851. Fcap. 8vo, pp. 250.

3. *The Ethnology of the British Colonies and Dependencies.* By R. G.
    Latham, M.D., F.R.S., &c.—*London*, 1851. Fcap. 8vo, pp. 264.

4. *The Ethnology of Europe.* By R. G. Latham, M.D., F.R.S., &c.—
    London, 1852. Fcap. 8vo, pp. 256.

5. *The Ethnology of the British Islands.* By R. G. Latham, M.D.,
    F.R.S , &c.—*London*, 1852. Fcap. 8vo, pp. 260.

We owe an apology to Dr. Latham for not having earlier noticed the first of the works on the above list, which well deserves the respect of a careful and discriminating review ; and we regret on many accounts, that circumstances have prevented us from fulfilling our intention of resuming those Ethnological discussions, on which the admirable works of Dr. Prichard had formerly led us to enter. Among British Ethnologists, there are none who have advanced higher claims than Dr. Latham to be regarded as the successor of that distinguished man ; but his claims are of a very different kind. Of Dr. Prichard it was well said, that he was a physiologist among physiologists, and a scholar among scholars ; so eminent was he in both these departments of Ethnological inquiry. Dr. Latham has not given us much ground for judging of his physiological attainments ; and we miss in his writings that comprehensive grasp of the whole series of questions involved in this intricate study, which was the distinctive feature of Dr. Prichard's treatment of it. But his *forte* evidently lies in philological and historical analysis ; he is a zealous collector of linguistic materials, and shows great acuteness in the use which he makes of them. As we do not pretend, however, to any profound philological lore, we shall not venture to pass judgment upon the many novel views which our author has based upon the results of his researches ; but must leave it to others more competent than we can pretend to be, to decide upon their value. We shall note, however, a few of the points in Dr. Latham's general treatise, which strike us as being of most interest.

Although he does not enter into any systematic discussion of the question, it is obvious that he is a firm upholder of the doctrine of the *specific unity* of the Human Races ; and this in the sense not only of conformity to a common type, but of descent from the same parentage. He considers them all as capable of being grouped under three primary varieties— namely, the *Mongolidæ*, the *Atlantidæ*, and the *Japetidæ ;* and there are

several points of great interest in his distribution of these. His term *Mongolidæ* is far more comprehensive than the Mongolian Race of Blumenbach; for it includes the Malayan and American with nearly the whole of the true Asiatic races, excluding the Semitic or those of the Syro-Arabian group. That the American and Malay, or Polynesian races, were originally Mongolian, had been surmised by many previous Ethnologists; and Dr. Latham adds no additional evidence of importance to that which had been previously adduced. It had been generally considered, however, that the principal part of the population of India and Persia belongs to the Japetic, or Indo-Germanic race, of which the great bulk of the European nations whose languages have a Sanskritic basis, were supposed to be an offset. Dr. Prichard, we know, had obviously leaned in his later years to the opinion, that a considerable part of the population of the Indian Peninsula, belonging to what are termed the " hill-tribes," are not of the same stock with the true Hindoos, but were aborigines who had previously occupied the country, having migrated thither from the Mongolian area previously to the occupation of the country by the Indo-Germanic race; and the inquiries of General Briggs regarding these mysterious tribes, and of Mr. Hodgson on the inhabitants of the sub-Himalayan countries, tended to confirm and extend this view. But it is now asserted by Dr. Latham, that the same is true of the great bulk of the proper Hindoo population, who have been usually considered to be the lineal descendants of the people whose original language was the Sanskrit; and not only of them, but of their Persian neighbours, whose ancestors were supposed to have spoken the Zend, a dialect kindred to Sanskrit; so that, as he himself says (p. 546), " the nation that is at one and the same time Asiatic and Indo-Germanic, remains to be discovered," there being, in his opinion, few or no descendants, in Asia, of the people whose vernacular was the Sanskrit group of languages. He does not fully enter upon the discussion, however, in any of the treatises before us; and leaves us very much in the dark as to the evidence on which he ventures to base a position so strongly opposed to the opinions of almost all his predecessors and contemporaries. —It is curious to find the Georgians and Circassians, who have been considered as the typical members of the *Caucasian* group, ever since Blumenbach figured the skull of a Georgian female as the most perfect in his collection, now ranked as members of the Mongolian stock; yet such there is little doubt, from the peculiarities of their language, that they are rightly to be considered. To this conclusion, also, Dr. Prichard had approximated, though the linguistic evidence in his possession was not sufficient to justify him in positively affirming it.

Under the general term *Atlantidæ*, our author associates the African nations with the Syro-Arabian, or Semitic group; considering that the time is now come, in which an affinity, long suspected, may be considered as firmly established. The gradual transition from the one to the other is marked, both as regards language and physical characters, in the inhabitants of the Nile Valley; but even among the negroes of the Great Desert and of the Guinea Coast, there are numerous traces of community of origin with the Semitic races, both in the roots and structure of their languages, and in peculiar manners and customs. To this conclusion, also, we know that Dr. Prichard had been for several years approximating, it

having been expressed by him conversationally to ourselves, in a manner far more confident than that in which he would have felt justified, without further evidence, in placing before the public; and we took an opportunity of bringing it before our readers previously to the publication of Dr. Latham's treatise. (See vol. v. p. 99.)

With respect to the *Japetidæ,* or Indo-Germanic nations, whose present distribution is considered by Dr. Latham to be limited to Europe, we need only remark, that Dr. Latham limits the (supposed) original Celtic (or, as he spells it, Keltic) area, and extends the Slavonian, in a way which is opposed to the conclusions of most of those who have gone before him.

The foregoing *classification* Dr. Latham states to be the *chief* end of his work; and to the establishment of its correctness, the principal part of his pages is devoted. Hence this treatise can only be fully understood by those who are sufficiently versed in Ethnological science, to distinguish what is new in Dr. Latham's scheme, and to appreciate the evidence he adduces in its support. For ourselves, we must honestly say that we have found it most terribly hard reading, and that we cannot conceive how any one who may come fresh to the subject, can derive any general idea of it from this treatise, so fragmentary is its composition, so unequally is the space devoted to the several topics proportioned to their respective importance. The result of an elaborate investigation is sometimes compressed into a line or two, without the least idea being afforded of the data whereon the decision is based; whilst in other instances, many pages are bestowed upon the elucidation of a comparatively trivial matter, which would have been much more appropriately considered elsewhere. Whilst, then, we strongly urge the perusal of Dr. Latham's work upon all such as desire to master the present bearings of the subject, and to acquire an idea of the new and important materials which have been brought to light since the publication of Dr. Prichard's Treatises,—we cannot but consider that the latter still afford to the student the best introduction to Ethnology, and the fairest example of the right mode of pursuing it.

The subjects of Dr. Latham's four smaller treatises will be sufficiently indicated by their titles; and of the mode in which these subjects are treated, it gives us pleasure to be able to state, that they present a great improvement upon the larger volume; this being especially the case with the two later members of the series. To those who know Dr. Latham's attainments, we need not say that they are richly stored with various learning; and we can especially recommend the two volumes on the British Islands and on the British Colonies, as supplying information of the greatest interest to every one who has any curiosity about his own ancestry, or about the various tribes to which, in various ways, he stands in relation.

ART. III.—*Medical Jurisprudence.* By ALFRED S. TAYLOR, M.D., F.R.S., Lecturer on Medical Jurisprudence and Chemistry in Guy's Hospital, &c. &c. Fourth Edition.—*London,* 1852. Fcap. 8vo, pp. 826.

So well is this work known to the members both of the medical and legal professions, and so highly is it appreciated by them, that it cannot be necessary for us to say a word in its commendation;—its having already reached a fourth edition being the best possible testimony in its favour.

The author has obviously subjected the entire work to a very careful revision. We find scattered through it numerous additions and alterations, some of them of considerable importance; and reference is made to a large number of cases which have occurred since the date of the last publication. To several of these changes, the attention of the reader is directed in the preface; but it would answer no important purpose for us to transcribe the enumeration.— One rather singular addition we must notice, however ; namely, a Glossary, containing an explanation of medical terms, for the use of " those members of the bar who make the book a kind of circuit companion." We cannot think that the brief explanations here given are likely to be of any great service. In fact, we fear that, in many instances, they will lead to misapprehensions, of which Dr. Taylor will be quoted as an authority; "Colic," for example, being defined to be "a disease attended with severe pain, chiefly referred to the colon." We could mention a number of terms of the first importance in the description of post-mortem appearances, the results of injuries, &c., of which no mention is made in this glossary; *e. g.* 'inflammation,' 'ulceration,' 'suppuration,' 'gangrene,' 'sloughing,' &c. Surely it is of far more consequence that a lawyer should be able to attach definite meanings to these terms, than to 'crypsorchides' or 'monorchides,' 'carcinoma' or 'schirrus.' Nothing is more difficult than to give any clear idea to the uninitiated, of the essential nature of pathological changes; and we fancy that Dr. Taylor may have given up the attempt in despair. But still this objection applies to the whole *idea* of his glossary ; and we think it would have been better if he had not encouraged his legal friends in the belief that, on the strength of such a contrivance, they are qualified to discuss questions of a purely professional nature with medical witnesses.

We are glad to see that Dr. Taylor contemplates the speedy publication of a larger treatise on Medical Jurisprudence, which will embrace some subjects of great interest that have been hitherto excluded by want of room, and many facts and cases that are only briefly referred to in the present work. Among these new subjects are, the various *modes of death*, *sudden death*, the changes which the *dead body* undergoes, and the examination of the body, or its fragmentary remains; topics whose omission in the manual before us we have always greatly regretted.

Art. IV.—*The Prescriber's Complete Handbook.* By M. Trousseau, Professor of the Faculty of Medicine, Paris, and M. Reveil. Edited, with Notes, by J. Birkbeck Nevins, M.D.—*London*, 1852. 12mo, pp. 499.

The name given to this little book would naturally lead us to expect to find it a work suited for the medical practitioner, when in doubt as to the strength, dose, &c., of any preparation he might be desirous of prescribing, containing also a short account of the nature of the different drugs which are more commonly employed, and of their officinal preparations. If, however, any British practitioner purchase the present book with such expectation, he will be much disappointed; for, as will be seen by the title-page, it is only a translation of a French work, compiled by M. Trousseau (joint author with M. Pidoux, of the *Traité de Thérapeutique et de Matière Médicale*) and M. Reveil; translated into English by Dr. J. Birkbeck

Nevins. We have no fault to find either with the work or with the translation; we only much question its value to the British practitioner or student of medicine, and consider that many other continental works, on the same subject, might have been selected for translation with greater advantage.

The contents of the Handbook are as follow:

1. A classification of the substances used in medicine, derived from the vegetable, animal, and mineral kingdoms; giving, in a table, the French and English common names, the Latin names, parts employed, forms of administration, doses, and therapeutic actions, with short observations on the nature and composition of the drugs, &c.

2. A section on the art of prescribing; containing a brief account of the mode of action of medicines, methods of applying them, circumstances influencing their action, and a table of incompatibles.

3. A short sketch of Pharmacy.

4. A chapter on Magistral Prescriptions.

5. Outlines of Toxicology.

There is no doubt, but that many very useful facts are to be found under the various divisions above enumerated; but most of any value are contained in almost all our standard works on Materia Medica and Toxicology; and, as we have before said, we think that the book is more adapted to the French than the English student, and we question if it is likely to gain much popularity in this country. Those who are most likely to benefit by it, are such as are able to draw suggestions from the modes of practice of our Gallic neighbours, for the improvement or extension of their own.

ART. V.—*Life of Dr. John Reid, late Chandos Professor of Anatomy and Medicine in the University of St. Andrews.* By GEORGE WILSON, M.D., author of the ' Life and Works of the Hon. Henry Cavendish.'—*Edinburgh,* 1852. Foolscap 8vo, pp. 316.

THAT Dr. John Reid's life was one which well deserved to be written, as amply fraught with most important lessons, we need scarcely tell those who have read our review of his scientific labours (vol. iii. p. 318), and the brief obituary notice (vol. iv. p. 577) which too soon followed it. And although we could have wished that the task had been performed by some friend who knew him in the most active period of his life, as well as in those later days when death had already set its seal upon his brow, yet it would not have been easy to find one who could have more truly appreciated his worth than Dr. G. Wilson has done, or who could have more faithfully delineated the great change which took place in his religious state, after he was first led seriously to reflect upon the probably fatal issue of his disease. This change appears to have specially dated from the solitary wanderings among the mountains of Cumberland, alluded to in the letter of which we formerly gave an extract. At the commencement of the period which he spent there, we find him writing of his gloomy prospects with that mere stoical courage, which his vigorous nature was well fitted to manifest. " I often fear the worst; but if it should turn out so, I must submit quietly to my fate, as many a better man has done before me."

But soon he seems to have felt how insufficient was this to sustain him through the period of trial and agony which he was compelled to anticipate, and how poor was the prospect for him if he could not direct his thoughts with hope to a future world; and no sooner did he begin to look upon time, and its pleasures and rewards, as valueless in comparison with eternity, than he gave up his whole soul to the contemplation of his great change, and never afterwards withdrew his attention from it, even when the favourable result of the first operation held out the prospect of prolonged life and restored health.   All the details which Dr. G. Wilson has given of the state of his mind between this period and the final close, partly derived from his own letters, and partly from the statements of those who were most constantly with him, are full of interest; and not least do we prize those which show how strong his sympathies still were with all whom he had previously loved, and how much of his interest still remained in those pursuits which had formed the great objects of his life.

The fault of biographies of this kind, executed by those who have *only* known the later phase of the lives of the subjects of them, is usually, that they make *too great* a difference between the state of mind *after* what is commonly termed "conversion," and that which has been *previously* the habitual condition of the individuals; the latter being unduly depressed, in order to give the desired exaltation to the former. From this fault we do not think that the present memoir is altogether free; and we speak not merely from our own knowledge, but from that of other old and intimate friends of Dr. Reid's, when we say that the essentials of his character underwent no change, but that parts of his nature which had been previously dormant were called into activity by the strong excitement of circumstances, and a new direction was thus given to his thoughts.   The John Reid of October, 1848, was to his friends in London the John Reid of years gone by, with a mind chastened and heaven-directed by the trial through which he had passed, and by the consciousness of that which might yet be in store for him, but with as thorough an enjoyment of social intercourse, and as keen a relish for scientific inquiry, as he had ever displayed.

There are one or two blemishes in this memoir, which we would gladly see removed.   Dr. Wilson quotes (p. 237) a reference which Dr. Reid more than once made to the fact, that the seat of his sufferings was in the same nerves on which he had made so many experiments, as if they were a *judgment* on him for the sufferings which he had inflicted on animals: he qualifies this expression, however, by saying that Dr. Reid adverted to it, not as an avenging retributive judgment, but as a kind and merciful one.   Now, the term *judgment*, which does not seem to have been actually employed by Dr. Reid, has a certain theological meaning, which (in such a connexion as this) means *punishment*.   And we feel called upon to protest most strongly against the doctrine, that well-devised and carefully-conducted experiments, such as Dr. Reid's, can be regarded as sinful by Him who gave man the capacity to investigate the truths of Physiology, and to apply them to the alleviation of human suffering.   Dr. G. Wilson's views on this point seem to coincide completely with our own; and we therefore regret that he should have admitted into his pages anything that could

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give an erroneous idea of Dr. Reid's later feelings on this subject, which remained, we feel sure, completely unchanged.—We doubt, moreover, as to the expediency of attempting to popularize the results of Dr. Reid's researches, which were of a kind whose merits can scarcely be made intelligible to the uninitiated. With these slight exceptions, however, we can most heartily commend the style and spirit of this biography, and can strongly urge the perusal of it upon our readers.

ART. VI.—*An Outline of Human Physiology, for the Use of the Chinese.* By BENJAMIN HOBSON, M.D. Lond., M.R.C.S., Agent of the London Missionary Society.—*Canton*, 1851.

THIS is certainly the most curious book that has ever been forwarded us for review. It is printed entirely in Chinese characters, from wooden blocks, on a sort of India paper. But under this strange dress we meet with a great many old friends, in the shape of well-known anatomical figures, with which the work is copiously interspersed. Not being ourselves versed in the Chinese language, we must take Dr. Hobson's own account of the book, which he states to be "an humble attempt to put the interesting and well-established truths of Human Physiology into Chinese," so as "to form a popular and useful compendium for Chinese physicians and scholars, who have often expressed an interest in this subject." This has been accomplished, with the aid of an intelligent native, and an anatomical model from Paris, upon which he was first instructed, and then caused to clothe his descriptions "in good and idiomatic Chinese." But, as we could easily anticipate, "there has been much difficulty experienced in fixing upon new terms, and finding suitable words for unnamed or improperly described parts of the human body." Having, on a former occasion, expressed our cordial concurrence in the objects of the "Medical Missions," with which Dr. Hobson is in connexion, we have now only to express our continued interest in his undertaking, and our hope that it may meet with the success he desires.

ART. VII.—*The Spirometer, the Stethoscope, and Scale-Balance; their Use in Discriminating Diseases of the Chest, and their Value in Life-Offices; with Remarks on the Selection of Lives for Life Assurance Companies.* By JOHN HUTCHINSON, M.D., Assistant Physician to the Hospital for Consumption, &c.—*London*, 1852. 8vo, pp. 79.

THE valuable results obtained by Dr. Hutchinson's method of measuring the volume of air that can be expired from the lungs, and comparing this with the height and weight of the body, are now so well known to the profession, that we need do no more than state that in this *brochure* they are put forth in a form well calculated for general application, with full directions respecting the construction and use of the Spirometer. Certainly no medical referee to an insurance office ought to be without one of these instruments; which, although by no means infallible in its indications, will often afford most valuable information.

ART. VIII.—*Disease in Childhood, its Common Causes, and Directions for its Practical Management.* By ROBERT ELLIS, F.L.S.—*London*, 1852. Fcap. 8vo, pp. 288.

THIS little work is designed, to use the author's own words, "to direct attention to the existence of a vast amount of mortality among children and the young, to develop some of its more common causes, and to afford such instructions as may serve to its diminution by leading to a right system of their management in health and disease." It is obvious, therefore, that its scope is very much the same as that of Dr. A. Combe's admirable little work on the 'Physiological and Moral Management of Infancy;' the chief difference being, that Mr. Ellis addresses himself more particularly to the causes of the high rate of infant mortality, which the registration returns continue to disclose, and aims to show how large a part of this mortality is preventible. The book is obviously the production of a thoughtful and intelligent man, who has a strong desire to promote the welfare of his fellow-beings; it is soundly and clearly expressed; and though there are a few passages which we cannot but consider irrelevant, and a few more as to which our opinions are not in accordance with those of the author, yet on the whole we can strongly recommend it as well adapted to the object he had in view in its composition.

ART. IX. — *The Physician's Holiday; or, A Month in Switzerland in the Summer of* 1848. By JOHN FORBES, M.D., F.R.S., Physician to Her Majesty's Household. With a Map and Illustrations. Third Edition.—*London*, 1852. Post 8vo, pp. 350.

IT gives us much pleasure to see how beneficial Dr. Forbes's 'Holiday' has been, not merely to himself, but to the public at large. Nothing can be more dissimilar than the genial record of his personal experiences and the dry didactic tone of the ordinary traveller's 'Handbook;' and the continued demand which has occasioned the call for this new (and cheaper) edition, is the best evidence of the truth of all that we said in its commendation when it first came under our notice.

ART. X.—*The Symptoms and Treatment of the Diseases of Pregnancy.* By WILLIAM JOHN ANDERSON, F.R.C.S., District Accoucheur to St. Mary's Hospital.—*London*, 1852. 12mo, pp. 119.

THERE are doubtless many disorders to which the pregnant female is especially liable, some of them altogether limited to her peculiar state, which need to be systematically considered; as a typical example of these, we might specify the obstinate vomiting which sometimes proves not only most distressing, but even most injurious, or even fatal. But there are many others, such as hæmorrhage, the consideration of which is so intimately linked-on with the subject of parturition in general, that we cannot see how any practical benefit can arise from thus isolating them. Mr. Anderson's treatise, attempting to include everything, does justice to

nothing. Instead of having the completeness which might be anticipated in a monograph, it has all the meagreness of a chapter in a systematic treatise in which brevity is the prime object; and we have scarcely met with a single point on which it tells the well-informed practitioner anything that he does not already know. The list of remedies recommended for obstinate vomiting shows a singular infertility in resources; and the question of the induction of premature labour as a last resource, which is one requiring a singular degree of discrimination and foresight (see p. 550 of our present number), is dismissed in half-a-dozen lines.

We cannot compliment Mr. Anderson, then, either on his choice of a subject, or on his treatment of it; and should strongly recommend him to find something better worth telling the profession, before he again makes his appearance as an author.

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ART. XI. — *Report of the Commissioners of Health, Ireland, on the Epidemics of* 1846 *to* 1850.—*Dublin*, 1852. (Parliamentary Paper.)

THE general facts contained in this Report must be sufficiently familiar to our readers, to render it unnecessary for us to dwell on them; but there is one statement which is so remarkable that we cannot refrain from citing it:—it is that of the relation between scarcity of food and the prevalence of epidemic fever. Every one knows that whenever there is a famine, pestilence might be expected to succeed it; but so close a correspondence as the following could scarcely have been anticipated. It must be borne in mind that the greatest prevalence of disease must be expected *after* the continuance of "famine-prices" for some time, and not when prices are actually at the highest.

	Average price of Potatoes per cwt.	Number of Patients admitted into temporary Fever Hospitals.
1845	2s. 2d.	
1846	4 10	
1847	8 4	95,890
1848	7 0	110,381
1849	6 11	87,135
1850	4 4	39,056

The reduction of mortality in 1850 is greater than could have been expected from the continuance of the price of potatoes at twice the average of 1845; but this seems accounted for by the cheapness of oatmeal during the autumn of 1849, which was such as to induce many who had previously fared on potatoes only to employ it in preference, finding that at twice the price it gave four times the nutriment, weight for weight.—We trust that the knowledge thus acquired will lead to the increased substitution of cereals for the potato, as the staple article of Irish diet.

PART THIRD.

Periscope.

ANATOMY, PHYSIOLOGY, AND ORGANIC CHEMISTRY.

On the Secondary Degeneration of Particular Fasciculi of the Spinal Cord, and of their Continuation to the Brain. By Dr. Ludwig Turck.

The author states that he has found a peculiar pathological alteration in the Spinal Cord subsequent to disease of the Brain, which consists of a deposit of numerous granular cells upon the side of the spinal marrow opposite to that portion of the brain in which the disease has originated. These observations were first communicated by him to the "Society of Physicians of Vienna," in the year 1850.

It since occurred to him that this fact might be constant in regard to particular fasciculi of the spinal marrow, in which there might be an alteration of tissue, without involving the entire structure of the cord. Induced by such conclusions, Dr. Turck commenced a series of careful examinations, by which he became convinced that particular fasciculi were always secondarily diseased, depending upon that point of the brain or spinal marrow which was the focus of morbid action; such fasciculi only becoming altered, as were in direct connexion with these diseased points; and that thus information was attainable relative to the origin and office of such fasciculi.

The manner of procedure was as follows: After the spinal nerves had been exactly counted, to establish the precise portion to be examined, the cord was horizontally divided by a pair of sharp scissors, and the stump obtained in this way turned upwards, and its exact contour, as well as that of the grey matter, delineated with precision. Next, from a very small portion of the horizontal surface, a thin layer of medullary matter is removed by a pair of delicate scissors curved at the point; this portion of the surface selected for examination being marked on the magnified drawing, previously made, of the entire section. The fraction removed is then subjected to the most accurate microscopic scrutiny, and the result written down under a letter of the alphabet, corresponding with a letter assumed to distinguish the same relative point on the previously delineated plan: this was continued until the entire surface had been thoroughly examined. Dr. Turck contends that the formation of granular cells is not a propagation of the abnormal condition from the point of disease, but the consequence of the loss of function in fibres which no longer receive those impulses requisite to continue them in the discharge of their duties; and as, in other structures, the loss of function involves a degeneration of tissue, the fibres of nerves, no longer conveying the currents of nervous excitation, degenerate in obedience to a universal law. In evidence that these granular cells are not propagated from a diseased point, Dr. Turck has never observed them earlier than six months after the occurrence of disease in the nervous centres, and then not in a decreasing ratio from such a point; but he has known the spinal marrow remain in apparently perfect health, while the walls of an effusion were thickly covered by these abnormal products; and upon their subsequent appearance he has seen them select certain portions of the spinal cord, as, for instance, just above the origin of the plexus of the lower extremities, to appear in much greater intensity than nearer the seat of disease.

Paraplegia offers a good example of the course of this degeneration of structure, the granular cells being found, in such cases, in large numbers at the seat of injury, from whence they diminish rapidly, till, within a short distance, they are entirely confined to the exact outline of individual fasciculi, in which they mount upwards as far as the pons Varolii. These were in no instance the same fasciculi in which a deposit was found secondary to disease of the brain ; the latter fasciculi remaining in paraplegia entirely free, from the circumstance (deduced from these observations) that these fasciculi, having a different office to fulfil, only sympathized with disease at points from whence they derived their functional power. Upon the occurrence of effusion into the spinal column, for instance, a deposit of granular cells must, after a time, ensue, in consequence of the abrogation of function of the various fasciculi composing the cord, either above or below the point of disease regulated by the employment of these fasciculi ; those devoted to the conveyance of centripetal currents degenerating above the point of effusion, and those used for centrifugal purposes below,—the fact of these fasciculi being centripetal or centrifugal, being derived by *à posteriori* reasoning from the observance of constant deposits within their walls.

With reference to a very natural conclusion that these granular cells would be produced in an exuded fluid, observation teaches that the smaller capillary vessels of a part, those that would naturally take important action in the production of such a pabulum, are comparatively free from the presence of these abnormal products ; while the larger vessels have granular cells congregated around their external walls, as a thread is converted to a similar purpose when suspended in a saline solution, and becomes a fixed point for crystalline deposit. Another argument against these results being the product of exudation, is the fact of the exact limits to which they are restricted in being confined exclusively to individual fasciculi ; and as Dr. Turck has never observed the slightest vestige of inflammation, it would seem that these abnormal cells were rather the consequence of anomalous nutrition, or a product of decomposition in structures no longer required to discharge a previous function. Dr. Turck states that he has seen cases in which the distribution of cells exceeded the limits that a strict accordance with the above conclusions would have prescribed for their occurrence ; for such a condition he confesses himself at present unable to account.

The granular cells increase in size in proportion to the long continuance of disease, and are often a precursor of atrophy—a result which Rokitansky mentions as occurring in certain portions of the nervous system secondary to local affections, and on one occasion he witnessed the loss of an entire hemisphere. These cells are soluble in ether, and in a high grade of disease exhibit the presence of free globules of fat.

The following is Dr. Turck's *résumé* of the deductions which he thinks himself entitled to draw from his observations :

1st. When, owing to protracted disease in the brain or spinal column, the nervous currents, through certain fasciculi, remain for a length of time interrupted, these fasciculi, in consequence of the abrogation of their office, have produced within them numbers of granular cells ; these represent the beginning of a more complete metamorphosis, to be perfected after a further lapse of time.

2nd. When, in such cases, transverse sections are made through the spinal marrow, medulla oblongata, pons Varolii, and the brain and its ganglia, and those points which are shown by the presence of abnormal cells upon these sections to be diseased are compared with reference to their position, we are enabled to pursue the anatomical track of these secondarily diseased fasciculi, and to arrive at conclusions with regard to the course of nervous currents. The results which follow agree in part with the previous views of the direction of nerve-fibres, and are partly such as could not be reached by anatomy and physiology alone.

3rd. A fasciculus of the spinal marrow descends from the crus cerebri, continuing through the longitudinal fibres of the pons Varolii and pyramid of the same side, until, reaching the decussation of the latter in the medulla oblongata (in

one case in two fasciculi), it passes over to the opposite side, and descends almost to the extreme end of the spinal cord, constituting the posterior half of its lateral column.   This we designate the "pyramidal track of the lateral column."

4th. The "pyramidal track of the lateral column" conveys a centrifugal current proceeding from the prominentiæ lentiformes, corpora striata, optic thalamus, and medullary matter of the cerebrum (of which it cannot be said with certainty that it is a motor impulse), towards that side of the body opposite to the diseased half of the brain, but to the same side on which the fasciculus conducting this current is placed in the spinal column.   This track is found to be diseased secondarily throughout, in cases of chronic apoplexy and other encephalic affections.

5th. A second fasciculus proceeds from the crus cerebri, and through the pons Varolii of the same side, as longitudinal fibres; but instead of crossing in the medulla oblongata, as was the case with the preceding pyramidal track, this second fasciculus descends on the same side of the spinal cord, as an internal division of the anterior column, where, however, its secondary affections terminate rather higher than those of the posterior section of the opposite lateral column. This we call the "enveloping track of the anterior column."

6th. The "enveloping track of the anterior column" conveys an impulse in a centrifugal direction to that side of the body opposite to the disease of the brain, and likewise opposite to the conducting track of the spinal marrow; and brought from the prominentiæ lentiformes, and corpora striata.   This track probably conveys a motor impulse, and it is found secondarily affected when disease exists in one or both of the last-mentioned ganglia of the brain.

7th. With the exception of the above-mentioned tracks, neither the grey matter nor any other of the fasciculi was found diseased secondarily to affection of the brain.

8th. It is not decided whether the motor impulses, proceeding from the cerebrum, are conducted downwards through these two above-mentioned tracks alone, or seek some other way.

9th. The internal section of the posterior column continues through the soft fasciculi of the crura, and appears to reach its final termination on the floor of the fourth ventricle.   By this track a centripetal nervous current is conducted, and it is found secondarily diseased above that point in the spinal cord at which effusion has destroyed the conducting power and arrested the centripetal stream.

10th. A second track, likewise centripetal, is found in the posterior half of the lateral column, and consequently a deposition of granular cells within this track above a point at which an affection of the spinal cord may have occurred. In this posterior half there is both a centripetal and centrifugal conduction. The fasciculi united in the posterior half of the lateral column separate in the medulla oblongata, the centrifugal coming from the pyramids, whilst the centripetal in the medulla, directing itself always more backwards, mounts to the corpora restiformia.

11th. With the exception of the centripetal tracks mentioned in Nos. 9 & 10, neither the grey matter nor any other fasciculi of the spinal cord were found secondarily diseased.

12th. Whether through these two tracks the sense of muscular tonicity or ordinary sensation is manifested, remains at the present unascertained.

13th. The remaining fasciculi of the spinal cord are to be considered as separated in their anatomical and physiological character from those that have been treated of above.   In the first place, the separation in the cervical portion of the spinal marrow exhibited between the external and internal sections of the anterior column by the sulcus intermedius anterior, is established as a complete division of those parts extending to the lowest extremity of the spinal cord.   The same is probable with regard to the separation of the posterior column into two lateral sections by the sulcus intermedius posterior, although this is only demonstrated so

far as the fourth thoracic pair of nerves. A similar division is found between the anterior and posterior sections of the lateral column, not exhibited, however, by any external mark of separation. Each half, therefore, of the spinal cord includes six fasciculi. Secondly, it is evident that these fasciculi do not conduct a centripetal current originating in either the lower extremities or the lower section of the trunk; perhaps they may serve for such currents from the upper extremities, or the superior portions of the trunk, but this is not probable. And it is yet undecided whether these tracks are used by centrifugal stimulation having its origin in special portions of the cerebrum or cerebellum.

14th. The grey matter is not subject to the formation within it of these abnormal cells; from which, however, no conclusion can be absolutely drawn with regard to its powers of conduction.—*Transactions of the Imperial Academy of Science, Vienna,* 1851.

[For the foregoing abstract of Dr. Turck's valuable Memoir, we are indebted to our able contemporary, the *American Journal of the Medical Sciences.* We have given it in full, deeming it probable that this method of investigation, if zealously and carefully pursued, will throw more light upon the disputed question of the continuity of the spinal and encephalic fibres, than any other is likely to do. And we would specially recommend this subject to the attention of the rising generation of anatomical and pathological observers, from whom we have already received so many contributions of great importance to medical science. We strongly suspect, however, that the "granular cells" described by Dr. Turck, are nothing else than the granular bodies described by Dr. Augustus Waller (Phil. Trans., 1850) as the result of the breaking-up of the axis-band of the nerve-fibres.]

Contributions to the Physiology of Vision. Part II.—On some remarkable and hitherto unobserved Phenomena of Binocular Vision. By CHARLES WHEATSTONE, F.R.S.

THIS paper is a continuation of the memoir presented by Professor Wheatstone to the Royal Society in 1838, and published in the 'Philosophical Transactions' for that year; in which he first announced his very remarkable discovery, that the notion of *solidity* or *relief* which we derive from the direction of the visual sense to solid bodies, is essentially dependent upon the reception and combination *by the mind* of two *dissimilar* perspective views projected upon the two retinæ respectively. The demonstration of this fact, not only most important in itself, but most essential to the due interpretation of a vast number of other visual phenomena, was afforded by the *Stereoscope* devised by Professor Wheatstone, the action of which was, to form upon the two retinæ the two dissimilar pictures, not from the object itself, but from two perspective projections of such an object; the mental combination of these two pictures, and the consequent reproduction of the object to the *mind's eye,* being then found to be as complete as if the object itself had been placed before the vision. Thus two perspective projections of a cube, of a truncated pyramid, or of any other geometrical solid, drawn even in simple outline, when so cast upon the two retinæ as to possess the forms and positions which they would have had if at once derived from the actual object, convey to the mind the most complete conception of that object; and the illusion is still more complete, when the views are not mere outlines, but give the correct lights and shadows of the body from which they are taken. The most perfect illusion is derived from the employment of two *photographic* pictures, taken at the same time, by two cameras, placed so as to form about the same angle with the object as that which the axes of the two eyes would form by their convergence on it; a portrait or statue being thus reproduced to the mind's eye with the completest perception of its solidity. This application was made by Professor Wheatstone soon after the publication of the photographic processes of Fox Talbot and Daguerre, and was announced by him in 1841. A form of stereoscope, partly contrived by Sir David Brewster, has recently come into general use, which has the

advantage of portability over the original mirror-stereoscope; but it is limited to the exhibition of a much smaller variety of phenomena than that to which Professor Wheatstone's instrument can be adapted.

We have given this *résumé* of the fundamental idea of Professor Wheatstone's former memoir, and a notice of what he has subsequently done in the matter, because some very extraordinary attempts have been recently made to mystify the public as to the real inventor of the Stereoscope, and the demonstrator of the true doctrine of Binocular Vision; claims having been advanced by Sir David Brewster on both these points, for which there is not the shadow of a foundation. We believe ourselves to be fully acquainted with the whole history, so far, at least, as it can be made out by published statements; and we have not the slightest hesitation in making the assertion, that the entire merit of the *idea* of the original dependence of our visual perception of solidity upon the mental combination of the two dissimilar impressions made upon the two retinæ,—and further, that the whole merit of the realization of that idea, by means of the mirror-stereoscope, long before Sir David Brewster's attention had been given to the subject at all,—belongs to Professor Wheatstone.

The Second Part of Professor Wheatstone's experimental researches, communicated to the Royal Society as the Bakerian Lecture for the present year, commences with an account of some remarkable illusions which occur when the usual relations which subsist between the magnitude of the pictures on the retinæ and the degree of inclination of the optic axes are disturbed. Under the ordinary circumstances of vision, when an object changes its distance from the observer, the magnitude of the pictures on the retinæ increases at the same time that the inclination of the optic axes becomes greater, and *vice versâ*, and the perceived magnitude of the object remains the same. The author wished to ascertain what would take place by causing the optic axes to assume every degree of convergence, while the magnitude of the pictures on the retinæ remained the same; and, on the other hand, the phenomena which would be exhibited by maintaining the inclination of the optic axes constant, while the magnitude of the pictures on the retinæ continually changed. To effect these purposes, he constructed a modification of his reflecting stereoscope. In this instrument two similar pictures are placed, on moveable arms, each opposite its respective mirror; these arms move round a common centre in such manner, that, however they are placed, the reflected image of each picture in the mirrors remains constantly at the same distance from the eye by which it is viewed; the pictures are also capable of sliding along these arms, so that they may be simultaneously brought nearer to, or removed further from, the mirrors. When the pictures remain at the same distance, and the arms are moved round their centre, the reflected images, while their distances from the eyes remain unchanged, are displaced, so that a different inclination of the optic axes is required to cause them to coincide. When the arms remain in the same positions, and the pictures are brought simultaneously nearer the mirrors, the reflected images are not displaced, and they always coincide with the same convergence of the optic axes; but the magnitude of the pictures on the retinæ becomes greater as the pictures approach. The experimental results afforded by this apparatus, so far as regards the perception of magnitude, are the following: the pictures being placed at such distances, and the arms moved to such positions, that the binocular image appears of its natural magnitude and its proper distance, on the arms being moved so as to occasion the optic axes to converge less, the image appears larger, and on their being moved so as to cause the optic axes to converge more, the image appears less; thus, while the magnitude of the pictures on the retinæ remains constantly the same, the perceived magnitude of the object varies, through a very considerable range, with every degree of the convergence of the optic axes. The pictures and arms being again placed, so that the magnitude and distance of the object appear the same as usual, and the arms being fixed, so that the convergence of the optic axes does not change; while the pictures are brought nearer the mirrors, the perceived magnitude of the object increases, and

it decreases when they are removed farther off; thus, while the inclination of the optic axes remains constant, the perceived magnitude of the object varies with every change in the magnitude of the pictures on the retinæ. After this the author takes into consideration the disturbances produced in our perception of distance under the same circumstances, and concludes that the facts thus experimentally ascertained regarding the perceptions of magnitude and distance, render necessary some modification in the prevalent theory regarding them.

The author next reverts to the relations of these facts to the effect produced by the Stereoscope. The two projections of an object, seen by the two eyes, are different, according to the distance at which it is viewed; they become less dissimilar as that distance is greater; and, consequently, as the convergence of the optic axes becomes less. To a particular distance belongs a specific dissimilarity between the two pictures, and it is a point of interest to determine what would take place on viewing a pair of stereoscopic pictures with a different inclination of the optic axes than that for which they were intended. The result of this inquiry is, that if a pair of very dissimilar pictures is seen when the optic axes are nearly parallel, the distances between the near and remote points of the object appear exaggerated; and if, on the other hand, a pair of pictures slightly dissimilar is seen when the optic axes converge very much, the appearance is that of a bas-relief. As no disagreeable or obviously incongruous effect is produced when two pictures, intended for a nearer convergence of the optic axes, are seen when the eyes are parallel or nearly so, we are able to avail ourselves of the means of augmenting the perceived magnitude of the binocular image mentioned at the commencement of this abstract. For this purpose the pictures, placed near the eyes, are caused to coincide when the optic axes are nearly parallel; and the diverging rays proceeding from the near pictures are rendered parallel by lenses of short focal distance placed before the mirrors or prisms of the Stereoscope.

Some additional observations are next brought forward respecting those stereoscopic phenomena which the author, in his first memoir, called "conversions of relief." They may be produced in three different ways:—1st, By transposing the pictures from one eye to the other; 2ndly, By reflecting each picture separately, without transposition; and, 3rdly, By inverting the pictures to each eye separately. The converse figure differs from the normal figure in this circumstance, that those points which appear most distant in the latter are the nearest in the former, and *vice versâ.*

An account is then given of the construction and effects of an instrument for producing the conversion of the relief of any solid object to which it is directed. As this instrument conveys to the mind false perceptions of all external objects, the author calls it a *Pseudoscope.* It consists of two reflecting prisms, placed in a frame, with adjustments, so that, when applied to the eyes, each eye may separately see the reflected image of the projection which usually falls on that eye. This is not the case when the reflexion of an object is seen in a mirror; for then, not only are the projections separately reflected, but they are also transposed from one eye to the other, and therefore the conversion of relief does not take place. The pseudoscope being directed to an object, and adjusted so that the object shall appear of its proper size and at its usual distance, the distances of all other objects are inverted; all nearer objects appear more distant, and all more distant objects nearer. The conversion of relief of an object consists in the transposition of the distances of the points which compose it. With the pseudoscope we have a glance, as it were, into another visible world, in which external objects and our internal perceptions have no longer their habitual relations with each other. Among the remarkable illusions it occasions, the following are mentioned:—The inside of a tea-cup appears a solid convex body. The effect is more striking if there are painted figures within the cup. A china vase, ornamented with coloured flowers in relief, appears to be a vertical section of the interior of the vase, with painted hollow impressions of the flowers. A small

terrestrial globe appears a concave hemisphere. When the globe is turned on its axis, the appearance and disappearance of different portions of the map on its concave surface has a very singular effect. But the most singular effect, to *our* eyes at least, is produced by looking at the interior of the base of the skull, which is brought out in relief, like the actual base of the brain which lies in it. A great number of other curious phenomena are brought into notice by this instrument; and the whole subject is one of such extreme interest both to the physiologist and the psychologist, that we rejoice to find that Professor Wheatstone is likely to pursue it with all the zeal and ability for which he has long been so conspicuous.—*From the Proceedings of the Royal Society, Jan.* 8, 1852.

On the Valves of the Heart. By W. SAVORY.

THE paper contains observations upon the structure and connexions of the auriculo-ventricular and arterial valves of the human heart, which the author thinks will assist in explaining their nature and functions; of these observations we can only cite a few.

The structure, connexions, and relations of the valves are examined chiefly by means of vertical sections carried through their centres and adjacent parts. Such sections of the *arterial* valves disclose an important relation which they have with the upper border of the ventricles. The aorta and pulmonary artery, expanding towards their termination, are situated upon the outer edge of the ventricular border of the orifice; the consequence of which arrangement is, that the portion of valve adjacent to the vessel passes over and rests upon the muscular substance, and is supported upon the inner border of the free edge of the ventricles surrounding the arterial orifices. This arrangement, in consequence of the small size of the parts, is not so obvious at the first glance in the human heart, but is more strikingly shown in an examination of the heart of any one of the larger animals. This appears of importance when viewed in connexion with the functions of the valves. The reflux of the blood is said to be sustained by the festooned rings at the base of the valves, but in fact they are thinnest at this very part, corresponding to the central portion of the convexity of the valves; and if the description previously given of the formation of the tendinous festooned rings be a correct one, it is obvious why it is so, the thicker portions being the projecting angle at the junction of two valves, to which points the tendinous fibres of the valves converge. Now, inasmuch as the posterior portion of the aortic orifice is continuous with the left auriculo-ventricular aperture, no muscular tissue of the ventricle existing in this part, the posterior aortic valve, and a portion of the adjacent one, have no support of this kind; but the muscular floor of the anterior aortic valve is especially broad, and it is the corresponding portion of the aorta which is particularly dilated, the posterior wall descending nearly vertically. The arrangement above described obtains in all three pulmonary valves; but as the border as well as the walls of the right ventricle are considerably thinner than those of the left, the muscular floor of these valves is much narrower than in the anterior aortic valve. All this is of course seen on a much larger scale in the hearts of the larger animals, as the horse and ox; and here, where the muscular floor of the valves (more especially the anterior aortic) is of very considerable breadth, the tendinous tissue of the valve may be traced over the muscular surface to form the wall of the vessel.—Among the tissues entering into the structure of the arterial valves, elastic fibres are described. They exist, not only in the corpus arantii, but delicate fibres of elastic tissue are found throughout the valve; most abundantly in the thicker portions, but even in the thinner portions (lunulæ) a few delicate but well-marked elastic fibres may be seen, particularly after the addition of acetic acid, which of course assists greatly in bringing them into view.—Muscular fibres have not been found in the arterial valves.

The structure and connexions of the *auriculo-ventricular* valves are next

examined by means of vertical sections.   In tracing down the muscular wall of the auricle, it is observed to pass on to the inner surface of the ventricular border, and if minutely examined is seen to terminate by two attachments.   The external portion, which is considerably the larger, is closely connected with the fibrous structure forming the "auriculo-ventricular ring," while the thinner internal portion is continued forwards for a very short distance between the surfaces of the valve, and terminates more or less abruptly by an attachment to its tendinous tissue.   This is generally best seen in one of the tricuspid valves, where, in a vertical section, the muscular fibres may be observed terminating beneath its upper surface immediately beyond its attachment to the ring.   In the posterior mitral valve the muscular fibres seldom penetrate so far forwards, and this appears to result, when a section of the parts is examined, from the much greater thickness and density of the lining membrane of the left auricle.—The extremities of the two bones which in ruminants replace a portion of the lateral and posterior divisions of the 'festooned ring,' nearly meeting in the centre, behind, give additional support to the structures entering into the formation of the mitral valve.

In examining the structure and connexions of the auriculo-ventricular valves, it is noticed that a considerable portion of tendinous fibres pass from the insertions of the cords, through the valves, to the zones, and many of the smaller cords pass up directly into the angle formed between the under surface of the valve and the inner surface of the ventricle, and at once enter into the formations of the fibrous zones.   These cords are short, and many of them spring from the wall of the ventricle, behind the valve.   Therefore it results, that these zones are densest and most strongly marked in those portions corresponding to the attached borders of the valves, and gradually become less distinct towards the intervals between them. Hence the greater portion of the auriculo-ventricular zones is more properly to be considered in connexion with the valves.

The fibres of elastic tissue exist in the auriculo-ventricular valves, but more sparingly than in the arterial valves.

The many contradictory statements which have been advanced concerning the existence of muscular fibres in the auriculo-ventricular valves, may perhaps be explained by a consideration of the mode in which the muscular fibres of the auricles terminate, which has been already described.   The internal fibres which have been mentioned, descending from the auricular walls into the valves just beyond their attached margins, may be traced to a greater distance in some cases than in others. They generally terminate by a tolerably well-defined margin, but this varies.   They usually descend for a greater distance between the layers of the anterior mitral valve, immediately beneath its auricular surface; but even here they are seldom found stretching far into the valve, not terminating, however, so abruptly.   If, therefore, a portion of the attached border of a valve immediately below its upper surface be examined, muscular fibres in abundance will generally be detected; whereas, if sought for in any other portion of the valve far from its attached border, according to the foregoing observations, they will not be found.—*Proceedings of the Royal Society*, Dec. 18, 1851.

On the Condition of Albumen in the Economy.   By M. MIALHE.

M. MIALHE states, in the following propositions, the results of a series of chemical and physical investigations which he has been pursuing.

1. The *normal* albumen of the serum of the blood and of white of egg does not traverse animal membrane.   When, in endosmotic experiments, there appears a certain amount of albuminous matter in the external fluid, this is not normal but *modified* albumen, proceeding from the maceration of the membranes, which have allowed the transudation of the albuminous matters with which they have become impregnated.   When the animal membranes are placed in a preservative fluid, as syrup, or the membranes of the egg, which long resist maceration, and are perfectly endosmotic, are employed, the serum of the blood and white of egg never

traverse them. Albumen is then insoluble.—2. This condition of insolubility should imply an organization similar to that of other substances which do not obey the laws of endosmosis, as fibrine, caseine, and cruor in animals, gluten and starch in plants—substances known to have a globular organization, and held in suspension by the liquids which act as their vehicles. The globular condition of albumen cannot be directly seen by the microscope, or even with the aid of barytes-water; and in spite of the analogies which lead to its admission, it cannot be considered as demonstrated. But it is certain that it must have a special organization, which keeps it in a state of suspension instead of solution, and renders it precisely similar in chemical and physical properties to globular substances.—3. Like these globular substances, it must undergo modifications, in order to enter the economy, which render it soluble, and capable of assimilation; and albumen, *modified* by the ferment pepsin, becomes quite soluble, and capable of traversing membranes.—4. In consequence of these transformations, albumen exists in the economy in three very different conditions as regards their properties—viz., *normal* albumen, *modified or caseiform* albumen, and *albuminose.*—5. Morbid influences, by modifying the conditions of the physiological state of the membranes and the liquids, give rise to phenomena different from those which occur in the normal state. As a consequence of inflammations, excess of watery principles, defective viscosity, or the introduction into the economy of virus, miasms, poisons, or putrid ferments, the membranes cease to be endosmotic, and only present the phenomena of imbibition or filtration, analogous to those which take place after death. The vitiated and disorganized liquids (the blood and its elements) transude through the vessels, and appear in the splanchnic cavities, the cellular tissue, or the products of secretion.—6. Amidst this passage of albuminous matters into the dejections, we again meet with the three conditions of albumen, each connected with different pathological states—viz., *normal* albumen in extreme alterations of tissue, *modified* albumen in a vitiated state of the fluids, and *albuminose* in defective assimilation, or under the choleric influence.—*L' Union Médicale*, No. 90.

On the Habitual Presence of Sugar in the Urine of the Aged.
By M. Dechambre.

During the great impulse which investigations into the characters of the excretions have received in the present time, we ought to be certain that some principles discovered are really due to a pathological condition, and do not, under some circumstances, exist normally. M. Bernard has shown that sugar may be physiologically produced by the liver, and the question is, what becomes of it. M. Reynoso has suggested that it is destroyed by pulmonary combustion, and that when the respiratory function becomes impeded, it will be found excreted in the urine. M. Dechambre, taking up the question at this point, argues, that if insufficient hæmatosis gives rise to glucosuria, we ought to meet with this in the aged. He refers to the well-known researches into the condition of the respiratory organs of the aged, carried on by himself and M. Hourmann at the Salpêtrière, and described in the *Arch. Gén.* for 1835. These exhibited lateral depression of the thorax, projection of the sternum forwards, rigidity of the costo-vertebral articulations, ossification of the cartilages, and a rarefied condition of the pulmonary parenchyma, in which the cell-walls were found thinned or ruptured, and the capillary vessels obliterated. The defective hæmatosis which results from these physical changes should, then, favour the production of glucosuria; and experiments performed upon the urine of a considerable number of the aged women of the Salpêtrière have so constantly exhibited it, that M. Dechambre considers himself justified in asserting that *sugar exists habitually in the urine of the aged,* although its presence there may be possibly explained upon some other hypothesis.—*Rev. Méd. Chir.*, tom. xi. p. 289.

PATHOLOGY AND PRACTICE OF MEDICINE.

On the Hereditariness of Phthisis. By Dr. Hervieux.

Dr. Hervieux' object is to contribute some facts towards the solution of the question, as to the *mode* in which tubercles are propagated from parent to child, and at what period these become developed. He quotes the results of 711 autopsies made at the Hôpital des Enfans Trouvées. Of this number, about 400 were less, and 300 more than 15 days old. Of the 711, tubercles were only found in 32; a fact sufficiently remarkable for those who know the frequency with which tubercles occur in young children. But it is to be observed, that these 32 cases are not equally distributed among the 711 children; and the chief value of the present communication lies in its insisting upon this distinction. Thus, among the 400 children who had not passed their 15th day, tubercles were only found in 2; one 11, the other 13 days old. In the other 300 children, they were found 30 times —viz., 8 times in children from 15 days to 1 year; 8 times in from 1 to 2 years; 10 times in from 2 to 3 years; and 4 times in from 3 to 5 years (very few children above 3 years being, however, received into the infirmary). Examining the distribution of the cases which occur between 15 days and 1 year, we find none prior to the 4th month; 1 at the 4th, 1 at the 6th, 2 at the 9th, 2 at the 11th, and 2 at the 12th. Thus the rarity of tubercles in infants extends not only to the first fortnight, but the first four months—the parent evidently only transmitting the predisposition to the disease. And if in an hospital, where so many causes of insalubrity prevail, this immunity extends for 4 or 5 months, it may, in private practice, be expected to be still longer, during which period we have the opportunity of employing prophylactic agents.—*Rev. Méd. Chir.*, vol. xi. p. 231.

On the Influence of the Puerperal State on the Duration of Diseases. By M. Gendrin.

In referring to a case of rheumatism in a pregnant woman, M. Gendrin observed that the efficacy of any treatment adopted would not be complete until some weeks after delivery. It is a rule without exception, that the exaggeration of the vital functions characteristic of the puerperal state, impresses a more tedious character upon the diseases which occur during pregnancy or immediately after delivery. In women who have undergone the most favourable delivery, and in whom the puerperal state has been unattended by any serious symptom, there are observable, at first, pallor and a morbid aspect of the skin, erratic sweats, &c.—health, in fact, as yet ill-established. Under the most favourable circumstances, seven weeks are required before the equilibrium of the functions becomes re-established; and of course a much longer period is necessary if the woman has been ill during pregnancy, or soon after delivery. Whatever treatment we adopt, intercurrent diseases will be prolonged during the duration of this temporary physiological condition. Hence we must not be surprised at their obstinacy, nor seek by violent remedies to obtain a termination that can only be compassed at a certain epoch.—*Rev. Méd. Chir.*, tom. xi. pp. 164.

On Growth in Relation to the Diseases of Children. By M. Bouchut.

1. *Influence of Diseases on the Growth.*—In considering this subject, we must not mistake *apparent* for real increase or arrest of growth. After great fatigue, the height of young people becomes diminished, to be reacquired after repose. A young man of five feet nine inches, after spending a night at a ball, measured only five feet seven inches, but recovered the two inches after twenty-four hours' rest. The conscripts who are only just above the legal height often manage to escape selection by undergoing violent exertion of all kinds on the eve of examination.

Nor must we mistake *apparent increase* for real. A child is measured when he falls ill, and then again when he gets up, after a fortnight's illness, and he is found to have gained three centimétres. Next day, however, these are reduced to one, which is his *actual* increase, the other two being only the *apparent* increase, which M. Bouchut attributes to the tumefaction of the intevertebral and inter-articular cartilages, induced by prolonged recumbent posture. The only disease which really *arrests* growth in children is *rickets.* In twenty boys, from one to two years of age, suffering from it, the height was found to be more than six centi-métres below the proper mean of the age; and in twelve girls, nearly seven centimétres below it; and in thirteen children between two and three years of age, the mean was found four centimétres below the normal one. By the side of rickets should be placed the influence of a diet ill suited to childhood, which, with-out giving rise to an actual rickety condition of the bones, interferes with the general nutrition, and may impede the growth. On the other hand, there are several diseases which *increase* the growth; and it results from M. Bouchut's observations, that in eclampsia, and in various acute and inflammatory affections, an increase of from one to three centimétres takes place. This is, however, only apparent; for, under the influence of fatigue, games, &c., one or two centimétres of the increase are lost, reducing the real growth to one-third of the apparent.

2. *Influence of Growth as a cause of Disease.*—Many ill consequences have been attributed by various authors to an excessive rapidity of growth. Such children are thin, and their muscles are flaccid and void of power; their joints are often painful, and their sphincters relaxed. The development of the intellectual faculties is retarded, and the child too long retains a liking for amusements beneath its age. In some cases, febrile action would seem to be due to this precocious growth. One general circumstance is observable in all cases—viz., a weakness of the muscular system, and especially that of the lower extremities, where it may go on to complete paraplegia. All children grow much during acute diseases; and whoever has observed them during convalescence, must have remarked their weak, uncertain, and tottering steps—very unlike what is observed in the adult under similar circumstances. This condition continues for a long period; and the debility which at first is manifested in all parts, persists in the muscles of the lower extre-mities long after it has disappeared from those of the upper.

In respect to the hygienic and therapeutic treatment, when a child does not grow, we may, in the absence of any special hereditary circumstances, suppose that rickets exists, or is commencing, and should change the regimen and mode of life. We should insist upon the exclusive use of milk diet in its various forms, suppress the use of meat, vegetables, or wine, and send the child to the country. If the affection is distinctly present, we should resort to salt-water baths and cod-liver oil. When, on the contrary, growth is too rapid, we must diminish the quantity of milk and light diet, and endeavour to habituate the stomach to a strongly animalized regimen. Cold affusions should be daily had recourse to, as should, in summer, cold bathing. Riding and walking are very necessary, and these exercises are the more important, as we know that exertion induces a tem-porary decrease in the vertebral column. Prolonged exertion while carrying a weight upon the head is very useful. These children should sleep on hair mat-tresses, and only remain in bed long enough to recruit their strength—seven hours usually sufficing for this purpose. General gymnastic exercises are in these cases of great utility, increasing the bulk and tonicity of the muscles, by which they are enabled to resist the extension of the bones they cover.—*L'Union Médicale,* Nos. 68 & 69.

On the Proportion of the Subjects bitten by Mad Animals, who become affected with Hydrophobia. By Professor RENAULT.

M. RENAULT, Clinical Professor at the Veterinary School at Alfort, has recently presented to the Academy a valuable Report, adverse to the claim again recently

set up for the prophylactic power of mercurial inunction in hydrophobia. As this claim has been over and over again refuted, we should not have adverted to the subject, but for the valuable incidental matter introduced by the professor into his Report. He observes, that before we can receive the numerous cases which have been published of the preservative power of mercurial salivation, it is necessary—
1. That it should be placed beyond all doubt that the animal inflicting the bite was really mad; and that all the persons supposed to have been preserved have really been exposed to the infection. On examining the histories of these cases, however, such proof is quite defective, as in some of those related—e. g., by Ehrmann and Audry, the persons had not been bitten at all, but had merely lain or drank with those who had, or had wiped away their saliva.—2. Next, when persons have been really bitten, and the animal has been really mad, we must be satisfied that the poison has been deposited in the wound. But numbers of the persons referred to have been bitten through their clothes, and every one knows how small a proportion of such acquire hydrophobia, even when no treatment at all has been employed.—3. But even when the poison has been duly deposited in the wound, to admit the preservative power of the mercury, we must allow that all such persons would necessarily suffer from hydrophobia if left to themselves. Numbers of cases contradictory of this are, however, on record. But although all careful observers admit this, the *proportion* of such as escape has never been duly investigated; and the subject being one of the highest interest, M. Renault takes the opportunity of communicating the results of the investigations he has been conducting upon it at Alfort since 1828.

To prevent confusion, he arranges the cases he has collected in two categories. (A.) Cases of dogs or other animals accidentally bitten by others, either mad or supposed to be so, and sent by the police to Alfort, to remain under inspection. Of 224 dogs so brought, between 1827 and 1837, which continued under observation for more than two months, without undergoing treatment, 74 or nearly one-third became mad, and 130 or two-thirds exhibited no symptoms. It is evident, however, that these cases do not afford any measure of the activity of the virus; for the dog that caused the bite may not always have been mad, the bites could not always be verified, and the hair of the animal may have prevented the poison from penetrating.

(B.) For this reason, another series of facts is adduced. From 1830 to the present time, certain dogs known as really mad have been made to bite, at the Clinical School, other dogs or herbivora in portions of the surface of delicate structure and devoid of hair, or inoculation has been performed with some of their saliva, collected during the height of the paroxysm. Of 99 dogs, horses, and sheep, so treated, 67 became mad, and 32 continued under observation more than 100 days without any symptom exhibiting itself. Thus, in this category, in which every circumstance favourable to infection was secured, no less than one-third of the animals escaped, without undergoing any treatment whatever.

Examining the experience of other clinical professors, M. Renault says, that Professor Rey, of Lyons, has found that among animals who were accidentally bitten in the streets, and then placed under *surveillance,* 1 to 5 of the dogs, and 1 to 4 of the horses, became mad. Of those bitten or inoculated experimentally, 2 to 3 became mad. Of 16 animals accidentally bitten at Toulouse, 5 became mad. Professor Hertwig, of Berlin, states, that of 137 dogs accidentally bitten, 16 only became mad, 121 remained uninjured; while of 25 experimentally infected 10 became mad, and 15 did not. Thus, in both the categories, the proportion affected was sensibly less in Berlin than in France, whether this be owing to climate or other cause.

Thus, taking things at the worst, it results from these observations, made at different epochs, that two-thirds of the animals accidentally bitten, and one-third of those artificially infected, escape.

Still, it must not be supposed, that these *mean results* of a great number of observations can be applied when speaking of the bite of a particular dog; for

M. Renault has repeatedly observed, that while one dog, evidently mad, bites several others, and only one-sixth or one-seventh of these shall suffer, the virus conveyed from another dog, to all appearance in just the same condition, will infect nearly every animal (five-sixths or six-sevenths).

Moreover, it is generally believed that the bites of *mad wolves* are oftener followed by hydrophobia than are those of dogs. Of 254 examples of such bites, the histories of which M. Renault has been able to collect in authors, in 164, or nearly two-thirds, hydrophobia followed — the proportion for accidental bites by mad dogs being only one-third. Whether this depends upon the fact, that in the wolf the rabies is oftener spontaneous, or upon the more remarkable one, that this animal almost always bites its victim in the face, neck, or head, is uncertain.— *Bull. de l'Acad.*, tom. xvii. p. 280.

[Hydrophobia has recently been very prevalent in France, so that something like a panic has been produced, and the journals teem with accounts of supposed remedies. The police have made a terrible onslaught on all wandering dogs; and the exemption of London from the disease, since the prohibition of the employment of dogs as beasts of burden, has been adverted to. It would be desirable to ascertain how far this exemption has extended beyond the metropolis, where alone the dog-act is in operation. How much remains to be cleared up in the history of *rabies canina*, is seen by a communication from Clot-Bey (*L'Union Med.* 1852, No. 94), in which he states, that while dogs exist in such vast numbers in various parts of Asia and Africa, hydrophobia is of the rarest occurrence. In Egypt, where he lived during twenty-five years, he never heard of a case, although the dogs wander about the towns and villages in vast numbers, under a burning sun, panting for breath, ill-fed, and often deprived of water. He thinks that much may be due to the promiscuous intercourse which their entire liberty allows to these animals. In Greece, on the other hand, the disease is excessively prevalent.]

On Cancroid, or Epithelial Cancer.   By M. MAISONNEUVE.

THIS affection has the greatest possible resemblance to cancer, deceiving the most experienced eye and the most matured practitioner, so that without the intervention of the microscope, it would probably always have continued impossible to recognise its true nature. Surgeons were, indeed, aware that certain cancers of the most hideous aspect, and most fetid smell, were capable of being removed by operation without relapse occurring, and that a cure was still sometimes obtainable after one or more relapses, notwithstanding that the neighbouring glands had become invaded. They were also aware, that some of these distressing affections could go on corroding the surface for years, without infecting the entire organism, and without inducing cachexia; and that, at last, they led to death much rather by inducing local disorders than by general infection. These facts were regarded as peculiarities of evolution, the reason of which was quite unknown; but the microscope, by revealing to us the fact that these peculiarities are dependent upon special anatomical characters, has enabled us to constitute a distinct family in the class of cancers, under the name of cancroid, or epithelial cancer.

Examined by the microscope, its essential character is the epidermic cell, which may easily be recognised by its distinct outline and regular form, and especially by its small and rounded nucleus. These cells are often seen agglomerated together into rounded bodies, which have been termed epidermic globes. The epidermic matter does not present itself under the lactescent or emulsive appearance of cancer, but under that of a whitish substance, having the consistency of soft paste, and spreading out in water in a lamellar form. It is sometimes seen in little rounded masses resembling tubercle; but it is oftenest infiltrated into the tissues which it invades, whence it can be squeezed or scraped. It always originates in an external or internal tegumentary surface, naturally covered with epithelium, as the skin, mucous membrane, or even serous membrane; but when

it takes on extension, the epidermic infiltration may penetrate to great depth in the various tissues. It frequently invades the lymphatic glands, which are in immediate relation with the diseased parts; but when it has even passed this barrier, it never determines a general infection, or even a cachectic state of the economy.

This absence of infection is the capital point in its history, and the one which, in a clinical point of view, constitutes its essential distinction from true cancer. Anatomy and physiology seem to afford an explanation of this difference. Thus, in considering the mode of nutrition of our healthy tissues, we find that the cells of which they are composed are constantly undergoing absorption and elimination through the natural emunctories. As in *cancroid* the fundamental cell is nothing else than normal epidermis, or epithelium, it is easy to see that if absorption carries it into the torrent of the circulation, it will be eliminated by the regular channels, and give rise to no general disturbance. But in certain morbid products, and especially *cancer* and *pus,* the volume of the cell is no longer in natural relation to the parenchyma of the different secretory organs, so that, if by any means it gains admission to the circulating stream, it has no opportunity of obtaining an exit, and becomes detained in the midst of the tissues, only to excite disorder. To simplify this view of the case, we have only noticed the difference in size of the cell, and especially of the nucleus; but many other differences, both chemical and vital, may exert an influence which we are unable to determine.

As we have no means of modifying the vitality of the tissues which secrete this abnormal quantity of epidermic cells, and no agent capable of inducing their absorption, their destruction is the only source left us; and for this purpose, M. Maisonneuve prefers caustics to excision or ligature. The caustic not only removes the diseased part, but modifies the condition of the tissue that secreted it; and, in fact, this means was preferred by most surgeons even before the microscope had revealed the difference between this affection and true cancer. Some caustics are preferable to others, seeming to exert almost a specific effect upon the epidermic cells, as the arsenious acid and nitric acid. The first of these not only acts upon the parts it is applied to, but it is also absorbed by the lymphatics, and may thus pursue the epidermic molecules into these organs.—*Gaz. des Hôpitaux,* No. 28.

On the Signs Indicative of Hereditary Predisposition to Insanity.
By M. Moreau.

It is an important problem in psychological medicine, to determine the signs which give rise to fears that the children of the insane may suffer like their parents, and which of them are most likely to do so. Certain zoological facts form the basis of M. Moreau's speculations upon the subject. The fact of *hereditariness,* both in regard to man and animals, is now too well admitted to call for further proof; but we have hitherto been content with stating the fact, without inquiring whether nature, in reproducing in the offspring certain organic conditions, acts upon any well-defined and pre-determined plan. M. Lhéritier has shown, in his reports to the Minister of Public Works, in 1848, that as regards animals, invariable laws regulate the mode in which the organization of the parent affects that of the offspring, giving rise to the production of resemblance. The study of these laws has led him to arrange the organs in three classes or series:—1. Locomotive Organs; 2, Nutritive Organs; 3, The Central Nervous Apparatus. This last is divided into two portions: an *anterior,* comprising the cerebrum and anterior portions of the spinal marrow; and a *posterior,* i.e., the cerebellum and posterior cords. The locomotive system is connected expressly with the cerebellum, and the nutritive apparatus with the cerebrum. It is upon such connexion that the laws of resemblance depend—viz., the regular and invariable mode in which one or other parent transmits to the progeny this or that distinct series of organs. The transmission takes place by fixed laws, so that when one parent transmits one series, the other transmits the opposite; and the fundamental point to be observed is, that the

resemblance is not transmitted by some isolated traits, but by one of two grand series of organs, which are well defined and perfectly distinct.

The laws of such transmission are—1. The *law of equilibrium*, which prevails between parents of the same variety, one or other of these *indifferently* giving rise to *either* of the two series of organs.—2. The *law of crossing* is exerted between parents of different varieties; and here the *male* always furnishes the *posterior* series, and the *female* the *anterior* series.

The *law of equilibrium* is that with which we have to do in the solution of the problem proposed, and which, if this law applies to the human species, becomes a mere matter of observation. The *posterior* series of organs, comprising, amongst others, the soft parts of the *face*, and consequently the physiognomy, is the most easy of examination; and the pathological facts thus examined by M. Moreau, confirm the physiological conclusions drawn by M. Lhéritier. In the great majority of cases, when the pathological modifications of the parts specially charged with the intellectual functions have revealed an hereditary resemblance to one of the parents, the distinctive characters of the second or posterior series, as exhibited in the physiognomy, were evidently transmitted by the other parent; and, *vice versâ*, we only meet with this second series in individuals whose brain has remained exempt from all morbid hereditary modification.

Before referring to the statistical results which he has obtained, M. Moreau explains that he understands hereditary predisposition to relate, not merely to the identical disease of which the transmission is dreaded, but to all affections or morbid modifications of the system in which it is seated. Owing to the difficulty in obtaining correct accounts from patients and their friends, the numbers M. Moreau has had at his disposal are not numerous; but the researches of several years confer on some of them a great value. The cases, the antecedents of which were known, amount to 192; and these are divided into three categories:—1. In 164 of the number, it has been invariably found, that when the individual has presented striking physiognomical resemblance to the one parent, he has been indebted for his cerebral organization (and its defects) to the other. Thus, personal resemblance and cerebral disorder may be transmitted by either parent, but never by the same. —2. There were 7 cases in direct opposition to this law—i.e., the individual bore evident personal resemblance to the parent from whom he derived his cerebral derangement.—3. In 21 cases, it could not be decided which parent transmitted the cerebral organization, this being identical in both.

Other facts also observed go to confirm the general results deducible from these figures. Thus (1) in 17 times out of the 192 cases, of two or more children of a family in which the hereditary influence was manifested, those became insane who differed most in personal appearance from the parent who was the subject of insanity, or of some functional disorder of the nervous centres; while those who most strikingly resembled such parent, retained their faculties entire. Again (2), it is known that boys generally bear a physiognomical resemblance to the mother, and girls to the father; and therefore, if our law is a true one, the insanity should be transmitted from mother to daughter, and from father to son. And so, in fact, of 22 females suffering from insanity, 17 had inherited it from the mother, and 5 from the father; while of 142 insane males, 95 had acquired the disease from the father, and 47 from the mother. But the contrary should be observed when this analogy of resemblance was inverted; and so it was; for 47 sons who resembled their father, derived their insanity from the mother; and 8 girls who resembled the mother, derived theirs from the father.

Thus, as a general result, derivable from the evidence of the 164 out of the 192 cases, we may state, in answer to the question with which we commenced, that in 72 per cent. of cases, we are in a condition to indicate which children in a family are those most likely to suffer from hereditary insanity.—*L' Union Méd.*, No. 48.

SURGERY.

Case of Large Laceration of the Colon without External Marks of Injury. By M. MORINEAU.

A WORKMAN received a kick on the belly during a scuffle, and died in nineteen hours. No traces of injury were discoverable externally; but on opening the abdomen, besides signs of peritonitis, there was found a nearly circular, jagged opening in the colon, $2\frac{1}{2}$ centimétres in diameter. Although there are several analogous cases on record, the lesion has been usually found to have occurred to some organ which, by its weight or size, offered great resistance; and when the stomach and diaphragm have been ruptured, there has usually been ecchymosis or other external sign of injury; and M. Morineau, though having frequent opportunities of witnessing this class of accidents, has never seen an example of the kind, without injury to the external surface.—*Revue Médicale,* tome i. p. 593.

On Lateral Deviation of the Great Toe. By M. MALGAIGNE.

WHILE M. Malgaigne was surgeon to the aged men's Asylum, the Bicêtre, one of his duties was to examine the feet of those who petitioned for shoes in lieu of *sabots ;* and he was soon struck with the great frequency with which deviation of the great toe presented itself,—this sometimes amounting to a true luxation. Laforest, Millet, and Broca, who have written on this affection, agree in attributing its occurrence to wearing tight shoes, and in stating that it is oftenest met with in women. Laforest has observed the deviation of the first toe above the second to be the most common; Millet, its deviation against it; while Broca has oftenest observed the deviation below. Malgaigne has observed all these, but especially the deviation above. M. Malgaigne at first, like others, attributed the occurrence to wearing tight shoes; and it was only after finding that the most rigid inquiry could only verify this cause in a few instances, that he altered his opinion. If, indeed, this were a sufficient cause for its production, every one with the shoes worn at present would be liable to it; while we find that, however tight the shoe may be, when it is removed, the toes expand as before it was put on, without leaving any trace of their temporary inclination. In several of the cases examined, cold and rheumatism were referred to as the original cause; and in some of these, the simultaneous deformities of fingers and toes indicated this general cause. The *rationale* of the affection, in other cases, seems to be, that the ligaments of the joint become enfeebled, so that even moderate muscular action, not counterbalanced by their resistance, suffices to produce the deviation. The cause of this weakening of the ligaments may be due to the slight but prolonged irritation, caused by the friction not only of too tight but of too loose shoes; and a callous condition of the skin indicative of this often precedes the deformity. A persistence of the deviation may cause the production of retraction of the muscle and ligament; and in some instances the irritation spreads from the ligaments to the bone, and by inducing hypertrophy of the head of the metatarsus, renders the deformity irremediable. The bone may become partially luxated, and the sesamoids displaced.—*Rev. Méd.-Chir.,* vol. xi. pp. 212—224.

On the Employment of Aluminized Bougies. By M. JOBERT.

WHEN the stricture is combined with chronic inflammation, M. Jobert finds a preliminary treatment by means of *aluminized bougies* of great service in procuring *dégorgement* of the mucous membrane,—the alum not acting as a caustic substance, but producing a modifying influence similar to that induced by nitrate of silver in ocular inflammations. He warms the end of a wax bougie, and then

incorporates with it some powdered alum, so as to form a kind of magma of the wax and alum. If the latter were lying at the surface of the bougie, it might induce ulceration in place of mere modification of the mucous surface, and the inodular tissue left by this, on healing, would only aggravate the patient's condition. When the *dégorgement* is accomplished—usually in from seven to twelve days—the dilatation of the stricture is proceeded with.—*L'Union Médicale*, No. 94.

On Discharge of Fluid from the Nipple in Innocent Tumours of the Breast. By M. RICHARD.

M. RICHARD observes, that while in cancerous tumours, which in their growth induce atrophy of the mammary gland, no discharge from the nipple takes place, innocent tumours, as partial hypertrophy of the gland, or cystic growths, are always attended with more or less discharge or oozing, the fluid being sometimes syrupy or adhesive, and at others bloody. Repeated opportunities of observation have convinced him that this is a valuable diagnostic and prognostic sign.—*Rev. Méd. Chir.*, tom. xi. pp. 18-29.

Case of Fracture of the Anterior-superior Spinous Process of the Ilium. By Dr. ASHBY.

DR. ASHBY related this case at the request of Dr. Mütter, who is not aware of any similar example on record. A strong, athletic negro, æt. 19, was walking rapidly with a large tub of water on his head, when he suddenly stepped into a gully, about 1½ foot deep. He did not fall, but had to be carried home. There was loss of power in and inability to raise the right thigh; and as it was found that the limb could be moved, in all directions, without much pain, and that there was no shortening or lengthening, neither fracture nor dislocation were supposed to have occurred. The lad, however, declared that he felt something give way at the time of the accident, and during the examination; and it was now found, that whenever the thigh was raised to a right angle, and then let rather suddenly down, a crepitus, audible to all present, was produced. The lad referred to the groin as his chief seat of pain, and here both tenderness and tumefaction were present; and a repetition of the movement described, while the fingers were pressed upon this spot, enabled a moveable spiculum of bone to be felt. As the case was so very obscure, much time was devoted to it, and the above demonstration was repeated again and again before its precise nature was decided to be ascertained. The success of the treatment adopted, confirmed the justice of the view taken of the case. "After flexing the limb, a roller, six or eight yards long, was passed firmly round the thigh, then passed firmly over a wet compress placed over the process, thence around the body, and back over the compress, and around the thigh again. The youth was placed on his side, and experienced complete relief. In four weeks he was walking about, but continued to wear the bandage and compress for several months, in consequence of the support they gave him."—*Phil. Med. Exam.*, vol. viii. p. 159.

On the Removal of Foreign Bodies from the Cornea. By M. CHASSAIGNAC.

M. CHASSAIGNAC was for long, in common with most other surgeons, in the habit of endeavouring to extract foreign bodies that had become impacted in the cornea, by means of a cataract needle, during any interval of immovability of the eye that could be seized. This is, however, sometimes both a difficult and dangerous procedure, and this in part from the great mobility of the eye, and in part from the colour of the imbedded body. Pieces of metal become rapidly oxidized, and assume a rust colour, which is easily distinguished in light or blue eyes, but

which so resembles the colour of the iris in dark eyes that it becomes a matter of difficulty to bring the point of the needle exactly where it should come. Again, the body may be so minute, that, although capable of producing a keratitis, it is not discernible to the naked eye.

The movability of the eye increases the difficulty; and M. Chassaignac endeavours first to obtain what he calls an *immovability by tolerance,* by touching the ocular surface several times with the back of the needle. There are subjects in whom this education is difficult and tedious; and in very embarrassing cases, chloroform may be had recourse to, with excellent effect. This is, however, not usually proper for so slight an operation; and M. Chassaignac then steadies the eye by means of Lusardi's speculum, and magnifies the size of the object to be removed by a good lens. Upon an emergency, the surgeon may hold both the speculum oculi and the lens in his left hand; but it is preferable, after having fixed the former, to give it to an assistant to hold. The patient kneels down and sits on his heels, and the eye being now steadied, and the exact locality of the foreign body ascertained by the lens, its removal becomes easy.

All persons accustomed to this class of accidents must have been struck with the terrible intensity of the inflammatory symptoms induced by particles so minute, their persistence and aggravation as long as the foreign bodies remain, and their sudden diminution and remarkable benignity from the instant the exciting cause is removed. These cases are advantageously contrasted with inflammations of the cornea from other causes, which so obstinately resist the most active treatment, and never disappear with the like rapidity. The lesson to be drawn from this is, that predisposing causes play the greater, and local or direct causes only the smaller, part in ocular inflammations.—*Gaz. des Hôpitaux,* No. 56.

On a New Mode of Operating in Varicose Aneurism. By M. Malgaigne.

The difficulty which usually occurs in operating for this, in securing the two ends of the artery, while the veins are incessantly pouring out blood, induced M. Malgaigne to try a new plan of procedure, by which opening the sac, or the integuments covering it, might be avoided. The case was an aneurism resulting from venesection, performed ten or twelve weeks before. A small pulsatory tumour existed at the bend of the arm, which caused little inconvenience. The artery was taken up, by means of two separate incisions, just below and just above the tumour, and the cure was rapid and complete, so that when the patient was seen seven months afterwards, no traces of the aneurism could be observed.—*Rev. Méd. Chir.,* tom. xi. p. 155.

Ligature of the Vertebral Artery, in a case of Gun-shot Wound. By M. Maisonneuve.

M. Maisonneuve relates a case, which, although it terminated fatally, is of interest on account of the course which the ball took, and the successful ligature of the vertebral artery. A lady was shot in the neck, and the ball glanced downwards from the side of the cricoid cartilage, and penetrated between the trachea and œsophagus, the great bloodvessels, and nerves, without injuring any of them, until it reached the sixth cervical vertebra, deep into the body of which it became impacted. In its course it fractured the left transverse process, and divided the vertebral artery. The hæmorrhage which occurred at the time of the accident soon stopped, but recurring again violently on the eighth and ninth days, the wound was enlarged in order to search for the vessel. On removing the finger from the hole in the vertebra, whence the ball was extracted, the ends of the bleeding vessel were perceived and secured; this, from the ease with which the operation was performed, being at the time supposed to be the inferior thyroid. The ligatures came away in 10 days, and all seemed doing well, when the patient, at the end of the third week after the

accident, suddenly fell into a state of coma, and soon expired. The two ends of the vertebral artery were found firmly plugged with coagula, but necrosis had affected the vertebra, and an aperture led to a communication with the spinal canal, where there was sero-purulent effusion.—*L'Union Médicale*, No. 34.

On the Treatment of Epistaxis. By M. Reveille-Parise.

M. Reveille-Parise observes that it is very desirable to be in the possession of a simple means of arresting epistaxis when severe. Plugging is not the simple operation it has been described; it is very tedious, and often excites vomiting or sneezing, which aggravates the bleeding. Moreover, we may not have any appropriate instrument at hand.

He has found three means of great utility :—1. Alcohol, applied upon dossils of charpie, is a most energetic styptic. It causes a strong or even a temporarily painful sensation, and often speedily checks the bleeding. The essential point is, before passing in the alcohol, to dry the nostril well by blowing the nose, and by charpie.—2. Equal parts of powdered gum and alum may be blown into the nares, and dossils rolled in the mixture then applied,—a magma, which arrests the hæmorrhage, being speedily formed. Before removing the dossils, they require to be well moistened with tepid water when the bleeding has quite ceased.—3. The best of all means is, however, the application of carded cotton-wool; and it is surprising that surgeons have hitherto made so little use of this hæmostatic agent. After the nostril is well dried, dossils of pure clean cotton should be passed in, until it is filled. They must not be too tightly rolled, or the blood cannot penetrate the interstices,—nor too loosely, or it will do so too easily, and the hæmorrhage will continue.—*Bulletin de Thérapeutique*, tom. xlii. p. 308.

[Other correspondents of the same journal speak also of the great utility of closing the alæ nasi with the fingers, either as the sole or adjuvatory treatment; also of the compression of the carotid on the same side as that on which the bleeding occurs.]

On the Prognosis and Treatment of Deafness. By M. Marc d'Erpine.

The following are the conclusions which the author arrives at, from the consideration of 159 cases, carefully noted in his own private practice, and compared with 200 cases reported by Tscharner of Berlin :

1. Deafness occurs more frequently in males, in the proportion of 6 to 4, in 10 cases; but while the disease is somewhat longer in yielding in women than in men, a radical cure is oftener procured in the former, and mere amelioration in the latter.—2. Double deafness is more common than single, in the proportion of 8 to 2; and unilateral deafness is oftenest observed in the left ear.—3. Probably because young persons apply more readily on account of this ailment than the aged, more than one-half the patients were below 30 years of age. *Age* is of importance in prognosis. All under 10 were cured or benefited, as were three-fourths of those between 10 and 20, and nearly two-thirds between 20 and 50. Between 50 and 60, only one-fourth were improved; and in all above 60, treatment was without success.—4. The *duration of the affection* also exerts an influence. The cases in which a cure was accomplished dated, upon an average, 14 months. Those which were simply improved averaged 4½ years, or 6 years, according to the amount of benefit; while the cases which resisted treatment averaged 8 years. Nevertheless, some of the oldest cases, and even congenital ones, have offered examples of cure, while others of only a few months' standing have resisted.—5. Failure is usually proportionate to the *intensity* of the deafness. Still, a cure sometimes occurs in very complete deafness. The cases in which there is an evident disproportion between the perception of mere sound and the power of distinguishing conversation, and those in which a single ear has

been long very deaf, are especially obstinate.—6. It is usually believed that diseases of *hereditary* transmission are least curable. M. Herpin has, however, shown that hereditary epilepsy offers as many examples of cure as do the other forms; and the proportion of cures in deafness is precisely the same, whether the disease is inherited or not. In one family presenting 7 cases, 3 were cured and 2 benefited. —7. The indistinct pronunciation of deaf persons does not exclude expectation of amendment.—8. The changed physical character of the *cerumen* does not influence the prognosis; but its absence is characteristic of obstinate deafness.—9. A perforated state of the membrana tympani does not prevent notable improvement.—10. More than half the cases suffered from *tinnitus*, but this does not influence the prognosis.—11. The rapid or slow coming-on of the deafness does not exert the influence on the prognosis that might be expected. The cases, however, which offer most examples of recovery, are those in which the deafness, without coming on quite suddenly, yet appeared in the course of some days, at an epoch the patient can refer to. 12. Among the various external or accidental circumstances, it may be observed that the prognosis is especially favourable when the deafness can be referred to the action of cold or damp, to coryza, bronchitis, sore throat, &c. Otorrhœa, even where fœtid, does not sensibly influence it; and it is favourable when the deafness is connected with scrofulous chronic skin-disease of children, or foreign bodies in the ear. The prognosis is unfavourable when the deafness results from contusions, the reports of firearms, or other loud sounds. Deafness resulting from metastasis, and that which comes on at indeterminate epochs and without appreciable cause, is obstinate. Rubeola, caries of the temporal bone, and acute puerperal disease, also give rise to very obstinate cases; while scarlatina, pertussis, and fever, may induce deafness capable of amelioration, but not of cure.

In the *treatment* of deafness, M. Marc d'Erpine states, that in all cases of deafness, except where it results from cold of a few days' standing, he relies, not upon general, but local remedies, injected through the Eustachian tube into the tympanum. He not only employs vaporous injections, but in obstinate cases throws in weak solutions of caustic potass, liquid ether, or even solution of veratrine. He finds that, from among the entire number thus treated—viz. 110, furnishing 206 ears—52 per cent. of the persons and 50 per cent. of the ears have been cured, or much benefited. Of the 78 cases last treated, 60 per cent. of the persons, and 56 of the ears, have yielded. The injections are graduated in strength, but even when they have occasioned a degree of irritation, no other inconvenience than some temporary pain or uneasiness has resulted.—*Archives Générales*, tom. xxviii. pp. 89, 179, 406.

On the Application of Nitrate of Silver in Acute Tonsillitis.
By M. Herpin.

M. Herpin states that he finds the application of nitrate of silver in substance to be a most excellent mode of abridging the duration of acute tonsillitis, preventing suppuration in persons liable to this occurrence. Even in the most intense cases, accompanied by great febrile action, he has not had to make more than three applications. If suppuration has already occurred, the application is of less avail, and is then, on account of the closure of the jaws, often impracticable. The application must be carefully and methodically made opposite a window. If the velum is inflamed, it should be touched in passing from one tonsil to another, as also may the uvula,—but as a spasm of the fauces is often then induced, this should be left to the last. If the application is made within the first twenty-four hours, a single one often suffices; and this happens in persons who are liable to relapse of this affection, and have already derived benefit from the caustic. If seen later, two applications at the interval of a day, or even three, are required, although the first at once checks the progress of the disease. More than twenty-four hours should

never be allowed to elapse between the applications. Since he first recommended this practice, many of M. Herpin's colleagues at Geneva have adopted it, and with the best effects, in securing the rapid dispersion of a disagreeable though not a dangerous disease.—*L'Union Médicale*, Nos. 75 & 76.

On the Dangerous Region of the Scalp. By M. CHASSAIGNAC.

IN the course of a paper giving an account of a new mode of removing sub-cutaneous tumours, by means of a slip-knot, M. Chassaignac indicates a region or zone of the scalp especially liable to the occurrence of erysipelas, or angioleucitis, after wounds or operations even of the most trivial character. It extends over the band of integument which unites the posterior and anterior fontanelles, along the median line, and is about two fingers in breadth. It is pre-eminently the dangerous region of the scalp; and, according to M. Chassaignac's observation, though upon this point more evidence is required, the traumatic injury of the posterior part of this track is most liable to be followed by angioleucitis, and of the anterior by erysipelas. In explanation of this aptitude of the region, it is to be observed, that the lymphatic net-work of the scalp is not equally distributed; and the *subepidermic* portion, which is to be distinguished from the intracutaneous and subcutaneous, is especially concentrated along the zone indicated. Thus, of 40 punctures for the purpose of mercurial injection made in any other part than this zone, only one or two were successful; while of 20, made along it, 15 or 16 succeeded. For this reason, M. Chassaignac always prefers this track to the temples, in the endermic employment of strychnine, or other substances. — *Bull. de Thérap.*, tom. xlii. p. 400.

MIDWIFERY, &c.

On the Continuance of Lactation during the Progress of Diseases. By DR. RÖSER.

DR. RÖSER states that the results of his observation during thirty-two years, have quite convinced him, that the routine practice of desiring mothers to discontinue suckling, when they become the subjects of serious disease, is erroneous, the ill-effects of the milk upon the infant having been ridiculously exaggerated. He has already called the attention of the profession to this matter, in an essay on typhus, published ten years since, and all subsequent experience has only confirmed him in his views. He objects to the cessation of suckling for any other cause than local inflammation of the breast, the resolution of which it may prevent, and the cessation of the secretion, which, however, is often due to the neglect in continuing to apply the child: and he cites cases in which the continuance of the child to the breast in apparently hopeless affections, was attended by the best results. He remarks upon the inconsistency in arresting a secretion which is so powerful an agent in abstracting the protein-compound from the blood, while, at the same time, by exciting other excretions, and employing various antiphlogistics, we endeavour to diminish the fibrinous and albuminous elements of the fluid. The effect of suppressing pathological secretions in causing or aggravating disease is admitted, and yet we suppress a normal one, which is exerting an important derivative action on existing disease, and has established the habit, on the part of the system, of requiring such a drain. In the various epidemics of typhus witnessed by Dr. Röser, he has found the continuance of suckling, while the milk remains, of great service in the prevention of bronchitis and pneumonia, which are the usual causes of death; and in inflammatory diseases, he recommends the practice even when collapse has occurred. Even when a woman is suckling much beyond her time, the child should not be taken from her during an attack of inflammation.

Dr. Röser strongly objects to the advice so commonly given to women suffering from, or threatened by, *tubercular disease*, to abstain from suckling. He regards it as one of the best of preservatives, as also a means of prolonging life when cavities are formed; and he states that he is in possession of many cases justifying this opinion. Suckling, too, pursued within normal limits (which vary in different women, but average nine months), is always advantageous in pure neuroses, including hysteria itself. In the same way is the numerous class of affections benefited, dependent on a stasis of the blood, and marked by chronic inflammatory action, and the generation of adventitious productions.—*Froriep's Tagesberichte*, Nos. 444 & 446.

On an Epidemic of Puerperal Gangrenous Vulvitis. By M. CHAVANNE.

DURING the early part of the cold January of 1850, several of the puerperal women at the Charité of Lyons were attacked, three or four days after delivery, with vomiting and diarrhœa, or with febrile paroxysms and abdominal pains, or slight hæmorrhage. These symptoms were followed, in twenty-six cases, by lassitude or prostration, and lowness of spirits, and by the development of œdematous redness of the vulva. In a few cases, the disease did not extend beyond this stage, active febrile symptoms becoming, however, developed; but in the great majority, pultaceous plates, resembling Delpech's pulpous form of hospital gangrene, formed on the interior of the vulva and vagina, closely adhering to the mucous membrane. Although their extension became limited in a day or two, they were not separated by the inflammatory process until the end of the first week, or during the second; small, superficial, suppurating wounds being left at the points they occupied, which usually soon healed up, though occasionally degenerating, and becoming covered with the same pultaceous mass. In four of the twenty-six cases, the disease extended to the uterus, and the patient died, having presented all the symptoms of intense puerperal fever, the gangrenous condition of the uterus becoming complicated with peritonitis. No *cause* could be assigned for the development of the epidemic; both the general sanitary conditions of the establishment, and the prior state of health of the patients, having been satisfactory. In twenty of the cases, the labour was natural, the forceps, however, having been applied eight times; and while the affection seized some of the patients who had very easy labours, others of the inmates, whose cases required active interference, entirely escaped. Besides the four cases above mentioned as having proved fatal, three others of the twenty-six died from metro-peritonitis, without extension of the gangrene. The other nineteen recovered, the gangrene usually soon yielding to tonic regimen, and the local use of the strong muriatic acid. A very similar epidemic was observed at Lyons in 1815; and another of the same character has been recently witnessed in Paris.—*Gazette Médicale*, No. 16.

On the Induction of Abortion in the Vomiting of Pregnant Women. By MM. DUBOIS and STOLTZ.

DURING a recent discussion at the *Académie de Médecine*, M. P. Dubois stated the results of his experience in relation to obstinate vomiting in pregnancy. In proof that this is oftener a more dangerous occurrence than is usually supposed, he stated, that in the course of thirteen years he had met with twenty cases in which it has proved fatal. That obstinate vomiting is but the exaggeration of the natural sympathetic vomiting of pregnancy, and not due to any special lesion, is proved by the facts, that at the autopsies nothing is found, and that when the process of gestation becomes arrested, whether spontaneously or artificially, the vomiting is ordinarily put an end to, although the woman may not be delivered until several days after, of a dead child, and may yet die of the effects of what she has already undergone. M. Dubois refers to several cases in which the women, apparently at the point of death, were saved by the *spontaneous* death of the

fœtus, this being expelled only some time afterwards. In respect to the question of how far *artificial* interference is attended with the same result, he furnishes notes of the four cases in which he has employed it. Three of these died and one recovered—this last being added to other cases on record, making the number of recoveries he is aware of certainly 7, and probably 9. In all the cases, however, whether fortunate or not, the vomiting was suspended by the operation. The difficulty is, indeed, to fix the *period* at which this should be resorted to; for it is the natural desire to delay this as long as possible, which leads to the fatal result —the woman dying, in fact, from the exhaustion and prolonged abstinence which the vomiting has induced, prior to the operation for arresting it being undertaken. M. Dubois lays it down as a rule, never to perform it when the signs of extreme exhaustion are present, as evidenced by considerable loss of vision, cephalalgia, comatose somnolence, and disorder of the intellectual faculties. On the other hand, we should also abstain from operating when the vomiting, though violent and frequent, still allows of some aliment being retained; when the patient, though wasted and feeble, is not obliged to keep her bed; when the suffering has not yet induced intense and continuous febrile action; and when other means still remain untried. In the first case, we should not save our patient, but perhaps accelerate her death, and bring discredit on the operation; while, in the other, we should sacrifice a pregnancy that might have gone on to the full time. It is, therefore, the intermediate period that should be chosen, and this is characterized by the following signs:—1. Almost incessant vomiting, by which all alimentary substances, and sometimes the smallest drop of water, are rejected.— 2. Wasting and debility, which condemn the patient to absolute rest.—3. Syncope, brought on by the least movement, or mental emotion.—4. A marked change in the features. —5. Severe and continuous febrile action.—6. An excessive and penetrating acidity of the breath.—7. The failure of all other means. But even within this period, which is of variable duration, the opportune moment must be chosen. This seems to have arrived, when the inefficacy of the most approved treatment has been proved, when fever is found to persist, and the debility and wasting of the patient are making sensible progress. The attendant should now declare that the operation is indicated, leaving to the patient and her friends the duty of deciding upon its adoption.

Professor Stoltz, of Strasburgh, has published a highly interesting communication upon this subject, in which he also states his belief, that vomiting during pregnancy is much oftener fatal than is usually supposed. He relates four cases, from among others, that have come under his own notice. In three of these, death occurred, and life was saved by the operation in the fourth, although the case seemed hopeless. M. Stoltz lays great stress upon the operation being performed *in good time*, because if we wait until the effects of the sympathetic reaction constitute in themselves a serious disease, the evacuation of the womb does not induce a cessation of these, and may, in certain cases, even hasten death—life, so to say, hanging upon a thread. It is undoubtedly difficult to say, *when* the moment has arrived that we can no longer hope for benefit from nature or therapeutical agents. But may not the same observation be made with regard to many important surgical operations? It is true, that neither spontaneous nor artificial abortion always saves life in these cases; but the former usually occurs only when the woman's powers are hopelessly exhausted, and the pain and discharge consequent on the delivery may expedite her end—the same result not being infrequently seen in severe fever. Some practitioners have expressed themselves very feelingly against sacrificing the child in these cases; but there is a great inconsistency on the part of those who do so, and who still advocate the operation in the case of narrow pelvis. A woman who has undergone artificial abortion for obstinate vomiting, may hereafter (and these cases mostly occur in primiparæ) give birth to a living child, which can never be the case in one who has so narrow a pelvis as to call for the induction of abortion rather than of premature labour.— *Bulletin de l'Acad.*, tom. xvii. pp. 557—582; *Gazette Médicale*, No. 23.

On Unavoidable Uterine Hæmorrhage. By M. Depaul.

M. Depaul read an elaborate paper upon this subject at the Académie de Médecine, in which he defended at great length the views held by MM. Stoltz and Dubois, upon the tardy development of the lower segment of the uterus. Our space admits only of the notice of some of the more practical parts of the paper. M. Depaul is of opinion that the attachment of the placenta to the lower segment of the uterus is of much greater frequency than those authors admit, who have only noted the cases in which it has been found inserted over the orifice itself; and he considers Lachapelle's statement, that the majority of cases of uterine hæmorrhage occurring after the sixth month are due to faulty insertion of the placenta, to be correct, when so understood. He does not regard the *diagnosis* of the occurrence by the thickness and softness it imparts to the lower segment of the uterus, as being so easy as stated, when the cervix is long and closed. Nor is the absence of *ballottement* at all conclusive, as he has perceived it in several of these cases. He considers that the hæmorrhage oftener shows itself in the course of the eighth or ninth month, than in the sixth or seventh, as stated by some. Even when the cervix is effaced, and the os opened, experienced persons have sometimes mistaken coagula or excrescences of the cervix, for the placenta. M. Depaul considers that the *prognosis* as regards the mother has been exaggerated, and that with due care the mortality, stated by Simpson at one-third, might be much diminished. It is more serious when the placenta is inserted over, in place of near, the orifice, and in the earlier period, when the cervix is hard and closed. He agrees with Lachapelle, that more than half the children are lost, especially when it is considered that many of those born alive are so before their time, and exhausted by the disturbance of the placental communication.

In the *treatment* of these cases, (1) when the hæmorrhage is slight, and the os closed, and no uterine contractions are present, he recommends the employment of the usual general means to arrest hæmorrhage, such as posture, opiates, acidulated drinks, &c., and small hæmostatic doses of ergot. When (2), with the same conditions of the uterus, the hæmorrhage is great and increasing, besides the above means, he advises plugging the vagina. This may act by favouring the coagulation of the blood, and the obliteration of the vessels left open by the detachment of the placenta; and by hastening the advent of labour and the dilatation of the orifice. This last end is not always attained; for cases have occurred in which the plug has been applied for twenty-four or thirty-six hours, with the effect of arresting the hæmorrhage without producing any modification in the cervix. When, in spite of the plug, the bleeding still continues, in part internally, and in part externally, and the woman appears in any danger, we should rupture the membranes; and if such partial emptying of the uterus is not attended with prompt improvement, delivery should be forced, even if multiplied incisions of the cervix uteri are necessary for this purpose. When (3) the hæmorrhage is slight, and has commenced at the full period of pregnancy, or when repeated hæmorrhages have induced premature labour—a certain degree of dilatation being present—if the loss is very moderate, we may order rest and other general measures, and give ergot to hasten the labour. When the dilatation has become considerable, we should rupture the membranes, especially if the hæmorrhage, without being great, has been of long continuance or frequent repetition, so as to have materially damaged the health. The same course is justified in the interest of the child, the life of which will be risked by its prolonged detention in the uterus after a considerable separation of the placenta. It is in this case that obstetric auscultation becomes so useful; and when this shows the child's life to be in danger, the forceps, or version, should be had recourse to, even when the dilatation is not so great as in other cases would be deemed desirable. When (4) the os is more or less dilated, and the hæmorrhage too serious to admit of temporizing, if the membranes are still intact, the dilatation not being quite complete, these should be ruptured, and the ergot given in preference to using a plug. M. Depaul has, however, the greatest objection to perforate the placenta

itself, at the risk of inflicting injury on some vessel that might prove fatal to the child. Even in central insertion, whenever possible, he detaches an edge of the organ to get at the membranes. In central insertion, too, he would employ the plug as a dilator. On account of the danger the child incurs, he likewise objects to Simpson's plan of prior detachment of the placenta. When, in spite of the discharge of the waters, the hæmorrhage continues, or the child's life is found to be endangered, we must deliver; and in these cases, the somewhat forcing delivery is preferable to the employment of the plug, lest even a moderate amount of uterine hæmorrhage prove too much for the exhausted powers of the woman, or a too long delay prove fatal to the child.

It is an error to suppose that the woman is always safe when delivery has been accomplished; for she sometimes sinks from subsequent hæmorrhage. This occurrence is explained by the special vascular distribution at the lower segment of the uterus, and the less disposition this part has to contract. To secure due contraction, the ergot is the best means; and perhaps it should always be given in these cases immediately after delivery.—*Bull. de l'Académie,* vol. xviii. pp. 849—874.

On the Removal of the Obstructing Mucus in the Suffocative Bronchitis of Infants. By M. Valleix.

In an instructive case which he relates, M. Valleix draws attention to the importance of mechanically clearing away the mucus from the throats of infants, in the suffocative paroxysms observed in the generalized bronchitis so fatal to young children. As soon as the child assumes a violaceous appearance, and suffocation seems imminent, the little finger should be passed along the back of the tongue. The child, closing its jaws, resists strongly, but the finger should be gradually advanced. When it reaches the isthmus, the child opens its mouth, and we should then pass on behind the epiglottis, so as to bring the pulp of the finger several times over the orifice of the larynx. This produces efforts at coughing and vomiting, and the mucus is expelled from the air-passages, a part being drawn out with the finger, and the rest swallowed. The child appears after this little operation much flushed and agitated, and almost suffocated; but it soon becomes calm, until renewed signs of asphyxia call for a repetition of the procedure. In the case which M. Valleix relates, apparent death, after one of these paroxysms, was successfully treated by artificial respiration, the employment of which he strongly recommends in similar cases.—*L'Union Médicale,* No. 48.

On the Treatment of Syphilis in Infants. By M. Cullerier.

Although, in exceptional cases, adults may become freed from constitutional syphilis without the aid of mercury, this is not the case with infants. By appropriate hygiène we may, indeed, disperse all the symptoms; but these will reappear at intervals of greater or less length, until a cachexia is induced, and the child either dies in a state of marasmus, or from its being unable to resist some acute intercurrent affection. A young infant, the subject of syphilis, and not treated with mercury, always dies within a given period. Mercury being indispensable, the question comes, whether it should be administered directly, or *indirectly* through the medium of the milk of the nurse, or animals. Seeing the great confidence with which the latter procedure has been recommended by high authority, one would naturally expect analysis to be at hand, showing that the milk of animals to whom mercury has been administered contains it. Nothing of the kind is to be found in the writings of the older practitioners, who so warmly approved this practice; and if some modern analysts have declared they have detected it, others equally expert have failed. M. Peligot published in the *Jour. de Connaiss. Méd.* the account of his numerous and varied researches, and of their utter failure.

When M. Cullerier was first appointed to the Lourcine, he adopted the pre-valent idea, and gave the mercury to the child's nurse; but the negative results obtained soon made him pause, and inquire whether the milk of women so treated really contains any mercury. His predecessor, M. Huguier, and M. Lutz, phar-macien to the hospital, had never been able to detect any; but new researches were now instituted, both on the milk of women taking mercury, and on that of a goat that had been freely dosed with it. Minute and multiplied examinations failed to detect any in the milk of the women, although they were nearly salivated; and it was not until the goat was far gone in mercurial cachexia, that mere traces could be detected. M. Personne conducted similar inquiries with other reagents, and only succeeded in discovering the merest traces. These infinitesimal traces explain the disastrous effects which M. Cullerier had observed to result from the administration of mercury through the nurse, as well as the tergiversation of those authors, who, after having warmly recommended the indirect, afterwards find themselves obliged to fall back upon the direct method.

Syphilis often makes rapid progress in the infant. A child born apparently healthy, may in a few weeks present the characteristic symptoms, get into an alarm-ing state in a few days, and if left without treatment, rapidly die. To save it, we must give the mercury to itself; and the worse its condition, the more urgent is vigorous treatment. M. Cullerier, on account of the liability of the digestive organs to become disordered, administers the mercury *endermically.* He does not deny that it may be often advantageously given by the mouth, in cases which are slow in progress, the constitution not much shattered, or when the progress of the affection has been arrested by its endermic employment. After placing the infant several times in a bran bath, so as to calm any irritation of the skin and increase its absorbent power, he rubs fifteen grains of the blue ointment into the side of the chest as high as the armpit, rubbing it into the opposite side next day. Twice a week the frictions are suspended, and the child is placed in a tepid bath containing from thirty to sixty grains of corrosive sublimate. For children older than a year and a half, the ointment has to be increased to thirty grains, and the sublimate to ninety grains. M. Cullerier has far greater confidence in the use of baths for children than for adults,—their only inconvenience being, that they cause the signs of the disease to disappear too rapidly, and the parents, believing the child cured, refuse to prolong the treatment for a sufficient period. It is very rarely that this mode of treatment induces erythema or vesicular eruption.

When, as is very common, the genitals and anus are the seat of soft condylo-mata, or ulcerations secreting abundantly, they are sometimes touched with a solution of nit. arg. (4 or 8 parts to 32); but when the secretion is only moderate, or there are only dry tubercles, demulcent decoctions of bran, mallow, or elder, suffice,—always powdering the parts with starch or lycopodium afterwards, and isolating them from each other by dry lint or charpie. When the face is attacked, the same lotions are proper; but as desiccation takes place rapidly, and painful lacerations or fissures are caused by crying or suckling, we should, whenever possible, keep unctuous substances applied.

In this way M. Cullerier has treated great numbers of these unfortunate infants, who, brought into the hospital in a frightful condition, are, after a fortnight or a month's treatment, scarcely recognisable, owing to the vast improvement that has taken place.—*Bulletin de Thérapeutique,* tom. xlii. pp. 433—447.

[M. Cullerier's objections to the internal use of mercury are directed to the employment of calomel and corrosive sublimate; and he does not seem to have used the hydrarg. cum cretâ. Although not, perhaps, familiar with such aggra-vated cases as come under his notice, we have always found this a mild and prompt remedy, rarely disordering the bowels if combined with minute quantities of Dover's powder.]

MATERIA MEDICA AND THERAPEUTICS.

On the Preservation of Leeches. By M. Schüller.

M. Schüller states, that of all the means that he has tried for preserving leeches in a healthy state, and for restoring those that have become diseased, he finds well-burned charcoal the best. It is to be well-soaked three or four times in fresh spring water, and then laid, unbroken, and while yet wet, in a large glass receptacle, into which the leeches, after being well washed, are then to be put, no water besides that which adheres to the charcoal being added. The whole is to be tied over with a cloth, and put into a cool cellar for five days. Unhealthy leeches thus treated become well and eager to bite. In summer, the leeches should be laid in the charcoal once a week, and in winter once a fortnight. By this means, M. Schüller is enabled to preserve his leeches in a most healthy condition, even in the hottest weather. Dr. Büchner has found wet turf as useful as charcoal.—*Büchner's Neue Repertor.*, I. 226.

[It is with great regret that we have to announce that Dr. Büchner, sen., the able editor of the *Repertorium für Pharmacie*, a new series of which he had just commenced, has recently died. From a very humble position in life, he raised himself, by his great talents, intense love of knowledge, and indefatigable industry, to the position of the greatest pharmacologist of modern times; and exerted immense influence in the elevation of this branch of medical science to the present high position it holds in Germany. His efforts were ever directed to the attainment of this object by improvements in education, the advancement of scientific development, and the cultivation of brotherly amity; and he reaped his reward in the affectionate and reverential feelings with which he was everywhere regarded. He was another example of the ruling passion strong in death. Making some movement just before the fatal event, one of his friends asked the cause. "I am thinking," he replied. "Of what?" "Materia medica."]

On Calomel and Soda as a Cathartic. By Dr. Hunt.

Dr. Hunt draws attention in this paper to the remarkable increase of purgative power which calomel acquires by the addition of bicarbonate of soda. So small a dose as two grains, combined with from ten to twenty of soda, acts most efficiently, without inducing the griping stools which calomel does when given alone. After the bowels and liver have been well acted upon, the medicine loses its power, and should be discontinued. Dr. Hunt believes that the soda exerts some power in preventing the mercury acting upon the gums.—*Boston Journal*, vol. xlvi. p. 161.

On the General Action of Tartar Emetic, as produced by its Local Application. By M. Duparcque.

The publication of M. Guérin's remarks upon this subject, adverted to in No. XVIII. has induced M. Duparcque to recal attention to views which he long since laid before the profession, concerning the refractory state of the skin to the operation of tartar emetic and other counter-irritants. He does not allude to the influence of idiosyncrasy, under the influence of which the skin may manifest either abnormal sensitiveness or insensibility; but to a temporary and *acquired* insensibility, dependent upon the presence of certain morbid conditions, especially acute phlegmasiæ and fever. During the existence of such insensibility, the power of *absorption* is still retained; for while blisters under these circumstances will not raise the skin, they will still influence the urinary organs. Soon after the introduction of the Rasorian mode of administering antimony into France, M. Duparcque discovered that, during the prevalence of fever and phlegmasia, while no pustular

eruption was producible on the skin by the tartar-emetic ointment, the antimony was absorbed, and acted medicinally, as if it had been taken into the stomach. Indeed, as the large doses ordered by Rasori are not always tolerated, his plan could not be carried-out in the treatment of inflammation of the brain and digestive organs ; while the local application of the substance admits of its operation being extended to these.   As soon, however, as the pathological condition which prevented the cutaneous irritation from taking place is relieved, and the vital activity concentrated on the internal organs becomes decentralized, an effort being made at the re-establishment of the functional activity of the entire economy, the skin recovers its normal irritability.   Parts which had long remained unaffected by sinapisms, now inflame or suppurate, or renewed applications of stimuli produce effects which they were not able to do before.

M. Guérin has also shown that this insusceptibility may not only be induced by affections occasioning a general perturbation of the economy, and having no immediate connexion with the portion of skin to which the ointment is applied—but likewise by a morbid peculiarity of the skin itself, transmitted to it from subjacent organs.

Another fact, upon which M. Duparcque lays stress, is the *impregnation of the tissues by the tolerated substances* with which they are brought into contact.   Thus parts to which blisters &c. have been applied during the existence of inflammation with little or no effect, may become the seats of ulceration or mortification when this subsides.   Those parts do not participate in the returning vitality of other portions of the periphery, such vitality having become extinguished by the imbibed substance.   This disposition may be strengthened or favoured by certain circumstances, such as the intensity or severity of the disease, the degree of intensity of action exerted by the substance employed, and especially the duration of the application without determining irritation.   For this last reason we should always limit the duration of the application of blisters to children, whether any local irritation be excited or not.   Other circumstances influencing the production of this effect are, sex, constitution, and age ; women, and particularly children, being most liable to its occurrence.   So, too, it is most likely to follow when debilitating treatment has been employed, as bleeding, and especially the administration of calomel.— *Bull. de Thérap.*, tom. xlii. pp. 49—58.

On the Action of Iodine.   By M. Bonnet.

M. Bonnet has recently published two interesting papers on the action of Iodine. In the first of these, he shows that iodine, when applied locally to ulcers or blistered surfaces, or injected into the cavities of abscesses and serous membranes, becomes largely absorbed and excreted, being speedily detected in the urine and saliva.   It may in this way be excreted by the urine to the extent of fifteen grains per diem, without in any way acting injuriously on the general health ; and to be therapeutically useful, such elimination must be continued for six or eight weeks, as shown by the dark-blue colour produced in the urine by starch and chlorine without preliminary evaporation.   In this way scrofulous ophthalmia may be effectually cured by dressing a blistered surface, remote from the eyes, without the employment of any internal or local medication.   The best of all the preparations for this mode of employment, is an ointment composed of iodine one part, iodide of potassium two parts, and lard thirty parts.

In his other paper, M. Bonnet enters upon the consideration of the mode of action of iodine.   He considers that it only operates beneficially in proportion to the amount of eliminatory action it gives rise to, in which respect it surpasses most other substances ; or at all events, our chemical re-agents enable us better to trace its operation.   In the diseases in which the iodine proves useful, there is also morbid material to eliminate, and by catalytic action the economy is enabled to effect this by the eliminatory effort excited by the iodine.   The greater activity of elimination is followed by a greater activity in the renovation of organic mate-

rial. In consonance with this view, M. Bonnet observes,—1. If we are desirous of acting energetically on the economy by means of iodine, we must maintain this active twofold power of absorption and elimination. The patients in whom he has found its use to be especially attended by increase of appetite and strength, are those suffering from large ulcers, in whom the tests exhibited the free excretion of the iodine by the urine; while after each dressing of the ulcer with it, a temporary feeling of febrile heat of the skin was induced.—2. The employment of the medicine should be associated with good hygienic measures, which render the organic renovation more active, and with such remedies as favour elimination. Thus, exercise, free exposure to the air, purgatives, and diaphoretics, second the effects of the iodine, as it in turn augments the influence of these agents.—3. Iodine is no specific, and might be replaced by any substance which can easily penetrate into the economy, and of which the latter can easily rid itself again. Among such are sulphureous and terebinthine preparations, whose excretion is demonstrable, and probably the salts of mineral waters, which, however, our means are unable to detect in the excretions. However this may be, the most varied substances produce effects in common with iodine; and it is this common character of their mode of action which explains how they may be substituted for each other, when the power of exciting eliminatory action becomes enfeebled in either of them.—4. Notwithstanding this view of the eliminatory action of iodine, M. Bonnet believes the action of the iodides on the blood and fibrine, as shown by the experiments of Dumas and himself, must not be overlooked. When an alkaline iodide penetrates into the blood, it exerts a solvent action. If the fibrinous elements are in their normal fluid state, it may only render them less disposed to coagulate; while if they are coagulated it dissolves them. Such coagulation exists within vessels that are the subject of inflammation, and beyond the vessels when effusion of lymph has taken place into the tissues.—*Gazette Médicale*, Nos. 20, 21, & 22.

On the Administration of Sal Ammoniac in Enlarged Prostate.
By M. VANOYE.

M. FISCHER, of Dresden, has long been in the habit of administering this medicine, in large doses, in chronic enlargement of the prostate; and since he published an account of his success with it in 1831, various other German practitioners have supplied corroborative testimony. Seeing that surgical treatment is so often inefficient, this means would seem to supply a valuable resource, and this especially as it may be employed in conjunction with other remedies. In the present paper, M. Vanoye relates two cases in addition, in which the success obtained seems quite to justify former encomiums. In both, the gland became progressively and greatly diminished in size. Large doses are, however, required. We may commence with fifteen grains every two hours, and go on to double or treble this quantity, so that nearly half an ounce is taken *per diem*. When the dose given is too great, we are admonished by disorder of the digestive organs, a miliary eruption, profuse sweats, and especially by scorbutic symptoms. These ill-effects may be prevented or diminished by the employment of mucilaginous vehicles, bitter extracts or aromatics, and a good animal diet. Persons suffering from hæmorrhagic disposition, or affections due to poverty of blood, should not be subjected to this treatment.—*Bulletin de Thérapeutique*, tom. xlii. p. 521.

On Pagliari's Hæmostatic.   By M. SEDILLOT.

M. PAGLIARI, a pharmacien at Rome, professes to have discovered a styptic liquor of great power; and several of the officers of the French army have testified to its efficacy. M. Sédillot has also, on several occasions, brought forward cases in corroboration; and in the present paper he adduces additional ones, in some of which,

considerable vessels, although not those of the first class, furnished the blood. He says that it has been objected, that compression is employed by means of bandages and charpie; but this is merely to prevent the coagula which form being removed from the mouths of the vessels; and it has only to be continued for twenty-four or forty-eight hours. So little plastic is human blood, that compression alone, unaided by styptics, would have to be so prolonged and forcible, that it would risk the formation of ulcers or gangrene in the parts to which it was applied.

M. Pagliari has now revealed the composition, which is as follows: Eight ounces of tincture of benzoin, one pound of alum, and ten pounds of water are boiled together for six hours in a glazed earthen vessel, the vaporized water being constantly replaced by hot water, so as not to interrupt the ebullition, and the resinous mass kept stirred round. The fluid is then filtered, and kept in stoppered bottles. It is limpid, slightly styptic in taste, aromatic in odour, and the colour of champagne. M. Hepp, of Strasburg, has substituted white resin for the benzoin. Every drop of this fluid poured into a glass containing human blood produces an instantaneous magma; and by increasing the proportion of the styptic to the quantity of the blood, a dense, homogeneous, blackish mass results.

Many are the circumstances in which the surgeon may not be able to have recourse to the ligature, as in the case of friable arteries, secondary hæmorrhage from deep-seated, painful, or inflamed wounds, the impossibility of seizing the artery, or where the hæmorrhage results from numerous arterioles, which are too small or retracted, or from veins and capillary vessels. In all cases, in fact, where compression is now usually employed, without much benefit being expected to result from it, and often indeed proving useless or dangerous, this fluid seems indicated.—*Bull. de Thér.*, tom. xlii. p. 491—502.

FORENSIC MEDICINE, TOXICOLOGY, AND STATISTICS.

On the Characteristics which distinguish Impulsive Insanity from Moral Perversity. By M. MICHEA.

M. MICHEA observes, that the signs usually advanced in proof of the insanity of a person charged with crime, such as the contrast of the action to his usual habits, the change of disposition or temper, the indifference to consequences, &c., however interesting to pathological psychology, are not of a character to make an impression upon judges and juries; and he believes, that if we wish to accomplish this, we must not confine our attention to mere psychical signs, to the exclusion of the observation of phenomena which are independent of the will of the accused. In M. Michea's opinion, it is in the *physical or somatic* order of characteristics, that the means of diagnosis is most certainly to be found; and he lays down as a principle, that so surely does mental disorder always coincide with phenomena of pathological physiology, that when these are absent the accused person should be held as a responsible agent. In answer to the question, whether in the case of a homicide, for example, we are in possession of physical signs sufficiently distinct and constant to enable us to declare that the individual was not, at the time of the commission of the act, in the possession of his free will, M. Michea replies *affirmatively.*

Before adverting to the evidence derivable from *spontaneous anæsthesia of the skin,* M. Michea passes under review the various conditions which should influence our judgment.—1. *Dyspepsia.* Nervous affections of the digestive organs are often a determining cause of insanity. M. Michea cites a case in point, in which the patient became insane during the suffering incident upon digestion, the attack ceasing about three hours after each repast, to recur again quickly after the next. Alibert mentions similar cases.—2. *Disordered Menstruation.* M. Michea does

not agree with Marc, that the mere fact of the presence even of normal menstruation, during the commission of crime, should be taken as an extenuating circumstance; but he thinks the non-appearance or irregularity of the menses, when connected with chlorosis, ought to receive great consideration. He knows of the case of a young girl who always possesses strong homicidal propensities prior to and during the flux.—3. *Disordered Muscular Action.* Spasmodic affections, and among others, indolent tic, constant winking, temporary strabismus, simple carpology, chorea, catalepsy, hysteria, epilepsy, and epileptiform convulsions, are of very common occurrence among the insane. There are few lunatics, indeed, who have not already suffered from this class of affections, or who do not at present, or will not hereafter, suffer from them. Van Swieten declares, that almost all the insane have suffered from convulsion in their infancy. M. Falret exhibits the coincidence every year in his lectures, and daily observations of the insane for seventeen years have convinced M. Michea of its reality.—4. *The Puerperal State.* Gestation often produces during the early months a special disturbance of the digestive organs, which, sometimes reacting on the brain, may determine certain perversions of the instincts or feelings. After delivery, homicidal mania is not very rare—infanticide not always being a result of misery or fear of public opinion. Marc has collected several instances of the desire to murder becoming developed during suckling. — 5. *Hereditariness.* It is generally admitted that at least one-half, if not three-fourths, of the insane, have insane relations, even the same kind or variety of insanity being transmissible from parent to child. M. Descuret relates the case, in his *Médecine des Passions*, of a man, who, after leading a sober life until forty-five, then became a drunkard. Three daughters and a son born prior to this period were very sober; but two sons, born after the development of the dipsomania, manifested a most decided disposition to drunkenness, the one at seven, the other at nine years of age. — 6. *Spermatorrhea.* Involuntary seminal discharges, even when not very abundant, constitute in young subjects one of the most common causes of cerebral disturbance—the affective suffering much more than the intellectual faculties. Several such persons, having conceived a complete disgust with life, and yet not daring to commit suicide, have provoked violence at the hands of others. Spermatorrhœa not only abolishes the instinct of self-preservation, but by extinguishing all generous and sympathetic feelings towards others, and all idea of pity or humanity, engenders that entire indifference to the life of others, which is but a step removed from the disposition to homicide. So dependent are these propensities on the spermatorrhœa, that they disappear or recur with it; and several of these patients, on recovering, look back with trembling at the fearful acts they had meditated, and were on the point of executing,—so purely instinctive, and removed beyond the operation of the sphere of reason, were their actions at that time.

Insensibility of the Skin to Painful Impressions.—Prior to the discovery of artificial anæsthesia, little attention was paid to this important somatic symptom. Most writers had alluded to the fact, that lunatics could endure cold and heat, blows, burns, &c., without the expression of pain: but they took no further notice of the phenomenon, and made no application of it to the medico-legal diagnosis of insanity. The discovery has, however, raised it from the oblivion it had fallen into since the days of ancient legal medicine. It will be found that in the trials of the unfortunate beings accused of sorcery, great stress was always laid upon the existence of *anæsthesia,* as a sign of demoniacal possession. The skin was pricked, and if the person exhibited no sign of sensation, he was condemned.

To prevent any illusion in this kind of proof, it must be put into force unknown to the patient, whose interests or dissimulation might otherwise interfere with the exactitude of the results. In M. Michea's own researches, he has, while an assistant distracted the attention of the patient by conversation, tickled the ear with a feather, thrust a needle into the skin of the occipital region and the extremities, and brought lighted amadou against the surface. The anæsthesia was found most complete at the upper and lower extremities, being less distinct in the cervical

region. It is especially observable in affective monomania; and now that attention
has been drawn to it, several practitioners have noted it. M. Beau has observed
it in lypemania in general, and M. Moreau especially in suicidal monomania.

The demonologists and medical jurists of the sixteenth and seventeenth cen-
turies have also mentioned another somatic symptom, which it is right to refer to,
as it may be associated with anæsthesia, and even be a mere consequence of it.
Torre-Blanca and Zacchias declare that pricks in sorcerers yield no blood; and all
exorcists and examiners have regarded this as a strong proof of demoniacal pos-
session, even physiologists offering to explain it. Lordat observes, that of all the
emotions, fear operates most strongly in impeding the circulation; and the terror
of these poor creatures, accused of a crime that would lead them to the stake, may
be imagined. So, too, on the supposition of the existence of cutaneous anæsthesia,
we may believe that the course of the blood would be retarded in those capillaries
to which are distributed nervous filaments that have lost a portion of their animating
power. However this may be, M. Michea has observed, in the course of his re-
searches, in some cases of religious or suicidal lypemania, that not a drop of blood
followed even deep punctures by means of needles, while in other instances the
contrary was the case. This sign is therefore only mentioned incidentally, it not
being constant enough to serve the purposes of legal medicine; but as to the
existence of *spontaneous anæsthesia* in certain cases of disposition to homicidal
or suicidal monomania, there can be no doubt.—*L' Union Méd.*, Nos. 29, 30, 31.

[Although not agreeing with M. Michea, that the somatic signs of monomania
to which he has adverted, will have much weight with juries, we think their con-
sideration is highly important, those of a psychical character having too exclusively
engaged the attention of medical jurists.]

On a Case of Poisoning from Swallowing Chloroform, and on its Administration in Lead Colic. By Dr. ARAN.

THIS occurred in a case of severe lead colic, for which the chloroform was given
internally with advantage, when, on the eighth day of treatment, the patient
by mistake drank a draught from the bottle containing the chloroform. The burning
sensation produced informing him of his error, he drank large quantities of water,
and made ineffectual attempts to vomit. He was found a few minutes afterwards
with his eyes shining, his features animated, singing and talking incoherently, and
unable to recognise those about him. There were various convulsive movements,
and the skin was insensible to pinching, pricking, &c. The pupils acted naturally,
but the power of vision seemed gone. Pulse between seventy and eighty. In
twenty or thirty minutes he fell into a sleep, which became very deep, and was
accompanied by anæsthesia of surface, and complete relaxation of limbs, the
pupils and respiration continuing normal. Leeches were applied behind the ears,
and purgative enemata given, and in a few hours he rose like a drunken man, to
fall into a sound sleep again. Next day, he could remember nothing of what
occurred, and the ill-effects gradually wore off. As near as could be guessed, the
man must have drank between eight and ten drachms of chloroform; and the inno-
cuity of so large a dose can only be explained by the rapid elimination which Snow
and others have proved that chloroform undergoes. M. Aran thinks that, in similar
cases, a preferable treatment would be the administration of strong coffee, and the
application of cold to the head and sinapisms to the feet—means whose efficacy
is known in cases of poisoning by alcohol, opium, ether, &c.

M. Aran speaks in warm terms of the internal use of chloroform in hysteria,
spasmodic colic, lead colic, &c., given in increasing doses from 20 to 150 drops
in the twenty-four hours. Not only has no accident occurred to any of the very
numerous cases in which he has given it, but the physiological effects have at
most in some few cases resembled the transient intoxication induced by champagne.
He has compared its efficacy in *lead colic* with that of the usual remedies, espe-
cially purgatives and alum, in a great number of cases. He does not deny the

efficacy of *purgatives*, and thinks that they should be always employed in conjunction; but when given alone, only very powerful ones are efficacious, and then only temporarily so. He thinks even less favourably of *alum*, as he has found it very slow in operation, and of doubtful efficacy, beyond enabling mild aperients to act where drastics would otherwise have been required. *Chloroform* acts as opium or belladonna, by relieving the spasm, which constitutes so important an element in the disease. It has this advantage, that it may be given in considerable doses, without the incessant watching required by the other two, owing to its rapid elimination. It should not, any more than they, be employed alone, but in conjunction with means for ridding the economy of the lead, as sulphur and vapour baths, &c. The chloroform is to be applied locally, and administered in a mixture, in doses of from fifteen to twenty drops morning and evening, and as much in a glyster, increasing this quantity if required.—*Bull. de Thérap.*, tom. xlii. pp. 296—304.

Case of Poisoning by the Vapour of Hydrocyanic Acid. By M. Regnauld.

M. Regnauld was called to a medical student who had lain for four hours and a half in a state of extreme coma. There was slight cyanosis of the face. The eyelids were closed, and on opening them the pupils were found excessively dilated. The breathing was laboured, the heart's action irregular, pulse hardly to be felt, and the extremities were cold. The coma was only interrupted by occasional sudden convulsive movements. Some contraction of the extremities existed, but no tetanus. Those who had first seen him, suspecting that he had taken some cyanide of potassium, gave him emetics (which did not act), and ordered sinapisms and inspirations of chlorined water. The intensity of the head-symptoms induced M. Regnauld to order venesection and an enema; and guided by the smell of hydrocyanic acid, and materials for preparing it in the apartment, he came to the conclusion (which was confirmed by the subsequent avowal of the young man), that this was an example of poisoning by the vapour of hydrocyanic acid, disengaged from a mixture of the ferrocyanide of potassium and sulphuric acid. An abundant evacuation was procured, sinapisms were applied to the feet, and a stimulating embrocation to the temples—the head and cervical region being surrounded by ice. The symptoms gradually abated, and on the third day the patient had quite recovered.

M. Regnauld takes this occasion to draw attention to the importance of employing general treatment for the relief of alarming symptoms, in cases in which we are sure that the poison is completely absorbed. In this case the chlorine vapour was of little or no use; but a most remarkable change for the better quickly followed the employment of the venesection, enema, and ice.—*Annales d'Hygiène,* tom. xlvii. p. 455.

Case of Poisoning from the External Application of Cocculus Indicus. By Dr. Thompson.

An Irish child, æt. 6, was admitted into the Emigrant Hospital, Ward's Island, on account of porrigo; and being infested with vermin, an infusion of Cocculus Indicus (made by displacement, a pound to three gallons of dilute alcohol, six ounces being employed for three children) was ordered to the scalp, after the hair had been cut close. Half an hour afterwards, the child was seized with tetanic spasm, and the pupils were excessively contracted. As the spasm abated, the pupils dilated to their fullest extent, to become again contracted on its recurrence. By touching the eyelids, the spasm could be induced at pleasure. Counter-irritation, warm baths, and antispasmodics, were resorted to; but the attacks continued to increase in frequency and force, and the child died in a few hours. At the *post-mortem* examination, all parts were found in a healthy state. A sister, æt. 4, for whom the application was also used, manifested the same symptoms; but the persevering use of warm baths, large mustard sinapisms, and assafœtida clysters, subdued them at the end of three hours.—*Philad. Med. Exam.*, vol. viii. p. 227.

POSTSCRIPT.

MR. COULSON having addressed a communication to the Publishers of this Journal, —complaining of the imputation of undue frequency in the announcement of his work in the public prints, which was contained in the review of his treatise ' On Diseases of the Bladder and Prostate Gland,' in the last number (p. 71),—and stating that, from the date of the publication of the Third Edition, nine years previously, he has entirely left the advertising of his work in the hands of his Publisher (whose property it is), the Editor has felt it incumbent upon him to make further inquiry upon the point; and he now readily avows his conviction that, so far as regards this period, the imputation is unfounded, and freely expresses his regret that it appeared in the pages of the Review. He would assure Mr. Coulson and his readers, that no other motive induced the Reviewer to make this allusion, or the Editor to admit it, than the desire, on the part of both, to uphold the dignity of the Profession, by repressing, so far as their power extends, all means of rising to practice which are incompatible with it.

BOOKS RECEIVED FOR REVIEW.

Movements or Exercises, according to Ling's System, for the Development and Strengthening of the Human Body in Childhood and in Youth. By Mathias Roth, M.D. London, 1852. 8vo, p. 41.

The Russian Bath; published with a view to recommend its introduction into England for Hygienic as well as Curative Purposes. By Mathias Roth, M.D. London, 1852. 8vo, pp. 38.

Lectures on Clinical Medicine. By John H. Bennett, M.D., F.R.S.E., &c. No. VII. Edinburgh, 1852. 8vo, pp. 336.

On Rupture of the Perinæum, and its Treatment. Illustrated by Cases. By Isaac Baker Brown, F.R.C.S., &c. London, 1852. 8vo, pp. 30.

The Physician's Holiday; or, A Month in Switzerland in the Summer of 1848. By John Forbes, M.D., F.R.S. Third Edition. London, 1852. Post 8vo, pp. 350.

Disease in Childhood, its common Causes, and Directions for its Practical Management. By Robert Ellis, F.L.S. London, 1852. Fcap. 8vo, pp. 288.

Pathological Anatomy, considered in its relation to Medical Science. By Thomas S. Holland, M.D., M.R.C.S. Cork, 1852. 8vo, pp. 31.

Rheumatism, Gout, and Neuralgia, as affecting the Head and Ear: with Remarks on some forms of Headache in connexion with Deafness. By William Harvey, Surgeon to the Royal Dispensary for the Diseases of the Ear, &c. &c. London, 1852. 8vo, pp. 291.

Military Surgery; or, Experience of Field Practice in India, during the years 1848 and 1849. By J. J. Cole, H.E.I.C.S. London, 1852. 8vo, pp. 223.

Practical Remarks on Palpitation and other Functional Diseases of the Heart. By John Calthrop Williams, M.D., F.R.C.P.E., &c., late Physician to the General Hospital near Nottingham. Second Edition. London, 1852. 8vo, pp. 207.

A Compendious History of Small-pox; with an Account of a Mode of Treatment, Constitutional and Local, &c. By Henry George, Surgeon. London, 1852. 8vo, pp. 134.

Practical Observations on Diseases of the Lungs and Heart. By Archibald Billing, M.D., A.M., F.R.S. London, 1852. 8vo, pp. 138.

On a New Method of Treating certain Diseases of the Cervix Uteri. By Robert Ellis, F.L.S., &c. London, 1852. 12mo, pp. 23.

Medical Jurisprudence. By Alfred S. Taylor, M.D., F.R.S. Fourth Edition. London, 1852. Fcap. 8vo, pp. 826.

A Practical Treatise on Diseases of the Skin. By J. Moore Neligan, M.D., &c. Dublin, 1852. Small 8vo, pp. 439.

The Prescriber's Complete Handbook. By MM. Trousseau and Reveil. Edited, with Notes, by J. Birkbeck Nevins, M.D. London, 1852.

The Practice of Surgery. By James Miller, F.R.S.E., Professor of Surgery in the University of Edinburgh, &c. Second Edition. Edinburgh, 1852. 8vo. With 227 wood-engravings.

Annual Report of the Grant Medical College, Bombay; Sixth Year, Session 1851-2. Bombay, 1852. 8vo, pp. 194.

A Toxicological Chart. By William Stowe, M.R.C.S. Eleventh Edition. 1852.

On a New Way of Treating Gonorrhœa. By John Milton, M.R.C.S. London, 1852. 8vo, pp. 103.

The Medical Report of the Female Side of the Colney Hatch Lunatic Asylum, Middlesex. By James George Davey, M.D. London, 1852. 8vo, pp. 48.

Report of the Commissioners of Health, Ireland, on the Epidemics of 1846 to 1850. (Parliamentary Paper.) Dublin, 1852. 8vo.

Appendix (C) to the Report of the General Board of Health on the Epidemic Cholera of 1848 and 1849. Abstract of Report by James Wynne, M.D., on Epidemic Cholera, as it prevailed in the United States in 1848 and 1849. (Parliamentary Paper.) London, 1852. 8vo.

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